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Fourteen Arguments in Favour of a Formalist Philosophy of Real Mathematics

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Abstract. The formalist philosophy of mathematics (in its purest, most extreme version) is widely regarded as a “discredited position”. This pure and extreme version of formalism is called by some authors “game formalism”, because it is alleged to represent mathematics as a meaningless game with strings of symbols. Nevertheless, I would like to draw attention to some arguments in favour of game formalism as an appropriate philosophy of real mathematics. For the most part, these arguments have not yet been used or were neglected in past discussions.

Keywords: foundations of mathematics, philosophy of real mathematics, formalism, platonism, consistency, inconsistency

1 Introduction

The formalist philosophy of mathematics (in its purest, most extreme version) is widely regarded as a “discredited and hopeless position”. This pure and extreme version of formalism is called by some authors “game formalism”, because it is alleged to represent mathematics as a meaningless game with strings of symbols (for references, see the account by A. Weir (2011)).

Nevertheless, I would like to draw attention to some arguments in favour of game formalism as an appropriate philosophy of real mathematics. For the most part, these arguments have not yet been used or were neglected in past discussions.

2 Carl Johannes Thomae as Founder of Game Formalism

The idea of the game formalist foundations of mathematics can be traced to 1898 when Carl Johannes Thomae stated in his book:

“The formal conception of numbers... does not ask, what are and what shall the numbers be, but it asks, what does one require of numbers in arithmetic. For the formal conception, arithmetic is a game with signs... The formal standpoint relieves us of all metaphysical difficulties, that is the benefit it offers to us." (quoted after M. Epple (2003), p. 301)

For me, this statement represents an advanced version of the formalist philosophy, devoid of Hilbert's *wir-werden-wissen*-optimism, an optimism that failed after Gödel's incompleteness theorems. David Hilbert believed that, despite the paradoxes of set theory, mathematics is, in fact, perfect, and that there must be a *single* formal system of axioms covering the entirety of this perfect mathematics. With such a formal system, it should be possible to prove (as a mathematical theorem) that the system is, indeed, perfect (i.e., that it is both consistent and complete).

However, Kurt Gödel showed in 1930 that this is impossible. While Thomae died in 1921, I do not think, he would be shocked by the incompleteness phenomenon. If we stop asking, "What are and what shall the numbers be?", and ask instead, "What does one require of numbers in arithmetic?", i.e., what are one's arithmetical axioms, then Gödel's incompleteness theorems say simply that there are no perfect axioms. Any system of axioms either is not universal, or is inconsistent, or is incomplete (details below, if needed). Why is our perfect mathematics leading us steadily to imperfect axioms only? Does this not mean that mathematics itself is not perfect?

Argument 1. *All metaphysical difficulties concerning mathematics will be solved if mathematicians will always explicitly formulate their axioms. The benefit of the formal standpoint is that it relieves us of any metaphysical difficulties, as expressed by C. J. Thomae in 1898.*

3 Mathematical Platonism/Realism

Mathematical platonism, as a regular term, has been in use since P. Bernays (1935). Bernays considered mathematical platonism as a *method* that can be – "taking certain precautions" – applied in mathematics. However,

"... Several mathematicians and philosophers interpret the methods of platonism in the sense of conceptual realism, postulating the existence of a world of ideal objects containing all the objects and relations of mathematics. It is this absolute platonism which has been shown untenable by the antinomies, particularly by those surrounding the Russell-Zermelo paradox. ... the need for a restriction is often not noticed."

Because of these obvious difficulties, platonists have been trying, for many years, to restrict their "world of ideal objects", thus arriving at various forms of *realism*. In a sense, sometimes, formalists are doing the same – in order to avoid contradictions, they are trying to restrict axioms.

What are the lessons to be learned from the antinomies surrounding the Russell-Zermelo paradox? It seems, many people think that there are no such lessons, except a single technical one: that the unrestricted comprehension principle of set-building, introduced by Georg Cantor leads to contradictions.

Argument 2. *However, isn't the lesson to be learned from the antinomies much more fundamental? Namely, it seems, that at some moment in its development, any mathematical intuition loses its way.* There is no such thing as "perfect mathematical intuition". *The best method of cleaning up a defective intuition is* axiomatization. *More than 100* *years after the paradoxes, do we have any serious evidence contradicting this argument?*

4 Working Mathematicians "in Between"

The situation of working mathematicians between the above-mentioned extreme philosophical positions has been discussed by several authors. An excellent description was given by Y. N. Moschovakis (1980):

"The main point in favor of the realistic approach to mathematics is the instinctive certainty of almost everybody who has ever tried to solve a mathematical problem that he is thinking about "real objects", whether they are sets, numbers, or whatever; and that these objects have intrinsic properties above and beyond the specific axioms about them on which he is basing his thinking for the moment. Nevertheless, most attempts to turn these strong feelings into a coherent foundation for mathematics invariably lead to vague discussions of "existence of abstract notions" which are quite repugnant to a mathematician.

"Contrast with this the relative ease with which formalism can be explained in a precise, elegant and self-consistent manner and you will have the main reason why most mathematicians claim to be formalists (when pressed) while they spend their working hours behaving as if they were completely unabashed realists." (p. 469)

R. Hersh (1979) expressed the same idea as an aphorism:

"... the typical 'working mathematician' is a Platonist on weekdays and a formalist on Sundays." (p. 32)

Hersh derived his aphorism by quoting from two prominent papers published in 1971 – papers by Jean Dieudonné and Paul Cohen.

In a productive explanation, none of the aspects of real mathematics should be "explained away". So, then, what are the effective functions of platonism (platonistic thinking) and formalism (axiomatization) in real mathematics?

As put by Dieudonné:

"This [realistic] sensation is probably an illusion, but is very convenient. That is Bourbaki's attitude toward foundations." (quoted after R. Hersh (1979))

5 The Effective Role of Platonistic Thinking in Mathematics

A more detailed explanation was proposed by K. J. Devlin (1991):

"... Why go to the additional step of introducing a realm of abstract objects?

"The answer is that it is far, far easier to reason using such entities. I happen to think that our intellectual development does not progress very far beyond our childhood manipulations of marbles, sticks, counters, beans and what-have-you. We reach a stage of maturity when we can reason using *abstract* objects created by the mind, ... the use of abstract objects greatly facilitates this increased logical complexity. But it is still reasoning *about objects*." (p. 67)

Thus, it seems, *for humans, platonistic thinking is the most efficient way of working with mathematical structures*. Mathematicians are accustomed to thinking of mathematical structures as independently existing objects, that (as put by Moschovakis) "have intrinsic properties above and beyond the specific axioms about them". However, is this sensation of "properties above and beyond" only a convenient illusion (as suspected by Bourbaki)? Or, is it (the sensation, called also mathematical intuition) an "authentic" source of information about "independently existing objects"?

6 The Effective Role of Axiomatization/Formalization in Mathematics

In the history of mathematics, from time to time, pieces of such "authentic" intuitive information were formulated explicitly as increasingly complete axioms and definitions. The main reason for this process was the need to correct those defective intuitions that had lost their way. As the result, the classical mathematical structures were axiomatized: geometry, calculus, arithmetic and set theory.

The second reason for axiomatization was (and remains) the possibility of *generalization*: if one is using an explicit set of axioms, then his/her results will be applicable to any structures that satisfy the axioms. In this way the modern abstract mathematical structures (fields, rings, groups, other abstract algebraic structures, abstract spaces, categories etc.) were invented, thus extending the applicability of mathematics.

One might wish to object that the second kind of axioms (for example, the group axioms) are, in fact, *definitions*. On the other hand, one might wish to consider the first kind of axioms (for example, the axioms of set theory) to be *implicit* definitions. Strictly speaking, *any* axioms become definitions when they are *interpreted*, i.e., when they are used to select some subtype of structures among a wider type of structures. On the other hand, definitions and even *premises of a theorem* can be regarded as axioms, if one wishes to explore the limits of applicability of the theorem.

Argument 3. Platonistic thinking is an essential aspect *of mathematician's work: for humans, it is the most efficient way of working with mathematical structures. However,* axiomatization is an essential aspect *of mathematics as well: it allows for the correction of defective intuitions, and extends the applicability of mathematics.*

7 Game Formalism As a Philosophy of Real Mathematics

As a philosophy of real mathematics, game formalism allows mathematicians to *postulate any axioms that make sense*, and explore the consequences that can be derived from these axioms (by the application of some accepted means of reasoning, i.e., of some logic). Making sense (there may be multiple ways to do so) is crucial here, of course. Mathematics has always contained elements of gaming, but this was never a meaningless game.

The consequences obtained from a definite set of axioms are applicable to any structures that satisfy these axioms. Thus, uninterpreted axioms are not meaningless, they are interpretable in multiple ways. As expressed by one of the founders of category theory, Saunders Mac Lane (1986):

"Mathematics aims to understand, to manipulate, to develop, and to apply those aspects of the universe which are formal." (p. 456)

Argument 4. *Uninterpreted axioms are not meaningless, they are* interpretable in multiple ways. *The consequences of uninterpreted axioms are applicable to any structures that satisfy the axioms.*

One cannot imagine working in the modern branches of mathematics for a long time without knowing exactly which axioms one is using. The simplest example: when people are working in group theory, their results will apply to all groups (or to some precisely defined subtypes of groups) only if they deliberately keep themselves within the framework of the group axioms. Or, when publishing in advanced set theory, people must indicate explicitly which large cardinal and/or determinacy axioms they are using. This is mandatory even for the most devoted set theory platonists. And, as mentioned above, people working in the old classical branches of mathematics agree easily (when pressed) that they are working "within ZFC". Those working in category theory and other modern mathematical theories are aware that their work can be formalized in ZFC extended by the axiom "there is a proper class of strongly inaccessible cardinals" (for details, see C. McLarty (2010)).

Argument 5. *In fact, real mathematics is developed within axiomatic frameworks. This is why* uninterpreted *formal systems (formal languages, axioms and logics) can serve as a clean representation of the real mathematics of modern times.*

Stephen W. Hawking (2002): "... we are not angels, who view the universe from the outside. Instead, we and our models are both part of the universe we are describing."

Argument 6. *Any formal system, after its definition is put on paper, becomes part of the physical universe. Therefore, asking about the "unreasonable effectiveness of mathematics in the natural sciences" (E. P. Wigner (1960)) is, in fact, asking about the applicability of a particular fragment of the physical universe to other fragments. This rebuts the "applicability argument" raised by Gottlob Frege against game formalism (for details, see A. Weir (2011)).*

8 The Question of Questions

As mentioned above, from time to time, pieces of "authentic" intuitive information about "independently existing mathematical objects" are formulated explicitly as increasingly complete axioms and definitions. Hence, the *question of questions*: could there be "independently existing mathematical objects" for which the above-mentioned process of axiomatization *cannot be completed*? Put another way, are there some structures "out there" that can be captured by appropriate mathematical intuitions, but cannot be represented explicitly by appropriate systems of axioms?

9 Gödel's First Incompleteness Theorem

The *pure mathematical contents* of Gödel's First Incompleteness Theorem, without any admixture of philosophical assessment, is represented in the following formulation (the modern version as improved by J. B. Rosser):

Theorem 1. *Assume T is a formal system of axioms (formal theory) in which the basic theorems about natural numbers $(0, 1, 2, ...)$ can be proved. Then there are two algorithms. The first one builds, depending on the axioms of T, a formula G_T that expresses some definite statement about natural numbers. The second allows for the conversion:*
a) of any T-proof of G_T into a T-proof of $\neg G_T$ (the negation of G_T); and
b) of any T-proof of $\neg G_T$ into a T-proof of G_T.

From this point on, one may start drawing philosophical consequences.

The most popular first step is the (seemingly harmless) re-formulation of the theorem given below. If T is an inconsistent system, then T proves anything, G_T and $\neg G_T$ included. However, if T is a consistent system, then T can prove neither G_T, nor $\neg G_T$. Hence, the re-formulation:

Theorem 2. *If T is a consistent formal theory in which the basic theorems about natural numbers can be proved, then there is a definite statement about natural numbers that T can neither prove, nor disprove.*

In short, if T is a consistent formal theory proving the basic theorems about natural numbers, then T is *incomplete*, hence, the term "incompleteness theorem". This is still correct, but the next step leads to confusion.

Is our theory T consistent? An easy theorem follows.

Theorem 3. *If there is at least one consistent formal theory proving the basic theorems about natural numbers, then there is no algorithm that makes it possible to decide, from the axioms of T, whether or not T is consistent.*

Hence, one cannot, simply starring at the axioms, decide, are they consistent, or not.

10 Is Arithmetic Consistent?

And, if so, which theory T do we have in mind? First-order arithmetic (also called PA)? Almost all people believe, following their intuition of the natural number sequence $0, 1, 2, ...$, that the axioms of PA are true for these numbers, and hence, "obviously", PA is a consistent formal theory. These people will not agree with the following argument.

Argument 7. *The argument about the "obvious" consistency of first-order arithmetic returns us to Argument 2 about the reliability of mathematical intuitions. Why should we regard our intuition about the natural number sequence as absolutely reliable? As we know, until 1895, Cantor's intuition of infinite sets was widely regarded as "obviously true", but then the "antinomies surrounding the Russell-Zermelo paradox" appeared. The arithmetical intuition is likely more reliable than Cantor's intuition of infinite sets, but should it be regarded as* absolutely *reliable?*

As a consequence of this argument, a philosophically neutral formulation of Gödel's First Incompleteness Theorem should be *symmetrical*:

Theorem 4. *If T is a formal theory in which the basic theorems about natural numbers can be proved, then T is either inconsistent, or incomplete.*

Working in T (for example, in PA, ZFC, or any more powerful theory), one will arrive inevitably either at contradictions, or at unsolvable problems belonging to the scope of the competence of T. The outcome of the process cannot be predicted in advance.

11 Gödel's Second Incompleteness Theorem

All the traditional versions of the formula G_T used to prove the First Incompleteness Theorem possess a common property: if T is a consistent theory, then G_T is a *true statement about natural numbers*. This property has caused much of confusion.

For most people, the axioms of the first-order arithmetic PA are "obviously true", i.e., PA is a consistent theory, and thus G_T is an "obviously true" statement about natural numbers, that cannot be proved in PA. It seems, human mathematicians are able to easily prove properties of natural numbers that cannot be proved in PA.

In fact, one does not need specific "human powers" to derive the truth of G_T from the *assumption* of the consistency of T. Let us encode the formal language of PA by using natural numbers (a so-called *arithmetization*). This allows us to represent the proposition "T is consistent" by some arithmetical formula $Con(T)$. If $Con(T)$ is built in the most direct way, then one can prove in T itself that, indeed, $Con(T)$ expresses the consistency of T (for technical details, if needed, see K. Podnieks (2015)). It appears, that the formula $Con(T) \to G_T$ can then be formally proved in T. Thus, if one *assumes* that T is consistent, i.e., if one assumes $Con(T)$, then proving the formula G_T does not require specific "human powers"; one can use the axioms of T instead!

The consequence is Gödel's Second Incompleteness Theorem. Let us start with the pure mathematical version:

Theorem 5. *Assume T is a formal theory in which the basic theorems about natural numbers can be proved, and assume T proves that the formula $Con(T)$ expresses the consistency of T. Then there is an algorithm converting any T-proof of $Con(T)$ into a T-proof of a contradiction.*

To simplify: if T proves its own consistency, then T is, in fact, inconsistent. Or, if T is consistent, then T cannot prove its own consistency. *To prove that T is consistent, one must apply some means of reasoning (axioms, logic) that are not formalizable in T.*

For example, using the set theory ZFC as a meta-theory, one can easily prove that PA is consistent – by building the so-called standard model of PA. But should one rely on a consistency proof achieved in a theory that is far less reliable than PA?

Note that if, in some meta-theory T_1, we have proved the consistency of T, this does not immediately mean that T_1 is stronger than T. It means only that T_1 contains means of reasoning that are not formalizable in T. As an example, refer to Gentzen's consistency proof, discussed below. Only if T_1 is, additionally, an *extension* of T (i.e., T_1 proves all theorems of T), then can it be qualified as being stronger than T. For example, ZFC (if consistent) is, indeed, stronger than PA.

Thus, normally, one should conclude from Gödel's Second Incompleteness Theorem that

Argument 8. *Absolute consistency proofs are impossible. To prove the consistency of some theory T, one must apply means of reasoning that cannot be formalized in T. This returns us to Argument 7.*

Argument 9. *Unless proven by using less reliable means of reasoning, the consistency of first-order arithmetic should be qualified* only as a hypothesis. *Does anyone need more than that?*

12 Reasoning About Theories Needs a Meta-Theory

The idea from which Hilbert launched his formalist program was as follows: if one formalizes some theory T, then that theory becomes a precisely defined mathematical object about which one can prove theorems. In particular, one can try to prove a theorem asserting the consistency and completeness of T. In this way, one could hope (could have hoped until 1930) to obtain an *absolute* consistency proof of T. (Prior to formalization, only relative consistency proofs were possible, by using translations of T into other theories.)

However, Henri Poincaré objected to the idea of absolute consistency proofs from the very beginning. As he noted in his book "Science and Method" published in 1908, for a consistency proof,

"It is necessary... to take all the consequences of our axioms and see whether they contain any contradiction. If the number of these consequences were finite, this would be easy; but their number is infinite – they are the whole of mathematics, or at least the whole of arithmetic." (H. Poincaré (1908), p. 166)

Hence,

"In order to demonstrate that a system of postulates does not involve contradiction, it is necessary to apply the principle of complete induction." (p. 182)

The axioms of first-order arithmetic (PA) include the principle of complete induction as the following schema: $F(0) \wedge \forall n(F(n) \to F(n+1)) \to \forall n F(n)$, where F is any formula in the language of PA.

Thus, according to Poincaré, Hilbert's absolute consistency proof, if successful, would involve *petitio principii*: one would have proved the consistency of the induction principle by using the induction principle.

Hilbert did not receive Poincaré's argument as a serious one. As late as in 1927, he still declared:

"Poincaré arrives at his mistaken conviction by not distinguishing between these two methods of induction, which are of entirely different kinds." (quoted after J. van Heijenoort (1977), p. 473)

Hilbert believed that one must distinguish between two kinds of induction – the "contentual induction" used to prove theorems about formal theories, and the "formal induction" defined by the formal axioms of arithmetic. Despite Poincaré's warning, Hilbert cultivated his illusion of two inductions until Gödel's incompleteness theorems. Poincaré died in 1912, but surely, he would be pleased to see the first step of Gödel's proof in 1931: by means of arithmetization the "contentual induction" was embedded into the formal arithmetic!

One can think "contentually", indeed, but only about particular actually-built formulas and actually-built formal proofs – as physical, "concrete objects" (written on paper, for example). However, the *set of all formulas and proofs* of some formal theory is infinite, hence, it cannot be physical, cannot be "concrete". It can be considered only theoretically: to reason about infinite objects, one must use some *definite theory*. To reason about a formal theory as a whole (for example, about its consistency or completeness), one must use some definite *meta-theory* (including, as noted by Poincaré, the induction principle). Currently, the usual choice is between PA (for pure syntactical analysis) and ZFC or another set theory (for model-theoretical analysis).

As a consequence, one can consider *models of meta-theories*, for example models of PA, used as a meta-theory of itself. It is a theorem of PA, that for each (meta-theoretical) natural number n there is, in the language of PA, a literal of the form $ss...s0$, containing exactly n occurrences of the successor function symbol s. Hence, in a *non-standard model* of the meta-theory PA, *non-standard literals* (and, correspondingly, non-standard formulas and non-standard proofs as well) would correspond to non-standard natural numbers. This inevitable phenomenon was first considered seriously by A. Robinson (1963):

"By introducing non-standard arithmetic already at the stage of the construction of the sentences of the object language, we emphasize the fact that the notion of arithmetic may be relative even at the metamathematical level." (p. 84)

However, the phenomenon seems to have no serious philosophical consequences, as subsequent work shows (see an overview by A. Cantini (2009)).

Argument 10. *The possibility of theory-independent ("contentual") reasoning about formal theories is an illusion. The reasoning about a formal theory as a whole is inevitably* theoretical*. One must choose a definite meta-theory: PA, ZFC, or some other.*

13 Gentzen's Consistency Proof

In 1936, Gerhard Gentzen published a combinatorial consistency proof of PA in which, instead of a trivial set-theoretical model-building, non-trivial proof transformations were used. That was a brilliant achievement, for in subsequent work, Gentzen determined, in a sense, the *minimal meta-theory* allowing to prove the consistency of PA.

To carry out his proof, Gentzen used an induction principle (the so-called ϵ_0-induction, ϵ_0 is a specific ordinal number) that is stronger than the induction, formalized in PA. Induction principles up to any particular ordinal numbers α less than ϵ_0 can be proved as theorems of PA (there is an algorithm, generating for each $\alpha < \epsilon_0$, a PA-proof of the α-induction principle). However, the induction up to ϵ_0 itself cannot be proved in PA (if PA is consistent) because of Gentzen's consistency proof and Gödel's Second Incompleteness Theorem.

Some people regard Gentzen's proof as a very serious argument in favour of the consistency of PA. Firstly, because it is qualified as a "genuine mathematical proof": instead of set-theoretical model-building, it is based on proof transformations. And, secondly, because it can be formalized in a meta-theory that is not stronger than PA.

Gentzen's proof can be formalized in so-called primitive recursive arithmetic (PRA) with the ϵ_0-induction principle (for quantifier-free formulas only) added. Let us call this meta-theory GZ. Since PA proves the consistency of PRA, PRA is much weaker than PA (of course, only if PA is consistent). In fact, Gentzen's proof needs only a small part of GZ. As any actually built proof, it uses only a finite number of axioms, in particular – it uses the ϵ_0-induction principle only for a *finite number* of quantifier-free formulas. However, even this small portion of the ϵ_0-induction cannot be formalized in PA, as predicted by the Second Incompleteness Theorem. Thus, while GZ is not stronger than PA, it goes beyond PA nevertheless.

Argument 11. *Gentzen's proof does not overcome the barrier set by Gödel's Second Incompleteness Theorem. It is a brilliant mathematical achievement with no philosophical consequences.*

14 Is Gödel's Theorem "practical"?

The attitude of working mathematicians toward Gödel's First Incompleteness Theorem is mainly one of indifference. For them, Gödel's construction of the formula G_T is too artificial and too far from real mathematical practice, it seems simply an exploitation of the possibility of diagonalization. Looking at a formal theory from the side (in the meta-theory), one can "diagonalize", but diagonalization is not "normal mathematics".

Despite skeptics, however, Gödel's First Incompleteness Theorem *predicts a ubiquitous phenomenon* in mathematics: a sufficiently universal mathematical theory cannot be perfect; while developing any such theory mathematicians will inevitably run into either contradictions, or unsolvable problems. Is such a general prediction "practical"?

In 1963, some 30 years after Gödel's proof, Paul Cohen proved that if the set theory ZFC is consistent, then this theory is not able to solve Cantor's Continuum Problem. Cohen's proof method (the so-called "forcing") is a sophisticated kind of diagonalization, but this does not affect the significance of the result. If one prefers calling Gödel's First Incompleteness Theorem "very theoretical", then Cohen's result must be acknowledged as its "empirical confirmation". Since 1963 many classical problems of set theory were proved to be unsolvable in ZFC. This is why set theorists are searching for (and finding) new axioms to solve these problems. So, we can speak about the "massive empirical confirmation" of the theoretical prediction of Gödel's Theorem. Can we exclude (an easy prediction!) that some of the long-standing problems of number theory (for example, the twin prime conjecture, despite the recent successes) will also be proved unsolvable (in PA, ZFC, etc.)?

Argument 12. *Gödel's First Incompleteness Theorem represents a very general theoretical prediction, one that has been confirmed empirically many times since 1963.*

15 Large Cardinal Axioms and Determinacy

For details (if needed) and references see the recent accounts by P. Koellner (2013, 2014).

In his informal and inconsistent set theory, Cantor used for set building only his famous comprehension principle (in modern terms – comprehension axioms), and implicitly, the "free choice principle" (the Axiom of Choice, which is not a comprehension axiom). Ernst Zermelo and followers corrected Cantor's inconsistent intuition by formulating the axioms of set theory ZFC. In this theory, only a restricted collection of comprehension axioms and the Axiom of Choice are adopted as set-building principles.

Already in 1908, when Zermelo published the first version of the axioms of set theory, another process was simultaneously beginning – an exploration of non-comprehension principles of set-building other than the Axiom of Choice. In 1908, Felix Hausdorff, and in 1911, Paul Mahlo introduced the first kinds of so-called *large cardinals*, now called weakly inaccessible cardinals and Mahlo cardinals. Since then, on one hand, Zermelo's first system was developed into ZFC, but on the other hand, we have now an impressive "tower" consisting of very many kinds of large cardinals. Building on the work of Gödel, we know that the existence of large cardinals cannot be proved in ZFC (if ZFC is consistent), and that (for the most part) the existence of the "next kind" of large cardinals cannot be derived from the existence of the previous ("smaller") ones. Hence, to obtain such cardinals in set theory, new axioms are necessary – *large cardinal axioms*. The top one of these axioms postulates the existence of so-called Reinhardt cardinals. This axiom was proved to be inconsistent with ZFC. Thus, ZFC proves that Reinhardt cardinals do not exist. However, all the remaining kinds of large cardinal axioms are still believed to be consistent with ZFC.

Would Cantor himself accept the large cardinal axioms? Cantor's way of running into his own paradox (the so-called Cantor's Paradox) seems to demonstrate that he would accept at least the lower end of the large cardinal tower. But does this mean that large cardinal axioms did not add new features to Cantor's notion of sets; that these features were "already there" in 1873?

In 1962 a kind of controversy could have arisen in set theory – Jan Mycielski and Hugo Steinhaus proposed the so-called *Axiom of Determinacy* (AD). From AD, one can derive surprising and beautiful consequences, for example, that every set of real numbers is Lebesgue measurable. Hence, AD contradicts the Axiom of Choice. AD solves the Continuum Problem: in ZF+AD, Cantor's Continuum Hypothesis is proved in its initial form: every infinite set of real numbers is either countable, or equivalent to the entire continuum. It seems that some could have questioned in the 1960s: which of the two alternative set theories would be better to explore – ZFC, or ZF+AD.

Since then, though, after years of surprising developments, it appears that AD and ZFC (extended by large cardinal axioms) are deeply inter-connected. On one hand, by assuming a powerful enough large cardinal axiom, that "there is an infinite set of Woodin cardinals with a measurable cardinal above it", one can prove in ZFC that AD holds in the so-called extended constructible universe $L[R]$. This restricted version of AD is usually denoted by $AD^{L[R]}$. On the other hand, by assuming $AD^{L[R]}$ in ZFC, one can obtain models containing very large cardinals – Woodin cardinals.

Thus, the possible controversy between ZFC and ZF+AD was solved in favour of ZFC, and exploring of the consequences of large cardinal axioms in ZFC remains the mainstream of set theory.

However, did $AD^{L[R]}$ already "hold" in 1873, or was that decision made much later?

The axiom of *Projective Determinacy* asserts that AD holds for the so-called projective sets, and is thus somewhat weaker than $AD^{L[R]}$. W. Hugh Woodin, one of the mathematicians responsible for the above-mentioned surprising developments, commented on it as follows:

"Should the axiom of *Projective Determinacy* be accepted as true? ... Accepting *Projective Determinacy* as true does not deny the study of models in which it is false. ... I believe the axiom of *Projective Determinacy* is as true as the axioms of Number Theory. So I suppose I advocate a position that might best be described as *Conditional Platonism*." (W. H. Woodin (2004), p. 32)

If Conditional Platonism is defined *in terms of axioms* that are believed as true, then even some of the formalists might prefer "to work with Woodin", i.e., within a specific fascinating axiomatic framework.

By agreeing "to work with Woodin", one could become involved in the development of a surprising solution to the Continuum Problem. The solution is formulated in terms of "good axioms", although the definition of "goodness" is technically complicated. P. Koellner (2013) notes:

"To summarize: Assuming the Strong Ω Conjecture, there is a "good" theory of $H(\omega_2)$ and all such theories imply that CH fails. Moreover, (again, assuming the Strong Ω Conjecture) there is a maximal such theory and in that theory $2^{\aleph_0} = \aleph_2$." (at the end of Section 3.3)

Argument 13. *Even the most advanced results of modern set theory (such as Woodin's argument in favour of $2^{\aleph_0} = \aleph_2$) are formulated in terms of axioms. Mathematicians might not agree about "what should be believed as true", but they are in agreement about "what follows from what". This returns us to Argument 1: the formal standpoint relieves us of any metaphysical difficulties.*

16 Is Arithmetic Inconsistent?

Poincaré's warning, taken literally, was about *petitio principii*, contained in Hilbert's idea of proving the consistency of arithmetic by using the "contentual" induction principle.

This is only a part of the creative potential contained in Poincaré's warning, however. If the abstract notion of formal syntax is based on the same principle of complete induction that is formalized in the formal theories of natural numbers, does this not mean, in fact, that any *formal theory of natural numbers must be inconsistent*? Poincaré himself did not draw this conclusion. It seems, as a mathematician, he was optimistic about the qualities of the intuitive notion of natural numbers.

Those working in modern set theory are even more optimistic, believing that they are exploring an absolutely definite, unique, and perfect "world of sets" that satisfies the axioms of ZFC. This attitude seems to be justified by the developments based on the above-mentioned large cardinal axioms.

However, in an abstract published by Nikolai Belyakin (2005) the following result is announced:

"From this fact follows, in particular, that the existence of strongly inaccessible cardinals is refutable in ZF."[1]

Does this mean that contradictions appear not only at the top level, but already at the second level of the large cardinal tower – the level that is necessary for a natural development of category theory?

A detailed proof of the result is still not available. The idea of the proof, as represented in the abstract, seems to "follow Poincaré": it is based on a new kind of embedding of the meta-theoretical induction into the formal set theory.

Could this development lead to the discovery of contradictions in PA? Will the future final version of incompleteness theorems state that *any formal theory proving the basic properties of natural numbers leads to contradictions*?

At least three more prominent mathematicians considered the inconsistency of PA as a real possibility: Edward Nelson (1986), Jacob T. Schwartz (2003) and Vladimir Voevodsky (2010).

Imagine for a moment that someone will succeed and will demonstrate how to derive a contradiction from the axioms of PA. Platonists-realists believe that these axioms are true, but incomplete descriptions of the "real" infinite sequence of natural numbers. They also believe that classical first-order logic (FOL) is "sound" in the sense that it is deriving only true consequences from true axioms. Now, though, imagine that the true axioms of PA and the sound FOL together allow for derivation of contradictions. What could have caused such a situation?

The first possible answer: defective logic; i.e., the axioms of PA are true, but the means of reasoning represented in FOL are defective. In this case, we could analyze, how the contradiction was derived and try building a better logic.

The second possible answer: the axioms of PA are defective – they do not represent a "true" description of the "real" natural number sequence. But then – which one of the axioms is false? To a platonist, all of them seem to be true. Perhaps, however, the principle of complete induction $F(0) \land \forall n(F(n) \to F(n+1)) \to \forall n F(n)$ is too strong. *Indeed, which instances of it were used, to derive the contradiction?* Similarly, the unrestricted comprehension principle of Cantor's set theory appeared as too strong – some of its instances imply contradictions. Thus, in this case, most probably, we would be forced to restrict the induction principle.

The third possible answer:

Argument 14. *If the axioms of PA are inconsistent, then the very idea of the "real" infinite sequence of natural numbers is defective.*

17 Conclusion

The above arguments in favour of (game) formalism as a philosophy of real mathematics lead to the following general conclusion.

[1] The English translation of the abstract contains a typo: one should remove [in] from the statement "It is not hard to check that T is [in]consistent wrt ZF+(existence of strongly inaccessible cardinals)".

Deliberately, or not, most mathematicians spend most of their time working within a restricted axiomatic framework. In this way mathematicians have learned to draw a maximum of consequences from a given set of premises. This ability of mathematicians is very likely another (see Argument 6) reason for the "unreasonable effectiveness of mathematics in the natural sciences".

Working for years in a restricted axiomatic framework; running, from time to time, into contradictions or unsolvable problems (as predicted by Gödel's Theorem); changing the axioms, or inventing new ones – isn't such a process more fascinating than exploring of seemingly independent mathematical objects "out there"?

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