

# Models and recursivity

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## Abstract

It is commonly held that the natural numbers sequence  $0, 1, 2, \dots$  possesses a unique structure. Yet by a well known model theoretic argument, there exist non-standard models of the formal theory which is generally taken to axiomatize all of our practices and intentions pertaining to use of the term “natural number.” Despite the structural similarity of this argument to the influential set theoretic indeterminacy argument based on the downward Löwenheim-Skolem theorem, most theorists agree that the number theoretic version does not have skeptical consequences about the reference of “natural number” analogous to the ‘relativity’ Skolem claimed pertains to notions such as “uncountable” and “cardinal.” In this paper I argue that recent proposals by Shapiro, Lavine, McGee and Field which aim to distinguish the number and set theoretic indeterminacy arguments by locating extra-mathematical constraints on the interpretation of our number theoretic vocabulary are inadequate. I then suggest that if we consider the manner in which the natural numbers figure in our computational practices – e.g. in characterizing how we compute sums and products or determine prime factorizations – it follows that our application of computational terms such as “computable function” and “tractable computational problem” indirectly uniquely determines their structure.

Consider the sequence of natural numbers  $0, 1, 2, \dots$ . No mathematical structure is more familiar or is more immediately part of our everyday practices of counting, measuring and computing. It is hence surprising that there are two well known arguments to the effect that the way in which we use the term “natural number” does not single out a unique mathematical structure. The first, and more famous, originates with Benacerraf [1] who argues that under the assumption that all mathematical objects are reducible to set theoretic ones, there are no *mathematical* reasons to identify the natural numbers with the Von Neumann ordinals over the Zermelo ordinals, since all arithmetic facts are preserved under both identifications.

Benacerraf’s argument is generally taken to demonstrate that we should seek to identify mathematical objects not with particular set theoretic ones but rather with set theoretic structures, identified up to isomorphism. On this view, “natural number” does not refer determinately to any particular class of objects but rather to the positions in an arbitrary  $\omega$ -sequence. Most theorists agree that this sort of indeterminacy in our concept of natural number is benign – since mathematicians only identify structures up to isomorphism, what sense can there be in pressing more finely grained questions of identity? The second argument by which the determinacy of the natural numbers has been challenged, however, threatens even their identification with a class of mutually isomorphic structures.

Informally stated, this argument runs as follows. One might think that the sequence of natural numbers has a unique structure: it contains the numbers associated with each position in the sequence  $0, 1, 2, \dots$  where the “...” abbreviates all elements in such a sequence with finitely many predecessors. Mathematicians give this structure a name,  $\mathbb{N}$ , and go on to treat this symbol as if it denoted a determinate structure. But what, one might ask, does it mean for a number to have *finitely* many predecessors? Note that the notion of finitude is generally defined in terms of the natural numbers: a set or sequence of items is finite just in case it can be placed in one-one correspondence with an initial segment of the natural numbers. Prima facie, it hence appears circular to *define* “natural number” in term of finitude. Similar problems beset the suggestion that the natural numbers correspond to the sequence obtained from 0 by iterating the successor operation arbitrarily many times: our concrete experiences with natural numbers do not extend arbitrarily far along the sequence  $0, 0', 0'', \dots$  and one is hence left to wonder whether the envisioned procedure is sufficiently well-defined to ensure that it ‘cuts off’ at a unique point, having generated all of the natural numbers but no extra elements.

These informal worries can be spun into a model theoretic demonstration of the first-order undefinability of “natural number” as follows. The obvious retreat from the claim that our informal characterization of the natural numbers

does determine a unique structure is to trade it in for an axiomatic one. The most widely accepted and readily defensible choice for such a theory is the axiomatization known as Peano arithmetic or  $PA$ .  $PA$  has as signature  $\mathcal{L}_A = \{0, 1, +, \times, <\}$  and its axioms consist of a catalog of the basic algebraic properties of addition and multiplication with an axiom schema stating the validity of mathematical induction for all  $\mathcal{L}_A$ -definable properties.

The *standard model* of  $PA$  is the structure  $N = \langle \mathbb{N}, 0, 1, +, \times, < \rangle$ . It follows trivially that  $N \models PA$ , but it may also be shown that there are  $M \models PA$  which are not isomorphic to  $N$ . This fact is generally presented as an elementary consequence of the first-order compactness theorem by considering the theory  $T = PA + \bigcup_{n \in \mathbb{N}} n < \mathbf{a}$ .  $T$  is finitely consistent as  $\mathbf{a}$  can always be interpreted as a natural number larger than any required to exist by a finite set of sentences  $S \subset T$ .  $T$  hence a model  $M$  which contains an element  $a$  realizing  $\mathbf{a}$  which has infinitely many predecessors. Such  $M$  are called *non-standard models of arithmetic* and elements like  $a$  are called *non-standard numbers*. It can be shown that all non-standard models have order-type  $\omega \oplus (-\omega \oplus \omega) \otimes \eta$  (where  $\eta$  is the order type of the rationals). From this it follows that all non-standard models are such that  $M \not\cong N$  and with a little more work it can be shown that there are in fact there are uncountably many pairwise non-isomorphic non-standard models of  $PA$ .

Intuitively one wishes to say that non-standard models contain ‘extra’ natural numbers. As such, we would like to be able to rule out such structures as potentially determinative of the extension of the term “natural number.” But this response is blocked if  $PA$  turns out to be *constitutive* of what we know about the natural numbers. For in this case, the domain of any  $M$  satisfying  $PA$  should be as good a candidate for fixing the extension of “natural number” as any other. Despite the indeterminacy that such a view appears to spell for our conception of natural number, there are several reasons why one might adopt it:

- 1)  $PA$  replaces our informal and potentially vague notion of natural number with a precise formal characterization of their structural properties with a theory whose theorems (with rare and controversial exceptions) coincide with both what our mathematical and everyday practices represent as being true of the natural numbers.
- 2)  $PA$  is a recursively axiomatizable theory. By Gödel’s first incompleteness theorem, consistent first-order theories such as  $PA \cup Th(N) \cap \Pi_1$  which are even slightly stronger than  $PA$  are not even recursively enumerable. And, as Putnam has put it, ‘...it is hard to envisage coming to have a non-recursive set of axioms in the literature or in our heads.’([13], p. 425)

- 3) Attempts to secure a categorical description of the natural numbers by adopting a stronger first-order axiomatic characterization don't work. For instance, there are still non-standard models of the theory  $Th(N)$  (i.e. the set of all  $\mathcal{L}_A$  sentences true of  $N$ ). Moreover, adopting a higher-order characterization begs questions about our ability to characterize mathematical structures such as the powerset of the natural numbers which are potentially more severe than those encountered in attempting to describe the natural numbers themselves.

Although I will argue in this paper that the foregoing argument should *not* be taken to demonstrate that there is any indeterminacy in our conception of natural number, it also gives pause that its rhetorical structure closely mirrors the argument by which Skolem claimed to exhibit his acclaimed 'relativity of set theoretic notions.' According to Skolem, the first-order theory  $ZF$  should be taken as a complete and adequate characterization codification of our practices and intentions concerning the use of the terms "set" and "membership." One component of these practices is our belief that the set of real numbers is uncountable – a fact represented in  $ZF$  by a theorem stating that the set of elements satisfying the  $\mathcal{L}_{ST}$  definition of real number,  $Real(x)$ , cannot be put into one-one correspondence with a countable set. Although we thus intend the extension of our term "real number" to be uncountable, by the downward Löwenheim-Skolem and Mostowski collapsing theorems  $ZF$  has countable transitive models  $\mathfrak{B}$  and hence ones in which the extension of  $Real(x)$  is countable. But according to Skolem (e.g. [16], [18]) and his followers, by virtue of satisfying  $ZF$ , such  $\mathfrak{B}$  should be as good candidates for determining the extension of our set theoretic terms as any other model. Skolem concludes on this basis that the extension of "real number" (and by a similar argument, may other concepts which lack 'absolute'  $\mathcal{L}_{ST}$  definitions) is indeterminate.

Skolem's argument has been quite influential. Among its most avid supporters is Putnam who not only accepts Skolem's conclusions about the 'relativity' of notions like "real number" and "uncountable" but seeks to extend their reach. In [13], he argues that Skolem's argument also demonstrates the indeterminacy of the *truth value* of statements  $\theta$  which are proof theoretically independent of  $ZF$  (and for which there hence exist  $\mathfrak{A}, \mathfrak{B} \models ZF$  such that  $\mathfrak{A} \models \theta$  and  $\mathfrak{B} \not\models \theta$ ). Putnam argues that if  $ZF$  is constitutive of what we know about sets, then Skolem's "relativity of set theoretic notions" extends to a *relativity of the truth of " $V = L$ "* (and, by similar arguments, of the axioms of choice and the continuum hypothesis as well.) ([13], p. 427, italics in the original) Putnam's brand of set theoretic skepticism has attracted its share of supporters: Wright [23], Field [9], Dummett [4] and Feferman [6] all argue that the existence of certain sorts of 'non-standard' models of  $ZF$  implies the indeterminacy

of particular set theoretic concepts or statements.

Given the scope of Putnam's indeterminacy argument for set theory, however, it is understandable why one might find the consequences of the number theoretic version unattractive. First, by Gödel's second incompleteness and completeness theorems, there exist models  $M \models PA + \neg Con(PA)$ . Skepticism about the structure of the natural numbers hence entails that we must regard the truth value of  $Con(PA)$  as indeterminate despite our strong intuitive tendency to think both that  $PA$  is consistent and that  $Con(PA)$  adequately expresses this fact. Second, note that the  $\mathcal{L}_A$  terms  $Sent_{\mathcal{L}_A}(x)$  and  $Proof_{PA}(x, y)$  which respectively are intended to express that " $x$  is the gödel number of an  $\mathcal{L}_A$  sentence" and " $x$  encodes a proof of the  $\mathcal{L}_A$  sentence with gödel number  $y$ " are satisfied by arbitrarily large natural numbers. By what is known as the overspill principle, however, it can hence be shown that since these predicates apply to arbitrarily large natural numbers, they will apply to non-standard numbers in any non-standard  $M \models PA$ . The number theoretic skeptic must hence maintain that the reference of the informal terms " $\mathcal{L}_A$  sentence" and "proof from the axioms of  $PA$ " is indeterminate.

But does our distaste for such consequences alone constitute a reason to reject the number theoretic version of the indeterminacy argument? Skolem, who in [17] was the first to explicitly construct non-standard models of  $PA$ , evidently thought that it does not:

As I [have] emphasized [the argument based on the Downward Löwenheim-Skolem theorem] leads to a relativization of set theoretic notions. [I]f one desires to develop arithmetic as a part of set theory, a definition of the natural number series is needed and can be set up as for example done by Zermelo. However, this definition cannot be conceived as having an absolute meaning, because the notion set and particularly the notion subset in the case of infinite sets can only be asserted to exist in a relative sense. It was then to be expected that if we try to characterize the number series by axioms, for example by Peano's, ... we should not obtain a complete characterization... This fact can be expressed by saying that besides the usual number series other models exist of the number theory given by Peano's axioms or any similar axiom system.([17], p. 1)

Putnam has recently reached a similar conclusion: 'If there are *possible divergent extensions of our practice*, then there are *possible divergent interpretations of even the natural number sequence* – our practice, or our mental representations, etc., do not single out a unique 'standard model' of the natural number sequence.' ([12], p. 67, italics in the original)

But the consensus which has emerged from a string recent of proposals by Shapiro[15], McGee [11], Lavine [10], and Field [8],[9] is that the number theoretic indeterminacy argument can and should be resisted, whatever the fate of its set theoretic counterpart. Let me repeat that I am in general agreement

with this conclusion. As I'll argue in the remainder of this paper, however, I think that the arguments offered by these theorists are either non-demonstrative or question begging and that the correct manner in which to demonstrate the determinacy of the natural numbers turns on concerns quite distant from those they consider.

To understand where earlier arguments for the determinacy of the natural numbers go astray, we must consider somewhat further the scope of Putnam's argument in [13]. Putnam argues that his case against the determinacy of set theoretic notions is effective not just in the case where we characterize our knowledge of the set theoretic universe via  $ZF$ , but also if we attempt to strengthen this characterization with additional mathematical, linguistic or empirical constraints on the interpretation of our set theoretic language. He argues, for instance, that we cannot locate models of our mathematical structure in the physical world and then expect to fix the references of terms like "set" and "member" by reference to them because even if we were able to non-circularly state a referential theory by which we could discern such structures, the statement of the theory which describes how we can do so would itself be susceptible to non-standard interpretation. Putnam is thus standardly interpreted as maintaining that *any attempt* to go outside of our practices in order to constrain how our mathematical terms refer can be subsumed under a broader characterization of our practices, which can itself be 'first-orderized' and demonstrated to have non-standard models.

The arguments of Shapiro, McGee and Lavine, and Field, however, all represent attempts to secure the determinacy of the natural numbers by going outside of our immediate practices. They are hence potentially susceptible to Putnam's 'more theory' reply, although, as I'll now briefly describe, each attempts to circumvent it a different manner.

**Shapiro** Shapiro argues that we should adopt the theory  $PA2$  – i.e.  $PA$  supplemented with a second order induction axiom — as our formal characterization of the natural numbers. By Dedekind's theorem,  $PA2$  is known to be categorical, and in fact all of its models can be shown to be isomorphic to whatever we take to be the 'ground model'  $N$ . Shapiro argues that we are justified in moving to a second-order characterization of mathematical structures because mathematicians treat concepts like well-order and minimal closure, and cardinality (all of which possess absolute second-order definitions but lack absolute first-order one) as if they were determinately defined.

**McGee and Lavine** McGee and Lavine believe  $PA$  to be deficient as a characterization of what we know about the natural numbers because its induction scheme is limited to first-order formulas. They suggest that since

our informal understanding of the natural numbers affirms the validity of induction for arbitrary well-defined properties of the natural numbers that we should adopt a theory called *full schematic arithmetic* [ $PA^s$ ] which includes an induction axiom which states the validity of induction for properties defined over arbitrary arithmetic languages  $\mathcal{L}_A^+ \supseteq \mathcal{L}_A$ . They then argue that something like a categoricity theorem can be proven for this theory in  $PA^s$  itself.

**Field** Field argues that the constraints governing our application of terms like “set,” “membership,” “function,” etc. to collections of physical objects make their extension in the physical domain determinate. On this basis, and with the addition of some assumption about the structure of physical space or time, he argues that we are able to define a *mathematical* finiteness quantifier  $\exists^{<\infty}$ . On this basis he argues that we can categorically describe the natural numbers as any structure satisfying the theory  $PA + \forall x \exists^{<\infty} y (y < x)$ .

I won't take time to argue singularly against the viability of these proposals. Note, however, that they all share the following feature: in attempting to state a constraint by which to secure the determinacy of the natural numbers, they all look beyond what can be reasonably construed as a description of our current number theoretic practices. Shapiro, for instance, asks that we consider the way mathematicians treat concepts mathematical concepts from algebra, analysis and set theory as indirect evidence about the machinery we are entitled to use in describing the natural numbers axiomatically. In so doing, however, he assumes that the extension of these terms is determinate relative to our mathematical practice at large. Similarly, McGee and Lavine assume that our dispositions for extending the principle of mathematical induction to predicates defined in terms of novel number theoretic expressions is relevant to determining what our number theoretic terms mean *now*. But if there is nothing about our contemporary use of “natural number” which singles out a unique reference for this term, why ought we to think that our usage will evolve in a way that conforms to only one of the interpretations with which it is currently compatible? These constraints succeed in determining the structure of the natural numbers only modulo the determinacy of the language of the constraints by which they propose to supplement  $PA$ . And it should be clear to readers of [13], that Putnam's model theoretic argument may be iterated against the individual proposals to construct models of the supplemented theories in which the extension of the term “natural number” is non-standard.

If we have any hope of securing the determinacy of the natural numbers, it thus appears that the prospects are dim for finding appropriate constraints on how we use number theoretic terms *outside* of our number theoretic practices.

There is, however, an as yet unexplored source of constraints *within* our practices, namely the manner in which we use the natural numbers in computational tasks such as calculating sums and products and finding prime factorizations. Not only do we possess a vast array of practical computing methods for solving such problems, but we also have detailed data – both positive and negative – on our ability to solve them efficiently. For instance, we find it relatively easy to compute the sum of two natural numbers, but considerably harder to determine the prime factorization of an arbitrary number. Moreover, there are certain problems which we do not know how to solve either in the general case – e.g. whether a given first-order sentence is a logical truth or whether a given Diophantine equation has roots in the integers – or by an efficient method – e.g. determining whether a boolean sentence is satisfiable or whether a set of linear inequalities has a solution in the integers.

Although I won't argue this detail, it should also be evident that  $PA$  provides no standardized way to characterize computational of the sort we routinely employ. For instance, even though if  $n$  is prime,  $PA$  will prove the  $\mathcal{L}_A$  sentence expressing this fact, the proof will bear little resemblance to (and will be likely to contain many more steps than) standard computational methods for deciding  $n$ 's primality. This suggests that in order to more aptly characterize the computational component of our number theoretic practices, we need to look to a different source. The obvious candidates here are the modern theories of computability (cf., e.g., [14]) and complexity (cf., e.g., [3]). These theories respectively suggest that there are two fundamental distinctions which describe and delimit our computational practices: that between a “computable function” and a “non-computable function” and that between a “tractable computational problem” and an “intractable computational problem.” As I will now argue, any characterization of our mathematical practices at large which seeks to avoid trivializing our computational practices must regard these terms as demarcating determinate distinctions in the structure of mathematical problems about the natural numbers and our abilities to solve them. And from the determinacy of these distinctions, I will argue the uniqueness of the structure of the natural numbers follows.

My case for the determinacy of computational notions is based on the fact that any reasonable analysis of the notion of computability as it appears in our practices must have an *intentional* component. In particular, the original proposals on which our current notion of “computable function” is based are all essentially conceptual analyses of the notion of a function calculable by a computing agent *such as ourselves*. This is perhaps most evident in the work of Turing who proposes that a function from natural numbers to natural numbers is computable just in case it can be computed by an abstract mechanical device known as a Turing machine. As Turing [21] makes clear, however, the individual

components of Turing machines are all motivated by a conceptual examination of what it means for a ‘computer’ (Turing’s term for a human computing agent) to be able to mechanistically calculate the value of a function. For instance, the fact that the device may have only finitely many internal states is justified by the fact that a human agent can be in only one of finitely many ‘states of mind’ at a given stage in a calculation; the fact that the machine’s tape is divided into discrete cells is justified by the fact that such an agent could not write or discern symbols which are arbitrarily close together; etc. As such, the formal analysis of computability that emerges from Turing’s work is intended to be answerable in a primary sense to our intuitions about what functions we ourselves are capable of computing when working in a purely mechanistic mode.

Church’s thesis is the claim that Turing’s analysis of computable function is extensionally correct – i.e. that the class of intuitively computable functions coincides with the class of those computable by Turing machines. Subsequent work has shown that this class also coincides with the classes of functions computable relative to, among many others, the analyses of Post, Church, Kleene and Gödel (all of whom presented independent but similarly motivated analyses of “computable function”) and which we now refer to as the *(partial) recursive functions*. Church’s thesis is now generally accepted and it is common to assume without disclaimer that the terms “computable” and “recursive” extensionally coincide.

But note that in taking this step, we do not need to assume that the *meaning* of “computable function” is given by a specific formal analysis. In particular, if a given analysis of computation had as a consequence that a mathematically well-defined function  $g(n)$  was computable but neither did that the analysis itself provided a concrete procedure for computing  $g(n)$  nor was  $g(n)$  known to be intuitively computable by any other means, our tendency would be to reject the analysis rather than abandoning our intuitions about  $g(n)$ ’s non-computability. Such intuitions can be bolstered by noting that it is a direct (if non-obvious) consequence of any formalization of our intuitions about computability that there exist mathematically well-defined but non-computable functions. An example of such a function is the ‘anti-diagonal’ function  $u(n)$  with respect to a given enumeration of Turing machines  $T_0, T_1, \dots$ :

$$u(n) = \begin{cases} 1 & \text{if Turing machine } T_n \text{ on input } n \text{ does not halt} \\ T_n(n) + 1 & \text{otherwise} \end{cases}$$

The assumption that  $u(n)$  is computable (and hence via Church’s Thesis is computed by a Turing machine) may be shown to lead immediately to a contradiction. Hence if we could show that *if* the intuitively non-computable function  $g(x)$  were computable, *then*  $u(n)$  would also be computable, this would provide a principled basis on which to maintain that  $g(x)$  is indeed non-computable.

On the basis of these considerations, I think a convincing case can be made for the thesis that our intuitions about which functions are computable and which are not determines the extension of the terms “computable function” and “non-computable function” for a fairly broad class of cases. The intuitive reason why this is the case is that the computability of a function can be ‘read off’ from an effective procedure for computing it. Since procedures are the sorts of things we can mentally survey, the fact that a function determined by a procedure is computable is something we can ascertain by examining the procedure itself and convincing ourselves that it specifies an intuitively effective method for calculating the value of the function on all inputs.

Somewhat more formally, there is a subclass of the total recursive functions known as the *provably total functions* (with respect to  $PA$ ). This class contains all primitive recursive functions and in particular it contains all familiar functions from elementary number theory (e.g. addition, subtraction, multiplication, division, exponentiation, etc.). Functions in this class not only possess intuitively effective and provably correct algorithms for computing their values, but it can also be formally proven that these algorithms are defined on all inputs and hence determine total functions. The application of the term “computable function” to members of this class should therefore be regarded as determinate. Conversely, if it can be shown that the assumption that a particular function  $g(n)$  is computable leads (either directly or indirectly) to a contradiction, we can regard  $g(n)$  as determinately falling under the term “non-computable function.”

If we agree that our computational practices fix the extension of the term “computable function” and “non-computable function” to the extent just specified, it turns out to be a fairly simple matter to distinguish between standard and non-standard models of  $PA$ . Let  $\mathcal{L}$  be a first-order language and  $M$  an  $\mathcal{L}$ -structure. We say that  $M$  is a *recursively presentable model* if there exists a model  $M'$  such that  $M \simeq M'$  such that the atomic diagram of  $M'$  (i.e. the set of atomic sentences and negated atomic sentences in the language  $\mathcal{L}$  adjoined with constants for each member of  $dom(M)$ ) is a recursive set (i.e. has a total recursive characteristic function)). Clearly the standard model  $N$  of  $PA$  is recursively presentable because its own atomic diagram is recursive (and in fact computable) since  $plus(n, m) (= \{ \langle n, m, p \rangle \mid n + m = p \})$  and  $times(n, m) (= \{ \langle n, m, p \rangle \mid n \times m = p \})$  are computable functions and the equality and less than relations are both computable. A result known as Tennenbaum’s theorem [22] states that  $N$  is the only recursively presentable model of  $PA$  and that any non-standard  $M \models PA$  will be such that in any  $M' \simeq M$ , the extensions of neither the addition function  $+_{M'}$  nor the multiplication function  $\times_{M'}$  will be computable. Moreover, in every non-standard model  $M$  there will be explicitly definable non-computable functions which can be ‘computed’ under the assumption that we can compute the values of  $+_M$  or  $\times_M$  for all

members of  $dom(M)$ .

Tennenbaum's theorem shows that there is a discernible discrepancy between the case in which the extension of our number theoretic terms is fixed relative to the standard model  $N$ , and that in which it is fixed relative to a non-standard model. The mathematical functions  $plus(n, m)$  and  $times(n, m)$  are both paradigmatic examples of provably total functions – i.e. there are provably correct algorithms  $\pi(n, m)$  and  $\sigma(n, m)$  (e.g. the ‘grade school’ addition and multiplication algorithms) which can be shown to determine the values of  $plus(n, m)$  and  $times(n, m)$  for arbitrary  $n, m$ . Tennenbaum's theorem tells us, however, that the extensions  $+_M$  and  $\times_M$  assigned to  $+$  and  $\times$  in any non-standard model must correspond to non-computable functions. Hence no such model can succeed in accurately reflecting our computational practices because it will misrepresent the computability of  $plus(n, m)$  and  $times(n, m)$  and in so doing violate the constraints on the interpretation of our computational vocabulary proposed above.

It would be premature, however, to conclude that this argument constitutes an unequivocal refutation of number theoretic skepticism. Note in particular that the argument turns on the fact that the functions denoted by  $+_M$  and  $\times_M$  are determinately non-computable. My case for the determinacy of “non-computable function,” however, relied on the existence of particular non-computable functions defined relative to an enumeration of all Turing machines. Note, however, that the formal definition of a Turing machine and a Turing machine computation both recourse to the notion of finitude: a Turing machine by definition has at most *finitely* many states and the computation of machine  $T_n$  on input an  $m$  returns only if the machine halts after *finitely* many steps. And as we saw above, the concept of finitude is indeterfinable with that of natural number. Hence both the envisioned enumeration of Turing machines, and the particular candidate non-computable function defined via diagonalization on this enumeration will inherit whatever indeterminacy applies to our term “finite.” It may hence be claimed by a determined skeptic that the functions denoted by  $+_M$  and  $\times_M$  in fact are “computable” when this notion is defined in first-order terms and relativized to a non-standard  $M \models PA$ . In particular, it falls out from the proof of Tennenbaum's theorem that both  $+_M$  and  $\times_M$  are “computable” by a machine that is able to carry out the Euclidean division algorithm on non-standard numbers.

There are, however, reasons to doubt the cogency of this reply. In particular, even though  $+_M$  and  $\times_M$  may be claimed to fall under one of the formal definition of computability suggested by Church's thesis, this fact alone does not provide us with a description of a procedure which *we* can use to compute their values for arbitrary  $m, n \in M$ . As I suggested above, if there is a discrepancy between our intuitions about which functions are computable and those

predicted by a particular formal analysis to be computable, our tendency is to think that it is the analysis which needs revision rather than our concept of computability. Our practices can hence be taken as providing both a positive and a negative criterion for determining whether a given function is computable. There is hence considerably less room for a skeptic to claim that there is a ‘relativity of computation theoretic notions’ analogous to Skolem’s ‘relativity of set theoretic notion’ simply due to the existence of models in which the first order characterization of computational terms receive non-standard extensions.

Note, moreover, that in the case under consideration the problem is not that Church’s thesis itself has lead us astray in requiring us to extend the term “computable” to  $+_M$  and  $\times_M$ , but rather that it does so only in conjunction with the dual imperatives that we must both assimilate computational notions to mathematical ones and also treat mathematical notions as determinate only if they possess absolute first-order definitions. But in the case at hand there is room for debate as to whether the indeterminacy in class of computable functions which would result if we were to take these step needs to be taken seriously. One might, for instance, argue that Church’s thesis can only be regarded as a meaningful hypothesis about our informal notion of computability in conjunction with certain methodological principles. For instance, note that our intuitive notion of computable function (*qua* function determined by a rule or procedure) is supposed to contrast with that of an extensional specification of a mapping as an infinite set of pairings. It hence follows that any analysis of this concept under which functions determined by devices with infinitely many states (such as non-standard Turing machines) may potential fall already deviates from what we informally mean by “computable function.” More generally, it may be argued that Church’s thesis can only be interpreted as a meaningful hypothesis about the boundaries of what we mean by “computable function” if the language in which we formally explicate various components (e.g. ‘finite specifiability’) of our informal conception is itself held to refer determinately.

Even though I suspect there is some merit in pursuing the reply just sketched, I won’t pursue this topic further here because I believe there to be a much more powerful means of answering the skeptical challenge as to the determinacy of computational notions. As it turns out, Tennenbaum’s theorem is merely the tip of the iceberg with respect to illuminating the anomalies which would arise were the reference of computational terms to be tied down no more firmly than the class of extensions which their first-order definitions allow. In particular, central concepts from complexity theory – notably that of a *tractable computational problem* – can also be shown to possess natural  $\mathcal{L}_A$  definitions. If the meanings of complexity theoretic concepts were only fixed relative to their these definitions, however, they too would inherit the indeterminacy of finitude. Complexity theoretic terms, however, are intended to express real world limita-

tions on our abilities which are yet more directly reflective of our what we can computationally achieve in practices than those studied in computability theory (in the sense of [14]). As a consequence, the plausibility of iterating the skeptical argument that our practices and intentions are insufficient to determine the reference of certain complexity theoretic terms becomes strained to the point of collapse.

By way of example, consider our informal notion of a tractable computational problem – i.e. one for which it is the case that if we can represent an instance of the problem, then there exists an algorithm by which we can determine its solution in a ‘feasible’ number of steps using a ‘feasible’ amount of auxiliary space. By this criterion, the problem of summing two integers is clearly tractable for if we can write down binary representations of integers  $n$  and  $m$  then we know a method (e.g. the ‘grade school addition algorithm’ alluded to above) by which to compute their sum in no more than, say,  $10 \cdot \log_2(\max\{n, m\}) + 10$  steps. It is widely believed, however, that the problem of determining the prime factorization of an arbitrary natural number is not tractable – the most efficient known procedures require a number of steps which is exponentially proportional to the length of their input. As a consequence, there are many specific examples of natural numbers less than 300 digits in length whose factorizations we currently do not know.

The notion of tractability is analyzed in contemporary complexity theory via the notion of a *complexity class* – i.e. a class of problems each of whose members is such that its instances can be solved using similar computational resource bounds. It is believed on the basis of several converging analyses that the complexity class  $\mathbf{P}$  of *polynomial time decidable problems* (i.e. problems whose instances can be solved in time bounded by  $c \cdot n^k$  where  $n$  represents the length of a problem instance and  $c, k$  are fixed natural numbers) corresponds to the class of problems we find tractable in practice. On the other hand, the problem of formulating and defending criteria relative to which a problem is determinately *intractable* – i.e. not solvable by an efficient algorithm – however, has proven considerably more challenging. In particular, we do not currently know whether the class  $\mathbf{P}$  is distinct from the class  $\mathbf{NP}$  consisting of problems solvable in polynomial time by a non-deterministic Turing machine.

There are, however, many examples of problems which can trivially be shown to be computable in the abstract sense, but are yet considered to be much more difficult to solve than those in  $\mathbf{NP}$ . For instance, call a first-order expression  $\psi = \forall x_1 \dots x_l \exists y_1 \dots y_n \varphi(x_1, \dots, x_l, y_1, \dots, y_n)$  such that  $\varphi$  is quantifier free and contains no function symbols a Schönfinkel-Bernays [SB]-sentence. The computational problem known as SBSAT is as follows: given an arbitrary SB sentence, is it satisfiable? It is known that if an SB sentence is satisfiable, it possesses a finite model of size no greater than  $n = l + m$ . From

this it follows that SBSAT is in the complexity class  $\mathbf{NEXP}$  consisting of problems solvable in time bounded by  $c \cdot k^n$  steps by a non-deterministic Turing machine. In addition SBSAT, can be shown to be *complete* for this class – i.e. if it were shown that SBSAT can be solved by a efficient algorithm, then it would follow that all other problems in  $\mathbf{NEXP}$  (including the factorization problem discussed above and literally thousands of other well-studied problems for which we currently lack efficient algorithms) also have tractable solutions. It follows from a result known as the Time Hierarchy Theorem, however, that  $\mathbf{NEXP}$  properly contains  $\mathbf{P}$ . Hence if  $\mathbf{P}$  accurately represents the class of informally intractable problems, this result suggests that SBSAT – as opposed to other well known computational problems such as the traveling salesman problem or the satisfiability problem for boolean formulas both of which are in  $\mathbf{NP}$  – can be taken as an example of a genuinely intractable problem.

If complexity theoretic notions are analyzed arithmetically and then subjected to Skolemite relativization, however, all distinctions between the relative tractability of computational problems collapses. Buss [2], for instance, showed that for any set of natural numbers  $A$ , the problem of deciding  $n \in A$  for arbitrary  $n$  is in  $\mathbf{P}$  just in case  $A$  is provably  $\Delta_1^b$  definable relative to  $N$  (i.e. such that there are formulas  $\theta_1$  and  $\theta_2$  containing only sharply bounded quantifiers such that  $A = \{n \mid N \models \exists x \theta_1(x, n)\} = \{n \mid N \models \forall x \theta_2(x, n)\}$ ). Moreover, the left-to-right direction of this results establishes that whenever a set  $A$  is definable in this manner, there exists an explicit polynomial time algorithm for deciding membership in  $A$ . By methods similar to those used in the proof of Tennenbaum’s theorem, however, it can be shown that in every nonstandard model all *recursive* sets  $S$  are  $\Delta_1^b$  representable. It hence follows that relative to all non-standard  $M \models PA$ , computational problems of arbitrary computational complexity satisfy the formal arithmetic definition of polynomial time decidability and in fact possess tractable “algorithms” by which their instances may be “solved” relative to  $M$ . In particular, relative to  $M$  it may be shown there are efficient procedures for factoring arbitrarily large natural numbers and for determining the satisfiability of arbitrary SB-sentences.

I contend that rather than demonstrating that there is a ‘relativity of complexity theoretic notions’ this situation constitutes a reductio of the number theoretic skepticism. For it is simply absurd to think of there being faithful models of our complete mathematical practices in which SBSAT turns out to be a tractable problem – since there are currently SB-sentences which we can write down but of whose satisfiability we are ignorant, any model of our practices which makes out the problem of determining the satisfiability of such sentences to be a tractable problem fails to be an accurate representation of how we transact numerical computation. As Buss’s arithmetic analysis of complexity theoretic demonstrates,  $N$  is the only model which fails to collapse distinctions

between computational problems whose significance is vividly borne out by our computational practices. Thus if, as the model theoretic skeptic suggests we must, we look at our mathematical practices as the true determinants of the structure of the natural numbers, we are hence ultimately led to the conclusion that there is but a single candidate for their structure.

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