Platonistic Formalism
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PLATONISTIC FORMALISM

ABSTRACT. The present paper discusses a proposal which says, roughly and with several qualifications, that the collection of mathematical truths is identical with the set of theorems of ZFC. It is argued that this proposal is not as easily dismissed as outright false or philosophically incoherent as one might think. Some morals of this are drawn for the concept of mathematical knowledge.

1. INTRODUCTION

In the introduction to his book on set theory, Saharon Shelah writes:

> If we interpret "true" by "is provable in ZFC" (the usual axioms of set theory), as I do, then a large part of set theory which is done today does not deal directly with true theorems – it deals, rather, with a huge machinery for building counterexamples (forcing possible universes) or with "thin" universes (inner models). Very often the answer to the question "can this happen?" is "it depends". (1994, xi)

These are the sort of statements one would expect being made by someone who holds a formalist or combinatorial view about mathematical truth. But Shelah is well known to be, like many set theorists, a straightforward platonist about sets. So this passage raises a philosophical question: Is there any coherent way, compatible with platonism – or, better still, with many positions in the philosophy of mathematics – of defending the position that the class of set-theoretic truths coincides with the class of theorems of ZFC?

It will be argued that, contrary to first appearances and with some qualifications, the thesis that the class of mathematical truths coincides with the class of theorems of ZFC is philosophically defensible. The defense of this thesis that will be given here is based on a distinction between mathematical and philosophical proofs of mathematical propositions. This defense will not presuppose taking a specific stance on deeper ontological and epistemological questions about mathematics, such as Benacerraf's problems. An attempt will be made to show that the thesis and its defense are in line with a relatively mild form of mathematical naturalism.

The structure of this paper is as follows. First, an explication is given of the content of the thesis that is put up for scrutiny (Section 2). Then an

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objection that immediately comes to mind is formulated (Section 3). This objection says, roughly, that if ZFC contains only mathematical truths, then statements such as the Gödel sentence for ZFC ought likewise to count as mathematical truths. It will be shown that the objection can be avoided if an important conceptual distinction is kept in mind, viz., the distinction between philosophical and mathematical proofs of mathematical propositions (Section 4). Specifically, it is argued that we have today no convincing *mathematical* reasons for taking sentences such as the Gödel sentence for ZFC to be mathematical truths. It is pointed out that there is a deep connection between this claim and the position that Daniël Isaacson has developed on arithmetical truth. In Section 5, it is shown that positions which entail that much less, or that much more than what is provable in ZFC is mathematically true are not compatible with mathematical naturalism, while the thesis that mathematical truth coincides with ZFC is in line with naturalism. Analogous to the distinction between truth of a mathematical sentence and mathematical truth, a distinction is then made between knowledge of a mathematical proposition and mathematical knowledge (Section 6). The paper ends with some closing remarks (Section 7).

2. THE THESIS

This paper addresses the question: *which sentences are mathematical truths?* In particular, it discusses one proposal for giving a non-trivial characterization of the collection of mathematical truths. There are some qualifications which need to be made at the outset in order to make the question that is addressed more precise.

First, what will be explored is one possible answer to the question what *our best guess today* (2000) would be for answering this question. The possibility that some of our future best guesses will be different from today's best guess is left open. In fact, in view of the history of mathematics it seems *likely* that our best guess now differs from our best guess in the year 2100. Nevertheless, our question asks for more than an answer to the question: which sentences do we *now know*, explicitly or implicitly, to be mathematical truths? (even though our answers to these questions may coincide). In addition, it has to be at least possible, as far as we know today, that this collection of sentences constitutes *from a timeless perspective* the collection of mathematical truths. It is admissible to politely decline to answer this question, on the grounds that we can make no 'informative good guess' that meets this additional requirement. But it would then have to be explained why this cannot be done.

Second, the *we* in 'our best guess' is *the mathematical community*. This presupposes a mild form of *naturalism* in the philosophy of mathematics, which will be taken for granted in this paper.[1] The form of naturalism that is presupposed here says that the best reasons we have for accepting or rejecting mathematical principles are internal to mathematical practice. Decisions about the acceptance and rejection of mathematical principles are and should be made by the mathematical community on the basis of mathematical as opposed to philosophical reasons. It is the task of the philosopher of mathematics (or of the mathematician, when in a foundational mood) to identify and analyze these basic principles and mathematical reasons for accepting or rejecting them. But every philosophical attempt to argue that a substantial body of mathematics that is generally accepted by the mathematical community is nevertheless fundamentally flawed has to be regarded with much suspicion. And the same holds for philosophical attempts to show that certain principles, not generally accepted by the contemporary mathematical community, are in fact basic mathematical truths and are henceforth to be taken as axioms.

Third, our attention will be confined to truths of *pure* mathematics. If the boundary between pure and applied mathematics is vague, then a corresponding vagueness in the characterization of the collection of mathematical truths has to be allowed for.

Finally, the question taken up here does not ask for a semantics for mathematical language, nor for an ontological account of what *makes* mathematical statements true, nor for a philosophical account of how we can have knowledge of basic mathematical truths.[2] It asks merely for an informative description of the class of mathematical sentences that *are* mathematical truths.

The thesis that is put up for scrutiny here is that insofar as we can tell today (in 2000), the collection of mathematical truths coincides with the collection of theorems of ZFC, formalized Zermelo–Fraenkel set theory with the Axiom of Choice. This thesis needs some fairly obvious qualifications, which are stated below. But substantially this is the proposal that will be explored. Henceforth this thesis, with the appropriate qualifications, will be referred to as the Thesis (with capital T).

Here are the necessary qualifications. First, mathematical truths are sentences of the *informal* language of mathematics (of which formalized ZFC is a proper part). So if we want to be a little more precise, then we would have to say that, as far as we can tell today, ZFC, when interpreted in the normal way,[3] provides a good *model* of the set of mathematical truths. Second, the Thesis is not intended to imply that numbers are *reducible* to sets. It may well be that natural numbers, real numbers, etc.

are not sets.[4] But there are well-known ways of representing the natural numbers, real numbers, etc. and number-theoretical facts about them in ZFC. To say that a number-theoretic statement is provable in ZFC would then be to speak loosely. It would be short for saying that there is a *routine translation* from number-theoretic to set-theoretic sentences under which the number-theoretic statement is provable in ZFC.[5] This then is a second sense, closely related to the first sense, in which ZFC should be taken as a *model* for representing mathematical truths. Third, there is the question whether first- or second-order ZFC (ZFC^1 or ZFC^2), or perhaps third- or fourth-order, etc. is intended. This question can be left open here. What will be said in the sequel should by and large hold (or fail to hold) on each of these interpretations.[6]

3. THE OBVIOUS OBJECTION

There is an objection that immediately comes to mind. If all theorems of ZFC are mathematical truths, then surely the Gödel sentence for ZFC (G_{ZFC}) and the sentence expressing the consistency of ZFC (Con_{ZFC}) are mathematical truths? Gödel showed us that while they are unprovable in ZFC, they are nevertheless both true. Therefore ZFC does not exhaust the collection of mathematical truths. And since something like this can be said about *any* even remotely plausible proposal for identifying mathematical truth with provability in an axiomatic system, should we not conclude that it is impossible to give an informative characterization of the collection of mathematical truths?

One could try to resist this line of reasoning in the following way. G_{ZFC} and Con_{ZFC} are not mathematical but *meta*mathematical sentences, metamathematics being the mathematical investigation of formal systems.[7] Since formal systems do not belong to the domain of pure mathematics, metamathematics is a branch of applied, and not of pure mathematics. In sum, sentences such as G_{ZFC} and Con_{ZFC} are not even *candidates* for being counterexamples to the Thesis.

But this does not help much. Aside from the disputable assertion that formal systems do not belong to the domain of pure mathematics, G_{ZFC} and Con_{ZFC} are in the final analysis combinatorial statements. Given a suitable coding scheme, metamathematical assertions can be "expressed" by sentences of the language of ZFC (or even by sentences of the language of elementary arithmetic). Such sentences are clearly of a purely mathematical nature. And since these set-theoretic equivalents of G_{ZFC} and Con_{ZFC} are evidently true but unprovable in ZFC, aren't we compelled to admit that *they* are mathematical truths?

I think many mathematicians' gut feeling is that they are *not* mathematical truths[8] – although they are very likely to then go on losing the resulting dispute with a philosophically inclined logician. But there is a way in which a defender of the Thesis can hold her ground. The defense which will be constructed here depends on a distinction between mathematical and philosophical proofs of mathematical propositions. To this distinction we now turn.

4. MATHEMATICAL AND PHILOSOPHICAL DEMONSTRATIONS

4.1. *The Soundness and Consistency of ZFC*

Sure enough G_{ZFC} and Con_{ZFC} are true: Gödel *proved* them to be true (although these proofs are not formalizable in ZFC). A Gödel-style proof of the consistency of ZFC, for example, goes roughly as follows:[9]

> The axioms of ZFC are true of the set-theoretic universe; the rules of inference of ZFC are truth-preserving. Hence every theorem of ZFC is true of the set-theoretic universe. So no sentence of the form $A \wedge \neg A$ is a theorem of ZFC.

Formally, consider the language of ZFC plus a new one-place truth predicate T. From the axioms of ZFC formulated in this extended language (so that T is allowed to occur in instances of the comprehension scheme) plus the axioms of Tarski's inductive theory of truth, G_{ZFC} and Con_{ZFC} can be derived.[10]

Now quite a few mathematicians do *not* accept the above proof as a convincing proof of Con_{ZFC}, e.g., because they feel that their intuition of the set-theoretic universe is not strong enough to produce the conviction that all axioms of ZFC are true in this structure. These mathematicians regard the question of the consistency of ZFC as genuinely open.

Other mathematicians do find the above consistency proof convincing. But even if it is a sound proof, the defender of the Thesis will insist that *to be a mathematical truth it is not sufficient to belong to the language of mathematics and to be true*. Gödelian proofs of G_{ZFC} and Con_{ZFC} are certainly *partly* mathematical in nature. The proof cited above, for example, involves an instance of the principle of mathematical induction, which is a mathematical principle if there ever was one. It is just that such Gödelian proofs are not *purely* mathematical proofs. For they essentially contain the notion of *truth*, which is itself not a mathematical but a philosophical notion. This is not to deny that mathematics can be applied to produce interesting theories of truth.[11] It is just that mathematical theories of truth do, on this view, belong not to pure mathematics but at best to *applied* mathematics, or to the more mathematical part of philosophy.

In the absence of a purely mathematical proof of G_{ZFC} and of Con_{ZFC}, we should not count them as *mathematical* truths. Comes a time when we accept mathematical axioms from which G_{ZFC} and Con_{ZFC} follow, then we will take them to be mathematical truths. Until then, not. So, for example, the consistency of Z,[12] which *is* provable in ZFC, is a mathematical truth.[13] And of course many of the statements of the form $Con_X \rightarrow Con_{X+Y}$ are mathematical truths, since many such statements are provable in weak fragments of ZFC. But the consistency of $V = L$, the independence of the continuum hypothesis, the independence of the existence of strongly inaccessible cardinals, etc. are not, as far as we can say today, mathematical truths. Hence the feeling of many mathematicians that by producing almost nothing but independence proofs set theory has in recent decades alienated itself from mainstream mathematics.[14]

It could be retorted that since we are convinced of the truth of G_{ZFC} and of Con_{ZFC}, we might as well take them as *new axioms*. But the point is that we do not even *consider* doing this. It is not sufficient for a sentence of the language of set theory to be recognized to be true for it to be eligible to become a new axiom. Whatever the additional necessary requirements are for a principle to be considered an axiom candidate for set theory, G_{ZFC} and Con_{ZFC} do not meet all of them.[15]

Given that G_{ZFC} and Con_{ZFC} are not mathematical truths, and given the partly philosophical arguments that purport to establish their truth, should these sentences be seen as *meta*mathematical truths? What the correct answer to this question is depends on whether truth is a metamathematical notion. Metamathematics is usually regarded as a branch of pure mathematics.[16] If that is so, then truth is *not* a metamathematical notion. Hence, as long as there exists no mathematical proof of G_{ZFC} or of Con_{ZFC}, these sentences are not mathematical truths. If truth *is* taken as a metamathematical notion, then metamathematics is not, strictly speaking, a branch of pure mathematics. In that case, if the above consistency proof and the related argument for the truth of G_{ZFC} are convincing, then these sentences should be taken to be metamathematical truths.

4.2. *Mathematical Truth as a Philosophical Notion*

It may be useful at this point to briefly contrast the defense of the Thesis that was mounted above with Hartry Field's position on this matter. He too claims that we do not have a mathematical proof of the Gödel sentence and the consistency statement for our most comprehensive mathematical theory M.[17] But his arguments for this claim are very different from those that were proposed in the previous section. According to Field, the language of M contains a primitive truth predicate and M contains axioms governing this truth predicate.[18] So the *language* of our most comprehensive mathematical theory is sufficiently strong to express a Gödel-style proof of the consistency of M. But the *premises* of this argument, expressing the truth of the axioms of M, will not be provable in M – otherwise M would be, by Tarski's theorem, inconsistent (Field 1998, 112–114).

So according to Field, 'true', as applied to mathematical sentences, is a perfectly good *mathematical* notion. I disagree. Even truth as applied to mathematical sentences is a philosophical notion. Again, this is not to deny that the notion of mathematical truth can be legitimately used, for instance, for conclusively justifying certain progressions of formalized mathematical theories.[19] But it does imply that such justifications do not constitute a *mathematical* proof of the soundness and consistency of the theories in such a progression.

Gödel at one point also seems to imply that mathematical truth is a notion of pure mathematics. When referring to an elliptical form of the above argument for demonstrating the consistency of a mathematical theory, he says that one thereby obtains a "*mathematical* insight not derivable from [the] axioms" (Gödel 1995[1952], 309) [my emphasis]. Again I disagree: what is thereby obtained is at least in part a philosophical insight.

However, in an earlier paper Gödel seems to be acutely aware of the distinction between purely mathematical and partly philosophical principles that can be used for proving mathematical propositions. In his *Remarks to the Princeton Bicentennial Conference* he formulates the following conjecture:

> ... the following could be true: Any proof for a set-theoretic theorem in the next higher system above set theory (i.e., any proof involving the concept of truth [...]) is replaceable by a proof from [...] an axiom of infinity. (Gödel 1990 [1946], 151).

Here "proofs involving the concept of truth" are (in my terminology) partly philosophical proofs, and "axioms of infinity" are set-theoretic, and therefore purely mathematical principles. Gödel's conjecture seems to be doubtful, since so far we do not even have accepted set-theoretical axioms which replace the philosophical reflection principle in the consistency argument for ZFC that was sketched in Section 4.1. But this passage does show an awareness of the fact that the notion of truth is not a purely mathematical notion.

4.3. *The Objector's Last Stance*

But one may wonder at this point why it should not after all be insisted that for a sentence to be a mathematical truth it *is* sufficient to be a mathematical sentence and to be true? How could one deny that? Isn't it simply *implied by the meaning* of the expression 'mathematical truth'?

The best the defender of the Thesis can do by way of response would probably go along the following lines. Pure mathematics as a social practice is concerned only with what can be proved from basic *mathematical* principles alone. The mathematical truths are the truths that mathematics is concerned with. Therefore the class of mathematical truths is determined by what can be proved from basic mathematical principles. This is not to deny that there are unknowable truths about the mathematical universe, or that there are truths about the mathematical universe that can only be known in (at least partly) non-mathematical ways. It is just that mathematics as a social practice is not concerned with them.

Someone who has been thinking all the while in terms of a higher-order axiomatization of ZFC (ZFC^2, let's say) could remark that in this case formal derivability does not coincide with semantical consequence. So there exists an alternative to the above characterization of the collection of mathematical truths,[20] namely to identify the collection of mathematical truths with the collection of *semantical consequences* of ZFC^2. On this characterization, Con_{ZFC^2} does count as a mathematical truth. Even either the continuum hypothesis or its negation will on this proposal be a mathematical truth.[21] The reaction of the defender of the Thesis to this alternative will be as before. If no mathematical principles will ever be found from which Con_{ZFC^2} can be deductively obtained, or on the basis of which the continuum hypothesis can be decided, then mathematics as a social practice is not concerned with these propositions. They are then not truths *of* mathematics.

A comparison of the concept of mathematical truth with that of *logical truth* is instructive here. The language of *pure* first-order logic with identity (containing no non-logical symbols) can express that there are at least five objects; the language of pure second-order logic can express a principle from which the Peano axioms can be derived. Many of us take the Peano axioms and fortiori the statement that there are at least five objects to be true; few of us take these to be logical truths. This leads Boolos to draw the following conclusion

> Here we should note that a truth's being couched in purely logical terms is not sufficient for it to count as a truth *of* logic, a logical truth, a truth which is true solely in virtue of logic. A distinction needs to be drawn between truths of logic and truths expressed in the language of logic. (Boolos 1995, 246)

Seen in this light, is it so clear *who* is being unfaithful to the meaning of the notion of mathematical truth: the defender of the Thesis or the objector?

4.4. *Comparison with Isaacson on Arithmetical Truth*

The Thesis about mathematical truth and its response to the 'obvious objection' are deeply related to the theory about *arithmetical* truth that has been proposed and defended by Daniel Isaacson.[22] In two important articles he argues that the collection of arithmetical truths coincides with the collection of theorems of first-order Peano Arithmetic (PA).[23]

For Isaacson the notion of arithmetical truth is in part an epistemological notion. For a statement to be an arithmetical truth it is not in general sufficient that it belongs to the formal or informal language of arithmetic and is true in the structure of the natural numbers. In addition, its truth must be "directly perceivable on the basis of our [...] articulation of our grasp of the structure of the natural numbers *or* directly perceivable from truths in the language of arithmetic which are themselves arithmetical" (Isaacson 1987, 217).

Isaacson argues that the statements the truth of which can be perceived in this way are precisely the theorems of PA. In this sense, PA is complete for arithmetical truth (Isaacson 1987, 222). For seeing that any particular statement that is unprovable in PA is nevertheless true in the natural number structure, insight is required into concepts that are not strictly speaking arithmetical. Such notions are called *higher-order concepts* by Isaacson. Examples of higher-order concepts are the notion of well-ordering, consistency of a formal system, provability in a formal system, and truth (Isaacson 1992, 96).[24] For example, the principle of induction up to the ordinal ϵ_0 is a truth which can be 'expressed' in the language of first-order arithmetic (via coding). But to see that this principle is true, insight is required into the notion of well-ordering, which is a set-theoretical and not a purely arithmetical concept. Therefore the principle of induction up to the ordinal ϵ_0 is not, in Isaacson's view, an arithmetical truth.

The present paper is concerned with the much broader notion of *mathematical* truth. So in order to make Isaacson's consideration relevant to our present concerns, we would have to replace everywhere in Isaacson's arguments the notions 'arithmetical' and 'PA' by 'mathematical' and 'ZFC', respectively. Indeed, were we to do so, we would arrive at a position that is fairly close to the position that is put up for scrutiny here. For instance, the earlier assertions that mathematical truth coincides with ZFC and that G_{ZFC} is not a mathematical truth are recovered in this way.

It is therefore no accident that many parallels can be drawn between Isaacson's argumentation and the arguments that have been put forward above in support of the Thesis. For one thing, neither Isaacson nor the defender of the Thesis can aspire to *conclusively establish* that nothing beyond PA (ZFC) is an arithmetical (mathematical) truth.[25] The best they can do is to show how particular putative counterexamples do not, on closer inspection, refute our respective thesis. Nevertheless, Isaacson appears to be in a somewhat more comfortable position than the defender of the Thesis. For the possibility that new irreducible axioms of set theory will become established in the mathematical community seems more realistic than the possibility that new irreducible first-order axioms of arithmetic will become accepted by the mathematical community. For one thing, there has been a tradition of actively searching for new set-theoretic axioms, whereas no counterpart of this activity has existed for arithmetic.

A distinction between Isaacson's position and the one that is investigated here is the fact that according to Isaacson, it is not sufficient for a *sentence* to belong to the informal or formal language of arithmetic in order to be called *arithmetical* in his sense of the word. In addition, what the sentence in question says about the natural numbers has to be directly perceivable or understandable on the basis of our grasp of the natural number structure. G_{PA}, for instance, is not of this kind.[26] It is a sentence of the formal language of first-order arithmetic. But its arithmetical content appears not to be directly comprehensible by us. We can assign concrete content to it only via the technique of coding (Isaacson 1987, 213–214).

In contrast, in this paper, the notion of 'mathematical statement' has been used in its more customary, nonepistemic sense: a sentence is a mathematical sentence if and only if it belongs to the (informal) language of mathematics.[27] This appears to be more in accordance with the general usage of the expression. In any case, all that really matters for the Thesis is that mathematical *truth* is, in part, an epistemological notion. Moreover, this seems to be a point that Isaacson could accept. It is telling that his assertion "... the notion of a statement in the language being arithmetical is *epistemic*" (Isaacson 1992, 95) is immediately followed by: "It has to do with the way in which we are able to perceive the statement's truth or falsity" (Isaacson 1992, 95). All that matters for the thesis that Isaacson wants to defend is that arithmetical *truth* be taken as a partially epistemic notion. It is of less importance for his central claim to take a stance on whether the notion of 'arithmetical sentence' is also in part an epistemological concept.

For the purposes of the present paper, it is crucial to make a distinction within Isaacson's notion of higher-order concepts between mathematical and philosophical concepts. Some of the 'higher-order' concepts that Isaacson cites (the notion of well-ordering, for instance) are clearly mathematical concepts, whereas others are not purely mathematical notions (the notion of truth, for example). Nothing in what Isaacson says precludes making this distinction. But for the thesis that Isaacson wants to defend, making this distinction is not necessary.

In sum, with the qualifications that were made above, it seems fair to say that the Thesis explored in this paper can be regarded as an *extension* of Isaacson's philosophical account of arithmetical truth. However, as mentioned above, the Thesis is in some respects less secure than the one for which Isaacson has argued. But there is a deep connection between the two.

5. MATHEMATICAL TRUTH AND NATURALISM

In the previous section it was argued that the Thesis is not so easily refuted, and that it appears to be in line with the sensitivities of the mainstream mathematician. It will now be argued that positions that assert that much more is mathematically true than what is provable in ZFC, or that much less is mathematically true than what is provable in ZFC, are in danger of being out of step with mathematical naturalism.

5.1. *More Mathematical Truths?*

There are statements of the language of set theory that are known to be independent of ZFC and that have been proposed as new axioms. Think only of large cardinal axioms, Martin's Axiom, V = L, Determinacy Axioms, and so on. They certainly have to be taken to be *mathematical* assertions. After all, they are expressed in the language of set theory, which is part of the language of pure mathematics. From each of these principles propositions like G_{ZFC} or Con_{ZFC} deductively follow (in the context of ZFC). Is this not a good argument for taking G_{ZFC} and Con_{ZFC} to be mathematical truths?

As hinted at before, the defender of the Thesis will reply that *none* of the principles, formulated in the language of set theory, that are independent of ZFC, have become accepted by the set theoretical community as new axioms. Nor even is there universal or near-universal agreement about the *truth* of any single one of them.[28] In set theory today, you have proved something if you have proved it from ZFC; if you have proved it even assuming only one inaccessible cardinal, then you have proved it *under a hypothesis*. A reflection of this attitude is the following table, given by Shelah and based on his impression, in which numbers measure the value of types of set-theoretic results on a scale of 0 to 100 (Shelah 1993, 4):

	Jensen	Magidor	Shelah
Consistency	40	40	30
From V = L	65	50	35
From large cardinals	50	60	40
From ZFC	100	100	100

This attitude is also reflected in the amount of effort that was invested to prove Solovay's celebrated theorem that there is a model of ZFC in which every set of reals is Lebesgue-measurable *without* assuming an inaccessible cardinal.[29]

In sum, large cardinal axioms, determinacy principles and the like are not, insofar as is known today, *mathematical truths*. Although there is at present no way of telling, the possibility cannot be excluded that this situation will change in the future. *If* at some point in the future a large cardinal principle, for instance, will rightly be regarded as a relatively uncontroversial basic axiom of set theory, then the Thesis will be seen to be false after all.

A different way of arguing that more is mathematically true than is dreamt of in ZFC would be to insist that there are legitimate concepts of pure mathematics which cannot be adequately formalized in the language of ZFC. This amounts to questioning even the non-reductionist connection between ZFC and informal mathematics that is posited by the Thesis, namely that ZFC contains *good representations* of all concepts of pure mathematics. In this context it can be noted that there seems to be no interesting set-theoretic representation of the notion (or notions, if there are more than one) of absolute randomness, which plays an important role in probability theory, that Church's thesis cannot be formalized in ZFC, that the informal notion of constructiveness of a proof cannot be formalized in ZFC and that the notion of a category cannot be formalized in ZFC.

No uniform reply can be given to this objection; the defender of the Thesis will have to look at each proposed counterexample separately.

Of some of these notions one should say that they do not belong to pure mathematics. Here the notion of an algorithm and the notion of absolute randomness can be cited. Thus probability theory and computer science are relegated to applied mathematics. The fact that these theories are nowadays generally seen as *not* belonging to pure mathematics is witnessed by the fact that in most universities they are housed in a separate department (department of statistics, department of computer science).

Of some of these concepts there *are* good set-theoretic representations. The notion of a Turing machine is a satisfactory mathematical analysis of the informal notion of an algorithm,[30] and Turing machines can themselves be given an adequate representation in terms of sets of ordered quadruples. Its status is then similar to the status of the analytic notion of a limit, of which few people doubt that there is a good set-theoretical representation.[31] And it should be emphasized that the relation between the informal notion and the set-theoretic representation is not itself a mathematical truth.

Finally, the status of the best theories we have today concerning some of these notions can be questioned. The foundational status of category theory still is controversial today – even though it has become an indispensable tool in some areas of mathematics (e.g., algebraic topology, algebraic geometry and pure algebra). [32] And there is probably too much disagreement among constructivists today for us to see a solid body of mathematical knowledge there. It may also be that the preoccupation of constructivistic mathematics with *epistemic* notions precludes it from being considered part of mainstream pure mathematics.

5.2. *Fewer Mathematical Truths?*

Many mathematicians take it to be the primary goal of set theory to systematize the collection of acceptable proof principles of pure mainstream mathematics. If set theory is included in mainstream mathematics, then it follows that all the theorems of ZFC are mathematical truths. For even of the set-theoretic axioms that are of least use outside set theory (the Axiom of Foundation and the Axiom of Replacement), essential use is made in set theory.[33]

But many mathematicians do not consider set theory as a part of mainstream mathematics. Their reasons for this are at least twofold. First, the connections between set theory and other mathematical disciplines are not deep enough.[34] The set-theoretical community is aware of this, and is working hard to change it. Secondly, and perhaps more importantly, set theory seems ill-equipped to solve the questions that naturally arise in set theory.[35] It is not *much* of an exaggeration to say that almost every question that naturally arises in set theory is independent of ZFC.[36]

If on the basis of these considerations set theory is not included in mainstream mathematics, then the Axiom of Foundation will not be regarded as a mathematical truth, for it is never used outside set theory.[37] Some have held that Replacement is never used in mathematical practice. But that situation has changed since the mid-seventies. There now *are* theorems of ordinary mathematics which require Replacement for their proof.[38] In sum, if this line is taken, then the Thesis should be replaced by something like: *The collection of mathematical truths coincides with ZF – Foundation + Choice.*

One could reject the emphasis, in the discussion so far, on generating *principles* that are to be accepted wholesale or to be rejected. Instead, one could say, the emphasis should be on the number of *ranks* of the set-theoretic hierarchy that are needed to carry out our mathematical constructions. An alternative to the Thesis in this vein, could then be, e.g., that at most all sets of rank ω^{ω} are needed in mathematical practice.

In sum, if the game is to formulate natural *general principles* from which all ordinary mathematics can be deduced and set theory is not included in ordinary mathematics, then one is lead to something like ZF – Foundation + Choice. But if the game is to gauge the amount of set theory that is needed not in terms of general principles, but in terms for instance of ranks, then very different answers may be forthcoming.

6. MATHEMATICAL KNOWLEDGE

6.1. *The Parallelism between Mathematical Truth and Mathematical Knowledge*

The position described in the preceding sections says, roughly, that insofar as we know today, the class of mathematical truths coincides with the class of sentences provable in ZFC. It is tempting to then go on to say that the class of mathematical sentences that *are* known is the class of theorems of ZFC that have *actually* have been proved. But we already know that that cannot be exactly right: Con_{ZFC}, for instance, is, at least according to many mathematicians, known to be true. The obvious way to weaken the claim then is to retreat to the familiar thesis that the class of mathematical sentences that are known is the class of mathematical sentences that have actually been proved – *not* necessarily in ZFC.

This last position is sometimes rejected as still being too restrictive. Steiner holds that at least some of Ramanujan's true conjectures were *known* by Ramanujan even though he did not have a proof of them.[39] A perhaps clearer example is the following. A famous mathematician proves a theorem, tells you that she has proved it and that it has been checked by her colleagues. You understand the proposition expressed by the theorem but do not know the proof (nor do you know any other proof of the theorem). In such a case, there is a strong tendency to say that you *know* the proposition expressed by the theorem.[40]

When he was asked in an interview what mathematics is really about, Andrew Gleason responded:

> ... proofs really aren't there to convince you that something is true – they're there to show you why it is true. That's what it's all about – it's to try to figure out how it's all tied together. (Gleason 1990, 86)

If that paints the right picture, then not all knowledge of mathematical sentences should count as *mathematical knowledge*. Between knowledge of a mathematical proposition and mathematical knowledge a distinction should be drawn which is parallel to the distinction between mathematical truths and true mathematical propositions. It can be conceded that there are many sources of knowledge of mathematical sentences, so that there is no *uniform* manner in which we come to have mathematical knowledge. But *mathematical* knowledge is intimately connected to the informal notion of mathematical proof. One has mathematical knowledge of a proposition if one sees, at least dimly, how the proposition can be deduced from basic mathematical axioms. As a special case, it follows that one has mathematical knowledge of an axiom if one sees *that* it is a basic mathematical axiom.[41]

So one may want to concede that we know $\mathrm{Con}_{\mathrm{ZFC}}$ (since we have a partly philosophical proof of it), and yet deny that our knowledge of $\mathrm{Con}_{\mathrm{ZFC}}$ constitutes mathematical knowledge. This seems to be in line with the fact that many set theorists consider it an open *mathematical* problem to prove the consistency of ZFC, while at the same time claiming to have conclusive intuitive evidence that all axioms of ZFC are true of the set-theoretic universe.

To have mathematical knowledge of a theorem, it is not necessary that one has a proof in the framework of set theory. In fact, Kenneth Manders has rightly emphasized that *mathematical understanding* of a problem can only be generated when the problem is investigated in its proper theoretical setting (Manders 1989).[42] The *creation* of this setting is often the main ingredient in the acquisition of mathematical insight into the problem. Set theory of course is not the proper setting for the investigation of most mathematical problems. Even though all known proofs in complex analysis, for instance, can be translated into the framework of set theory, it is clear that the resulting ZFC-proofs will not yield the same amount of mathematical insight into these theorems as the original proofs do. In this sense, the claim about mathematical knowledge that is defended here has nothing to contribute to the difficult and important philosophical task of shedding light on mathematical understanding.

Also, one does not always have to have a flawless proof of a mathematical sentence in order for one's knowledge of that sentence to be mathematical knowledge.[43] It must probably be admitted that there are intermediate cases: mathematical knowledge is a *matter of degree*.[44] The following situation sometimes obtains in a mathematical field. An old conjecture has been proved to be implied by each of four or five other, far more general conjectures. All experts in the field believe strongly in the truth of the four or five conjectures, and even more so in the truth of the old conjecture. And they have the feeling that they have made some headway in proving some of these four or five conjectures (partial results have been obtained). Even in such a situation it is no exaggeration to say that we have some degree of *mathematical* knowledge of the old conjecture.[45] All this is compatible with the epistemological corollary of the Thesis that is studied here, which merely says that if one has mathematical knowledge *in the fullest degree* of a mathematical proposition, then one has a mathematical proof of it which can be translated into a proof of ZFC.

6.2. *Second Thoughts*

But even about this epistemological corollary about mathematical knowledge one can have second thoughts at this point. One can wonder if we should not after all insist that for a person to have mathematical knowledge it *is* sufficient for her to have knowledge of a mathematical proposition. Suppose, for instance, that a reliable source tells you that Raasay is made up of some of the oldest rock on earth and some of the youngest.[46] Then even if you cannot verify this for yourself you can be rightly said to have acquired a piece of geological knowledge. Why should things be different for mathematical knowledge?

In the situation at hand, one rightly, but in a derivative way, speaks of geological knowledge. But that is because ultimately the *justifying reasons* are geological ones. This carries over to our example of the famous mathematician who tells you her most recent theorem without revealing the proof. Here too, it can rightly but in a derivative way be said that you thereby acquire a piece of mathematical knowledge, because ultimately the reasons supporting the theorem are mathematical ones. But the case of $\mathrm{Con}_{\mathrm{ZFC}}$ or of $\mathrm{G}_{\mathrm{ZFC}}$ is different, at least today. The only reasons we can adduce for supporting this proposition are of a partly philosophical nature.

Therefore a comparison with theology seems more apt. Traditionally it was held that there are two distinct sources of knowledge about God. On the one hand, there is purely philosophical knowledge that can be obtained about the existence and properties of God. This body of knowledge was called natural theology (*theologia naturalis*). On the other hand there is knowledge that is based on revelation. This was called positive theology (*theologia positiva*). Some properties of God were thought to be knowable both on the basis of revelation and by purely philosophical reasoning – God's existence and his omniscience, for instance. But other facts about God were thought to be knowable *only* on the basis of revelation. An example of this is the trinity of God, i.e., the fact that God consists of three separate entities and yet is in the fullest sense one. Yet some such facts – the trinity of God is a case in point – could be expressed in the philosophical terminology of the time. Thus it would have commonly been considered a mistake to include the trinity of God in the body of *theologia* *naturalis*. In the same way, it seems to me, it would be a mistake to take our present knowledge of $\mathrm{Con}_{\mathrm{ZFC}}$[47] to be mathematical knowledge.

7. CLOSING REMARKS

I have discussed the thesis that to the best of our present knowledge ZFC not only is a good model of the body of our present mathematical knowledge, but also exhausts the collection of mathematical truths. I have tried to explicate assumptions on the basis of which this *prima facie* implausible position is after all coherent and tenable.

The basic assumption was that a distinction ought to be made between philosophical and mathematical proofs of mathematical statements. Mathematical truths have to be regarded as truths that are derivable from basic mathematical axioms, and mathematical knowledge presupposes knowledge of how the known proposition can be derived from basic mathematical axioms.

It was shown how on the basis of these assumptions it is possible to make a case for the Thesis. I have tried to show that furthermore these assumptions are defensible, and in line with a mild naturalist stance in the philosophy of mathematics. Thus, I conclude that the main thesis of the paper is philosophically tenable. I can of course not make the much stronger claim to have *established* it beyond any possible doubt. For in Section 5.2 we saw that certain positions which hold that somewhat less than what is provable in ZFC is mathematically true are not easily dismissed. And it is always possible that new axioms become established. But I do think that the distinction between philosophical and mathematical proofs of mathematical propositions, and the consequent fact that the Thesis is after all tenable are already of philosophical significance.

The defense of the Thesis that was sketched in the previous sections does not presuppose taking any definite stance on deep questions about the ontology of mathematics. One can imagine someone holding that the basic axioms of mathematics are somehow 'true by convention' and that our knowledge of them is akin to the knowledge we have of stipulative definitions, while at the same time underwriting the Thesis on the basis of the arguments that were formulated in the previous sections. But one can also imagine someone holding that our basic mathematical axioms give a partial description of a non-spatiotemporal platonic realm of which we have perception-like knowledge, while at the same time underwriting the defense of the Thesis that was discussed in this paper. Especially this last possibility seems to me significant. It sheds some light on the puzzling fact that set theorists often appear formalists and platonists at the same time.[48] *Whether* some form of platonism about mathematical objects is in the end an intelligible and philosophically defensible position is a further question which I have no hope of resolving in this paper. As mentioned in the introduction, this is the domain of the challenges that were formulated by Benacerraf. They remain squarely before us even if all that was said in the present paper turns out to be correct.

If the distinction between mathematical truths and true mathematical sentences fails to hold, then I do not see how the Thesis could be maintained. It will then probably be harder to give a fairly *precise* characterization of the class of mathematical truths. $\mathrm{Con}_{\mathrm{ZFC}}$ will be among them. And for similar reasons, so will $\mathrm{Con}_{\mathrm{ZFC+Con(ZFC)}}$, and so on. In general, we have to include all sentences generated from ZFC by including consistency statements and Gödel sentences and by iterating this process into the transfinite.[49] And we have to do something like this for *all* known ways of diagonalizing out of a given (sufficiently strong) system to obtain a sentence that is *knowably true* on the intended interpretation of the system. If in addition we insist on a 'naïve' Tarskian semantics for the language of mathematics, then the situation is completely hopeless. For then we must also face up to the possibility that the class of mathematical sentences that will *ever* be known constitutes only a small fraction of the class of mathematical truths, which appears to imply that no informative extensional characterization of the collection of mathematical truths can be given. On the bright side, if the distinction does hold, then these philosophical dream- and doom-scenarios are not directly pertinent to the question that was discussed in this paper.

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NOTES

[1] I have no arguments to offer for this position that have not already been formulated in the literature. An excellent defense of naturalism in the philosophy of mathematics is given in Maddy (1997, see especially Part III, Section 4).

[2] The *locus classicus* for these problems is of course Benacerraf (1973). The reader will become aware of the fact that I am doing my best to stay away from the tangled web of problems that are raised by the classical papers Benacerraf 1965, 1973.

[3] I mean in the way in which mathematicians do interpret ZFC, without committing myself whether this should be construed in a platonistic, nominalistic, or some other way.

[4] The classical reference here is Benacerraf (1965).

[5] The discussion about the conditions which have to be fulfilled for a set-theoretic representation of an informal concept to be a *good* representation is left for another occasion.

[6] The only exception is the semantical alternative to the Thesis discussed in Section 4.2 (cf. infra).

[7] See Kleene (1967[1952]), Chap, 3, §15.

[8] And this seems to be so independently of the fact that G_{ZFC} and Con_{ZFC} are of little interests to mathematicians because they are lacking *concrete mathematical content.*

[9] This proof is formally analogous to Myhill's proof of the consistency of PA (see Myhill, 1962). Actually this proof and Myhill's proof show something stronger: they show the *soundness* of the respective theories for their intended interpretation. Using the soundness of ZFC for its intended interpretation, the truth of G_{ZFC} easily follows.

[10] For the details, see e.g., Halbach (1996, 56–57, Theorem 10.1).

[11] In fact, this has been done. See for instance Tarski (1983 [1935]) and Kripke (1975).

[12] Z is the Zermelo system, i.e., ZFC minus Replacement and Choice.

[13] For specific purposes one might ask more of a consistency proof than merely that it is a proof from accepted mathematical principles. For instance, if one wants a consistency proof for a classical theory T which strengthens our conviction in the consistency of T, then one might insist that this consistency proof should be in a broad sense (which is difficult to describe with any precision) 'constructive' (see e.g., Sieg 1990). Now the consistency proofs for Z that are available today do not significantly increase our antecedent conviction in the consistency of Z. Indeed, given that ever more abstract and complex constructive principles are needed to yield constructive consistency proofs of strong mathematical theories, it is a legitimate question how far this demand can be taken (Howard 1996, 276). But despite the fact that they are of little interest for certain research programs in proof theory, our standard non-constructive consistency proofs for Z *are* correct mathematical proofs.

[14] There are set theorists who share this feeling, and who are trying to reverse this trend (most notably perhaps, Shelah).

[15] Admittedly, more needs to be said here. But, as Tony Martin has pointed out, it is very hard to say anything informative about these requirements, since all properties that immediately come to mind seem to be violated by some of the axioms of ZFC.

[16] Kleene, in his *Introduction to Metamathematics*, clearly regarded metamathematics in this way (Kleene 1967[1952], 62). However, he followed Hilbert in allowing only finitary methods in metamathematical proofs. The broader mathematical theory of formal systems in which also non-finitary mathematical principles are used is called *set-theoretical predicate logic* by him (Kleene 1967, 175). However, this did not become accepted terminology. Nowadays non-finitary methods are regarded as acceptable as a matter of course in metamathematics. Thus, for example, Gentzen's consistency proof for elementary number theory is today taken to be a paradigm example of a metamathematical proof, whereas for Kleene it does not belong to metamathematics.

[17] In fact, he claims that we possess no proof at all (mathematical or partly non-mathematical) of these statements (Field 1998, 110).

[18] See Field (1998, 108–112).
[19] As is done, for instance, in Feferman (1962).
[20] If, as Quine and Putnam have urged, only first-order quantification is ultimately acceptable, then because of the completeness theorem this alternative does not arise.
[21] See Boolos (1975, 518).
[22] I am grateful to an anonymous referee for pointing out the relation between my discussion of the Thesis and Isaacson's position on arithmetical truth.
[23] See Isaacson (1987, 1992).
[24] In his 1987, Isaacson also mentions second-order quantification as a higher-order notion (Isaacson, 1987, 210).
[25] Isaacson concedes this point (Isaacson 1992, 100).
[26] "In a certain way, [G_{PA}] might even, be said not to be arithmetical. It is not saying anything about the natural numbers; rather, it is 'about' the statement itself [...]. That is my viewpoint in this paper" (Isaacson 1987, 213).
[27] Cf. Section 4.1.
[28] See Jensen (1995) for an argument to the effect that this holds even for the principle $\neg(V = L)$.
[29] Shelah proved that it cannot be done: an inaccessible is necessary to construct a model in which every set of reals is Lebesgue-measurable. See Shelah (1984).
[30] The *locus classicus* here of course is Turing (1936). Excellent scholarly reconstructions and discussions of the structure of Turing's argument can be found in Sieg (1994) and Soare (1996).
[31] See Mendelson (1990).
[32] Thanks to an anonymous referee for pointing this out.
[33] Replacement is needed for developing the theory of ordinals (Lavine 1994, 122–123), and Foundation is needed for associating ranks with all sets (Kunen 1980, 101).
[34] In a discussion, Mark Steiner gave this as an important reason for not taking set theory to belong to mainstream mathematics.
[35] Cf. the quote from Shelah (1994) that was discussed in the introduction.
[36] For a discussion of some of the questions that arise naturally in set theory but are undecidable in ZFC see Kunen (1980, Chap. 2).
[37] See Kunen (1980, 94) and Lavine (1994, 146).
[38] See Friedman (1971).
[39] Burge agrees with Steiner on this point. See Burge (1998, 25).
[40] Burge argues that in such cases you not only know the proposition expressed, you know it *a priori* (Burge, 1993, 466–467). His reason for this claim is that in situations such as the one that was sketched here, specific sense experiences or perceptual beliefs do not play a *justifying* role for the belief obtained by interlocution.
[41] It is a further question *how* we come to have such knowledge. This question has been much discussed over the recent decades (see e.g., Benacerraf 1973), and I have nothing to say here that has not already been said in the literature. To repeat, I am doing my best to avoid having to, in order to make a point, solve one or both of Benacerraf's problems.
[42] This, incidentally, is the reason why Isaacson takes seriously the possibility that his thesis about arithmetical truth has to be qualified somewhat, to the effect that perhaps not *all* theorems of PA count as arithmetical truths (Isaacson 1987, Section 6). He is worried that, for instance, the PA-proofs which prove the consistency of weaker systems of arith-

metic do not generate insight into *why* these systems are consistent. PA does not provide the right setting for the investigation of such problems.

[43] In a similar vein, Burge argues that non-demonstrative reasoning in mathematics is *a priori* (Burge 1998, 3).

[44] Mathematical truth, in contrast, is *not*, on the view that we are entertaining, a matter of degree.

[45] I was told that something like this was the case for Fermat's last theorem before Wiles proved it.

[46] This example was given by an anonymous referee.

[47] If we can be said to know this proposition at all, that is, if our philosophical consistency proof is convincing at all.

[48] Cf. again the passage from Shelah (1994) that was discussed in the introduction. It is significant in this context that Isaacson sees no difficulties in combining his theory about arithmetical truth with a form of conceptual platonism in the general philosophy of mathematics (see Isaacson 1994).

[49] Of course diagonalization procedures such that the *only* way to prove the soundness or the consistency of the resulting diagonal sentences is by invoking even relatively small large cardinals won't do for this purpose. So Friedman's way of generating sentences that are strongly independent of ZFC (see Friedman 1981) is not immediately relevant here.

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