Commentary

The Problem of Disagreement in Mathematical Philosophy: How Far Can It Go?

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This article stresses the need for accurate interpretation of mathematical results in meta-language, e.g., in English, by philosophers of mathematics. When we try to interpret, or unfold, mathematical statements in a compact and coherent manner in meta-language, then we often produce oversimplified statements which give us nothing but anxiety. The article exposes one path, among the many possible, through sources in the philosophy of mathematics that seems to exhibit a possibility of crucial doubt related to the truths of the Standard model of arithmetic and, implied by this doubt, disagreement among mathematicians. As a result, the flawed reasoning underlying such doubt is exposed.

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Introduction

In the enormously wide field of philosophical inquiry, one almost inevitably encounters speculations about a priori matters that usually force the person to take a stance toward such speculations, whether refuting, supporting, or avoiding the importance of the subject under scrutiny. Yet, we have at our disposal a well-developed area of human thinking that exhibits a great deal of explanatory power and treats seriously the option to speculate about a priori matters - it is mathematics. For example, one can be interested in digging into problems of epistemology and, in particular, moral epistemology, trying to develop moral intuitionism in the manner described by Audi[1]. Thus, such concepts as “self-evidence” of a proposition or “justification of truth value of the proposition on the basis of proper understanding of the meanings of its constituents” could be of interest. It is understandable that this author and many

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others tried to borrow a justification power from mathematics in this situation. The success or failure of such an approach is not considered in this article. This article is rather concerned with the question of what skills and academic background one should possess in order to feel confident in pursuing interdisciplinary research on the verge of philosophy of mathematics and other humanitarian problems. The question does not seem too complicated. One can be a philosopher in the broad sense and educate himself/herself in the philosophy of mathematics. Another can be a mathematician who happens to feel a nagging worry about a philosophical problem far outside the scope of mathematics and decides to deepen his/her understanding through self-education. However, the best scenario is if one has a solid academic background and skills in both areas and can playfully switch the vector of the train of thoughts between those areas. I had deemed myself to be such a person for having a fairly good background in the theory of probability, having studied ergodic properties of Markov processes in my youth, and having a degree in humanities, e.g., in theology. It turns out I was wrong. The process of thinking in order to merge the philosophy of mathematics and the humanitarian inquiry could be a painful and somewhat rough experience. Further, I expose one path, among the many possible, through sources in the philosophy of mathematics that I took recently, and some uncomfortable pitfalls in which I found myself. Obviously, the description does not claim to be general. Yet, it may serve as a case study, or even encouragement, for others who can feel lost, encountering texts in the philosophy of mathematics that turn one's thinking into an attempt at "herding cats" instead of giving a clear and concise picture of pure and solid mathematics. Also, I dare to think that some arguments in this article could be of value to mathematicians working with Peano Arithmetic.

Are all mathematical theories born to be equal?

While trying to entertain myself by scrutinizing more or less voluminous arguments considering philosophical problems, I came across a recent book by Clarke-Doane[2]. In general, I enjoyed the book and found it to be a concise overview of the current situation in the philosophy of mathematics, in particular, the overview of our doxastic attitudes toward mathematical axioms. I would especially note the idea that we can, and probably should, consider discrepancies in the metaphilosophical status of different axioms while stating our concerns and disagreements about the axioms. For example, Clarke-Doane[2] proposes to treat some axioms as "structural" such that they define the structure of mathematical objects and concepts under scrutiny. Thus, for instance, in the study of the mathematical groups, 'there is no nonverbal question as to whether the *Axiom of Commutativity* itself is true. It is true of

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Abelian groups and false of the others' (p. 3; Clarke-Doane's emphasis). Other axioms are "foundational". They give base for our 'metatheoretic reasoning'[2]. Indeed, one should have a stance toward, let's say, a claim about the consistency of a theory T because it is hard to give an equal epistemic value to the scrutiny of both structures: T+(ConT) and T+(~ConT). Yet, Clarke-Doane[2] proposes a third kind of axioms, which does not deserve a special name, though it seems to bear significant importance. The instance of such a kind is the parallel postulate - 'unlike group theory, this is not because geometry is about its class of models. It is because, if geometric reality exists, it is rich enough to afford an *intended model* of the postulate and its negation'[2] (Clarke-Doane's emphasis). It is a controversial claim. One is allowed to assert that geometry is about its class of models. One can also argue that Abelian groups expose quite a rich mathematical realm rooted in reality. However, I am in favor of the above approach in general, supposing that grasping some axioms is deeply wired in our cognitive abilities.

I allow myself to put forward a sort of naïve psychologism only to explain my intuition behind the above argument. If one favors an a priori approach toward mathematics, then a speculation about the necessity of two Kantian-like pre-theoretical a priori intuitions as representations of space and time is useful. In turn, if one favors a more constructive a posteriori view on mathematics, then a thought experiment related to the development of a cognitive agency of a newborn child may convince us that the first experience of the child should be of space and time. At least, sooner or later we expect a child to sense the spatial boundaries of his/her body and the timing of circles of his/her bodily functions. Thus, our placement in space and time frames our cognitive agency, at least to a certain degree. This somewhat naïve psychologism may push forward an intuition that most basic mathematical representations of space and time, and consequently their axioms, bear a special weight in mathematical thinking. I would propose to give special weight to geometry and arithmetic, where arithmetic represents time as an ability *to count* and geometry is loaded by the reality of space in the sense proposed by Clarke-Doane[2]. Thus, if I decide to speculate about axioms, I have a rationale for why I give different epistemic weight to problems and disagreements related to different branches of mathematics. Maybe it is a poor rationale, but at least I have one. It is important to emphasize that the above intuition doesn't entail one's commitment to mathematical constructivism or nominalism. For example, l am personally inclined to uphold a middle-ground position between logical and mathematical realism akin to Dummett's view[3].

I use Clarke-Doane's book[2] as an example of a text dedicated to the philosophy of mathematics because, in my view, this work exhibits several interesting new arguments; yet, it also demonstrates one widely disseminated, rather unsatisfactory, approach to arguing. There are engaging Clarke-

Doane's[2] arguments, e.g., the proposal to import epistemological conditions of sensitivity and safety into arguments about the reliability of mathematical knowledge. However, I want to emphasize one particular feature that persistently occurs in many, if not all, texts in philosophy of mathematics. It is a situation when a philosopher takes a meta-philosophical argument and feels free to support or refute it by any means of any mathematical theory. Thus, the reader often can be bewildered by this magic-like pull of mathematical problems out of the philosopher's sleeve. For example, consider the earlier-mentioned problem in moral epistemology; suppose that one wants to weaken the grip of the Sceptical argument about the non-existence of moral knowledge with the help of seemingly solid ground for beliefs in such propositions as axioms of Peano arithmetic. Immediately, an objector may come saying that the approach is poorly justified as we are still on the fence concerning the Axiom of Choice (AC) vs. Continuum Hypothesis (CH). The objector may assert that all mathematics is basically Set theory and that set-theoretic representations of Peano axioms and Peano arithmetic are the same, at least for the purpose of epistemological speculations outside pure mathematics. Thus, our beliefs in Peano axioms lack epistemological ground to export them by analogy into moral epistemology. Case closed.

I disagree with the above state of affairs in the philosophy of mathematics, especially in cases where mathematics imports meanings into meta-philosophical matters or areas of epistemological inquiries. Indeed, Set theory claims to be the foundation of mathematics. Yet, one's epistemological journey in mathematics begins with arithmetic and geometry. Thus, it is possible to give a special epistemological value to the axioms of geometry and arithmetic for a knower while recognizing the importance of the problematic of Set theory for a mathematician. It is the area where knowledge and the knower have to be differentiated despite their tendency to merge too often.

The existence of disagreement among mathematicians over different mathematical theories is not one-dimensional. There's nothing wrong with the disagreement per se. The problem arises when the disagreement goes too far and a reader of a mathematical/philosophical text loses the consistent comprehension of what mathematicians really believe. For example, one may feel worried reading the following quotation by Zeilberger: 'I am a platonist...[but] I deny even the...axiom that every integer has a successor'[2]. Why did Clarke-Doane choose to quote this sentence in the context of objections toward the concept of Reflective equilibrium? The assertion is obviously spicy. Yet, in his conference talk, Dr. Zeilberger[4] says that he is not a professional philosopher of mathematics; he just upholds the position of ultrafinitism. Thus, in his view, any kind of infinity is 'completely meaningless'[4]. This seems to be a bit extreme. Surely, Dr. Zeilberger has all the rights to uphold any philosophical views. Yet, should one be

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worried about the views of mathematical finitism in the context of reading an overview related to the philosophy of mathematics? In the next paragraph we are going to probe the implied conclusion that there is something wrong with arithmetic.

A case

The following selection of articles related to problems arising at the intersection of Peano arithmetic and Set theory is not exhaustive; it rather illustrates one of many possible trajectories of a reader of philosophical/mathematical texts who may find that even seemingly well-grounded beliefs in arithmetical truths can be doubted. Thus, it would be proper to consider the following chain of arguments as a case study of the decline of these beliefs' credibility. However, there's hope to restore, at least partially, one's trust in arithmetic.

In a freshly published, by academic standards, article, Freire[5] proposes an unexpected and interesting modality of consideration of the problem of disagreement among mathematicians. It reminds me of a philosophical thought experiment. Yet, the outcomes of the experiment are bound with proven mathematical theorems rather than with just pure verbal speculations. Suppose that two independent communities of mathematicians were responsible for the development of Peano axioms (PA) and axioms of Zermelo-Fraenkel (ZF) set theory. 'How likely are they to be coordinated regarding PA's interpretation in ZF?'[5]. The answer to this question is that coordination is possible, yet it should be carried out in a manner of constant collaboration between the above two groups of mathematicians. Freire[5] points out that when discrepancies occur between some extension of ZF and an interpretation of PA, the adequacy criteria should be applied to both the mathematical theories and, also, 'the adequacy of an interpretation should have reasons for itself apart from accommodating the interpretation' (p.9). However, the major concern of this article is to what extent we can trust in the truths of the Standard model of arithmetic. All standard models of arithmetic are the same up to isomorphism. By definition, a non-standard model is the model that is not isomorphic to the standard. One can see non-standard models as extensions of the standard where some mysterious objects may be added such as infinite cardinals or other sorts of weird numbers, sentences that the non-standard model sees as well-formed while it is not obvious in the standard model, etc. Yet, one's sincere assertion that the precise border where standard arithmetic ends and non-standard begins is known would be rather misleading. Otherwise, Dummett's[3] attempt to redeem mathematical logicism by amending the definition of natural numbers through a concept of infinitely extensive totality would be an empty game of a mind. Non-standard models of arithmetic are

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an enormously interesting and complex area of mathematics. Yet, they add little worry to the above concern of this article. A philosopher of mathematics can develop an argument standing on the ground of what is known or by probing the matters of what is not known yet. Our modest goal is to help the former endeavor. Let's simplify the concern: do qualified mathematicians see a problem with the arithmetic that was taught to us in school? Thus, further, we proceed with the analysis of the parts of Freire's[5] work related only to the standard models of arithmetic.

Friere[5] calls the model $N=\langle\omega, +, *, 0, s\rangle$ standard and considers ZF theory as 'expressive enough to define a truth predicate for this interpretation' (p. 3). Freire[5] points out that 'one should offer extra-logical (or second-order) reasons for choosing *N* from the myriad possible models for arithmetic' (p. 3). It is also noted that another strategy is to offer an epistemology for arithmetic truths beyond model-theoretical reasoning. However, the further substantial part of Freire's[5] article is dedicated to the former approach. Freire[5] provides proof to the following theorem: '*For a given interpretation* I *of* PA *in a recursive extension* S *of* ZF, *there will be formulas of* L(PA) *such that* S *do*es *not prove* φ^I and S *does not prove* $(\text{not-}\varphi)^I$' (p. 6).

Here, φ is a formula of L(PA) and φ^I is an interpreted formula in S. Also, let us define $A=\{\varphi|\ S \text{ proves } \varphi^I\}$. A sketch of the proof goes as follows. Freire[5] introduces a provability predicate Ths and an arithmetization and a Gödel's coding for arithmetic and set-theoretic formulas. The rest is obvious. We use Gödel's "fixed point" lemma for the formula (not-Ths). Thus, there exists an arithmetical formula λ, such that A proves $\lambda \longleftrightarrow \text{not-Ths}(\langle\lambda\rangle)$, where $\langle\lambda\rangle$ is the Gödel's number. If S proves λ^I, then A proves $\text{Ths}(\langle\lambda\rangle)$; therefore, we obtain a contradiction. To perform analogical reasoning in relation to the supposition that S proves $(\text{not-}\lambda)^I$, Freire[5] advises us to replicate 'the Rosser trick' (p. 7). Of course, the reader is encouraged to familiarize himself/herself with the original variant of the Freire's[5] proof. Now, let me propose several concerns that may bother an attentive reader of this theorem and its proof.

The first concern is the difficulty of operational mastery of Gödel arithmetization and coding in the application of the "fixed point" lemma in a particular mathematical setting. Salehi[6] provides a very clear consideration of how Tarski's undefinability theorem of arithmetical truth follows from Gödel's diagonal lemma; yet, he points out that the standard proof of the diagonal lemma resembles "pulling out a rabbit from a hat". I agree with this assessment because I am conscious of my personal experience. I feel an amazing sense of clarity and simplicity while considering Cantor's diagonal argument. Not only that, but I am even ready at any time to replicate this argument without consulting any notes. Gödel's diagonal argument poses quite a different challenge. It is not easy for me to merely keep track of steps of Gödel's original proof. It could be the problem of my lack of mathematical talent; yet, Salehi[6] gives an extensive set of quotations from other academics who seem to agree with the above picture. For example, Salehi[6] quoted a rather artistic but keen observation of McGee[7]: 'You would hope that such a deep theorem would have an insightful proof. No such luck. I am going to write down a sentence φ and verify that it works. What I won't do is give you a satisfactory explanation for why I write down the particular formula I do. I write down the formula because Gödel wrote down the formula' (p. 1). Thus, every time when we encounter a non-original Gödel's proof but an analogous process of steps applied to a different selection of mathematical objects and concepts, even a slightly different one, we would like to be sure that all the steps of arithmetization and Gödel's coding are adequate to the problem in question. Also, we would be happy to see a particular sentence, the "fixed point"; if such an extraction is not feasible, then, at least, we would like to know something about its characteristics. For example, in the above-mentioned Freire's[5] theorem, does the recursive extension S "see" the "fixed point" λ^I as a bounded and/or finite well-formed arithmetic formula? I am posing this question here because it seems to be supposed implicitly that the reader should be qualified to answer it independently. Yet, Freire's[5] recommendation to also perform Rosser's trick independently gives rise to my second concern related to the above theorem.

Rosser's theorem deals with a provability predicate involving analysis of the length of a proof and a negation function. It also replaces Gödel's demand for a recursively axiomatizable theory T to be ω-consistent with simple consistency. When restricted to first-order language, PA is known to have an infinite number of axioms; yet, PA is recursively axiomatizable. Freire[5] has extensively developed the procedure of building the interpretation part (from PA to ZF) of the provability predicate and proposes that we should just apply his apparatus to the process of building the provability predicate shown in Rosser's theorem. Thus, there's a supposition that all will work smoothly. Nevertheless, one can wonder what happens with the infinite set of axioms of PA in the process of interpretation in ZF. Especially important is the question of the behavior of axioms of induction. How are they considered: as an infinite set of sentences or as schemata (principle)? Can it be the case that some sort of second-order logic is smuggled there via quantifying over a domain of subsets or formulas? Clearly, first-order logic would not be enough to describe such induction schemata. There is nothing wrong with considering second-order logic in principle, or with the infinitely axiomatizable interpretation. It is just unclear whether all possible ramifications are checked by Freire[5] while forming and interpreting the provability predicate. Still, Freire's[5] scheme of proof seems to be correct.

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All the above concerns could be only subjective, or even just esthetical, complaints if not for the following source cited by Freire[5], because his result 'relates to results available in *Satisfaction is not absolute*' (p. 7; Freire's emphasis). Hamkins and Yang's[8] article "Satisfaction is not absolute" is cited by a substantial number of authors. Freire[5] provides the reference to the article via the authors' website. However, I was able to find only a shortened version there. Thus, further, I will use the full text from the database arxiv.org. Maybe this particularity explains the difference in attitudes, mine and Freire's, toward the Hamkins and Yang's[8] results. Nevertheless, we should pay attention to how Freire[5] expresses his understanding of those results: 'There may be arithmetical formulas ρ that two models of ZF disagree - even as these same models agree on what is the standard model for arithmetic' (p. 7). Even so, 'the result lacks a construction for the ρ formula'[5]. Indeed, Hamkins and Yang[8] do not explicitly provide us with the formula ρ due to the above-mentioned difficulty in tackling Gödel's arithmetization and coding while applying the "fixed point" lemma. Yet, Hamkins and Yang[8] claim that the disagreement over ρ is not a disagreement due to some incoordination of a meta-language; they claim that the disagreement is due to a possibility to adequately equip models of ZF with truth predicates disagreeing on the matter of ρ and obeying 'recursive requirements of the Tarskian definition'[8]. This is a strong claim that allows Freire[5] to say that formula ρ is 'obtained as the existential for a number representing a formula. In fact, exhibiting ρ is not possible, since it would imply the inconsistency of ZF' (p. 7). But what does the last statement even mean? What if in the future, a genius will be able to demonstrate ρ, whose existence is based on the existence of a particular Gödel's number, which is also not demonstrated clearly by Hamkins and Yang[8]? Will ZF just "collapse" in the a priori Platonian-like realm? The answers to those clumsy questions can be obvious for an expert in Set theory. Yet, as a simply attentive reader, I would like to make sense of the conundrum by tracking down the Hamkins and Yang's[8] results and giving them a more coherent description in plain English.

We are going to omit the discussion of subtitles of the first theorem presented by Hamkins and Yang[8]. The theorem asserts that every consistent extension of ZFC has two models that agree on the structure of $\langle N, +, *, 0, 1, <\rangle^M$ but disagree on the truth of some arithmetic sentence σ. The proof of the theorem is exposed as a sort of a blueprint. Gödel's coding is barely noted in the context. The major part of consideration following the formulation of the above theorem is dedicated to the construction of a particular truth predicate. It is stated that there is a truth predicate, or a full satisfaction class, Tr, for a given model of arithmetic *N*, 'such that Tr obeys the recursive Tarskian definition of truth'[8]. Several

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axioms for the predicate are provided; they seem to preserve first-order logic because an existential quantifier is applied to variables but not formulas. The list is not exhaustive, but it may possibly suffice for the purpose. It is postulated that 'Tr is applied only to assertions in the language of arithmetic, not to assertions in the expanded language using the truth predicate itself'[8]. This gives us a hint that we are in the realm of Typed axiomatic theories of truth. Thus, first-order logic is preserved as opposed to Type-free theories of truth. Also, it is postulated that Tr is inductive; thus, 'mentions of Tr may appear in instances of the induction axiom'[8]. Here is a bit unclear matter one can encounter due to the Hamkins and Yang's[8] choice of words: "instances" of "the induction axiom". So, how do they treat "the induction axiom": as an infinite set of axioms or a single axiom, i.e., axiom schemata? A reader may let it slide thinking, for example, that indeed, though two extensions of ZFC can agree on N, something strange can happen somewhere outside the standard part of arithmetic; so, those extensions may disagree on the matter of some σ, which may be not well-formed-formula by the criteria of the Standard model of arithmetic. However, the second theorem formulated by Hamkins and Yang[8] gives us much bigger worries. It says that *'there are models of arithmetic with different incompatible inductive truth predicates'*[8]. The formulation of the theorem is a bit vague. The devil is in the details of its proof. The reader is encouraged to take a look at the original proof in Hamkins and Yang[8]. Below I provide my analysis of the proof.

Hamkins and Yang[8] consider any model of arithmetic N_0 that admits a truth predicate, 'for example, the standard model $\mathbf{N}^M$ arising in any model of set theory M'[8]. Further, a consistent theory T consisting of the elementary diagram $\Delta(N_0)$ and the assertion "Tr is the truth predicate" is considered. Here, Tr satisfies 'the recursive Tarskian truth requirements'[8]. Further, the proof states that 'any model of the theory T provides an elementary extension N of N_0, when reduced to the language of arithmetic, together with the truth predicate for N'[8]. Thus, if all truth predicates have a unique satisfaction class, i.e., they all agree on the truth of all arithmetic sentences σ, then they all are definable implicitly. Therefore, by Beth's implicit definability theorem, they are definable explicitly. But we allowed extensions N to consist only of arithmetic and truth predicates. Thus, explicit definability of truth predicates should be expressed in the language of only arithmetic. It contradicts Tarski's undefinability theorem. Hence, the satisfaction class is not unique. There are incompatible truth predicates. The proof is completed.

Now we should decipher some of the steps in the proof in order to understand whether there is real danger to our beliefs about truths of standard arithmetic. Hamkins and Yang[8] use Beth's implicit

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definability theorem. It should be noted that Beth's theorem is applicable to first-order languages only[9]. Thus, we do not have any other options to grasp the Hamkins and Yang's[8] argument but to suppose that they consider extensions of Peano arithmetic with an infinite set of axioms of induction. The only trick to how one can argue about PA using strictly first-order logic is to allow the induction axiom to unfold as a set of axioms of induction, stating a separate axiom for every formula that demands inductive proof. Therefore, we must think that Hamkins and Yang[8] consider extensions of Peano arithmetic with some inductive Tarskian-like truth predicate. In this case their logic of proof stands and we arrive at the contradiction with Tarski′s undefinability theorem. Consequently, we are forced to admit that in the realm of first-order logic and Peano arithmetic there exist adequately constructed inductive truth predicates that can disagree on the truth of a sentence σ, where σ looks like a well-formed sentence. This is because we just hold all our objects and concepts in the above "normal" realm all the time. Indeed, we have a problem. One can object here that all those attempts to introduce predicates instead of Tarski′s idea of metalanguage just lose some adequacy on the way. Also, one can say that Beth′s theorem is considered to be weaker than Tarski′s undefinability theorem and, therefore, not applicable in this case[10]. However, it would be a position of merely dodging the arguments. Can we find a weak link in the chain of Hamkins and Yang's[8] proof? As it happens, we can.

It is worth taking a look at Heck's[11] article "Consistency and the Theory of Truth". The merit of this article is in the careful introduction of every definition, concept, and proof while unfolding several results when one adds to a theory T some theory of truth. Heck[11] begins meticulously listing all logical and non-logical axioms, including those that are necessary to form a language with predicates. This is the first difference between Heck′s[11] approach and the somewhat sketchy Hamkins and Yang's[8] condition that truth predicates should obey Tarskian requirements. Further, Heck[11] introduces the method of cuts, which is used for his proofs, and, finally, he formalizes compositional truth theories in a very precise manner. 'Since the semantic axioms for the quantifiers, as Tarski bequeathed them to us, make use of sequences of elements from the domain, we shall need a nice theory of sequences if we're to formalize theories of truth'[11] (quotations here and below are made by using page numeration of the online version of Heck′s[11] article). Heck[11] describes a theory of truth consisting 'of Tarski-style axioms for the logical and non-logical vocabulary' (p. 15). They are analogous, of course, to those of Hamkins and Yang[8], but with amendment for sequentiality and interpretability of theories. Heck[11] carefully defines them in the case of the language of arithmetic as we shall 'have these axioms for the non-logical constants' (p. 16);

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finally, he gives a definition of truth. ‘That is Tarski’s definition: Truth is satisfaction by every sequence’[11]. A reader is encouraged to familiarize himself/herself with the original mathematical formulations of the above axioms in the source. Here might be an objection that Hamkins and Yang[8] meant something different from Heck[11] while introducing the truth predicate. So, it is their responsibility to avoid vagueness and make explicit all the axioms for their truth theory. Further, Heck[11] considers sequential theory *T* and the theory $CT^{-}[T]$ ‘that extends *T* by adding truth-theoretic axioms of the sort just discussed for the logical and non-logical vocabulary of the language *T*’ (p.16). One more note is appropriate here. Though Heck[11] does not call $CT^{-}[T]$ inductive, he considers inductive property in detail through establishing material adequacy of the theory and whether it preserves logical axioms. Moreover, Heck[11] seems to go further than Hamkins and Yang[8], whose claim was rather vague: to allow a truth predicate to occur in instances of induction but not to be applied to itself. Heck[11] notes that ‘we do not have “semantic induction”, that is, induction for formulae containing semantic vocabulary. But we could overcome that lack by the method of cuts’ (p. 18).

Further, several interesting lemmas and theorems for sequential theories, e.g., for those that are interpretable in Robinson arithmetic Q, were proved. Note, ‘Q is not sequential, but there are lots of sequential theories that are interpretable in Q. For example, $I\Delta_0$ is sequential, and it is interpretable in Q’[11]. Here $I\Delta_0$ is Q plus induction for Δ_0 and Δ_0 consists of only bounded formulas. More importantly, Heck’s[11] results distinguish a major characteristic of such theories: being finitely axiomatizable vs. infinitely axiomatizable. This gives us reason, following Heck[11], to assert that ‘Peano Arithmetic is a special case’ (p. 26). This is because ‘if *T* is infinitely axiomatized, then there is no reason, in general, to suppose that $CT^{-}[T]$ will prove that *all* of *T*’s axioms are true, although it will prove that *each* of them is’[11] (Heck's emphasis). Thus, based on his results and Enayat and Visser[12], he proves ‘that CT^{-}[PA] *does* not *prove that all axioms of* PA *are true*’ [11] (Heck's emphasis). The explanation for PA’s anomalous behavior is twofold: it is infinitely axiomatized, and it is reflexive. ‘That gives us a sense in which what happens when one adds a truth-theory to PA can be very different from what happens when one adds a truth-theory to some other theory’[11]. So, what do we have now? A carefully described truth theory added to infinitely axiomatized Peano Arithmetic does not prove that all axioms are true, yet it proves that each one of them is. Now, if by Hamkins and Yang’s[8] supposition we can add to Peano arithmetic a truth predicate as a full satisfaction class - it should be Peano Arithmetic due to the need to apply Beth’s theorem - then by Gödel’s completeness theorem for first-order languages, *full satisfaction implies*

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provability of all axioms (author's emphasis). But it contradicts Heck's[11] results. It is obvious that I give more credit to Heck's[11] work in comparison to Hamkins and Yang's[8]. Such an attitude is based on the precise manner of Heck's definitions and proofs. Indeed, infinitely axiomatized Peano Arithmetic does not allow us to add a predicate that is merely supposed to present a full satisfaction class. At least, as we saw, the description and/or construction of such a predicate is not a platitude and bears responsibility. Now, I am perfectly comfortable if one says that such a predicate extends induction schemata into second-order language or if one says that a model of PA is non-standard. Using plain English in the former case, I can argue that it is an attempt to formalize a meta-language; in the latter, I would use the metaphor that the vicinity of infinity is a dangerous neighborhood. Non-standard models of arithmetic are an interesting area on their own but do not threaten truths of the Standard model so far due to the absence of isomorphism between them. Note, for example, work of Enayat[13], where consideration of any recursively axiomatized extension of ZF has immediate consequences in the area of non-standard models of PA.

Conclusion

In this concluding paragraph I will provide several disclaimers and remarks. I do not object to or dismiss any proof of theorems in the above analyzed articles except for one particular theorem in Hamkins and Yang[8], i.e., the second theorem by their numeration. The above list of concerns, objections, and arguments is not an exhaustive analysis of the current situation in Model theory of PA and ZF. It is rather an example of how one can wander into a crucial doubt related to the Standard model of arithmetic in the context of consideration of disagreement among mathematicians and/or philosophers of mathematics. However, it seems that I was able to find a flawed reasoning underpinning such doubt. No substantial disagreement related to the Truth theories in Peano arithmetic can exist if axioms are bound by first-order logic.

Last, I want to emphasize the need for accurate interpretation of mathematical results in the meta-language, e.g., English, by philosophers of mathematics. In an important sense, a mathematical statement is a way to compactly pack a much more lengthy statement in a meta-language. Yet, if we try to interpret, or unfold, the mathematical statement in a similarly coherent manner in meta-language, then we often produce oversimplified statements. In turn, they give us nothing but anxiety. The statement that one doubts the existence of a successor of every integer demands an elaboration[4]. It is unfortunate of us to allow the philosophy of mathematics to slide back into the doubts of a caricature image of

Ancient Greek philosophers: no thinking about infinity is allowed because our brains are finite. One can certainly think that number ω creates trouble for the concept of “being a successor”; yet, despite this uncertainty, Peano axioms keep their stability in the standard models of arithmetic.

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