ON ONTOLOGY AND REALISM IN MATHEMATICS

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Outline

The paper is concerned with the way in which "ontology" and "realism" are to be interpreted and applied so as to give us a deeper philosophical understanding of mathematical theories and practice. Rather than argue for or against some particular realistic position, I shall be concerned with possible coherent positions, their strengths and weaknesses. I shall also discuss related but different aspects of these problems. The terms in the title are the common thread that connects the various sections.

The discussed topics range widely. Certain themes repeat in different sections, yet, for most part the sections (and sometimes the subsections) can be read separately. Section 1 however states the basic position that informs the whole paper and, as such, should be read (it is not too long).

The required technical knowledge varies, depending on the matter discussed. Aiming at a broader audience I have tried to keep the technical requirements at a minimum, or to supply a short overview. Material, which is elementary for some readers, may be far from elementary for others (my apologies to both). I also tried to supply information that may be of interest to everyone interested in these subjects.

Section 2 illustrates the application of the proposed methodology by considering various positions, some of which represent main schools in the philosophy of mathematics. There are some technical items, but I hope that the gist of the discussion should be obvious from the more elementary examples. Some material from Section 2 is used in Section 6.

The more philosophical parts are the discussion of Quine's bound variable criterion (Section 3), the "ontology-ideology" distinction (Section 4), and the short discussion of some attempts to ground mathematics in "concrete" entities (Section 5).

Section 6, "Realism in mathematics," which is about half of the paper, is divided into several subsections that treat a variety of subjects, from Hilbert's program and the import of Gödel's incompleteness results to the significance of computer-aided proofs. Section 6.4 discusses certain connections with epistemic issues, which arise through the notion of a "natural mathematical question." The relation between epistemic and ontological issues is rather involved, and I cannot do it full justice here; the paper is intended to have an epistemic twin under the name "How do we know mathematical truths?"

Section 7 contains some final observations that bring together some of the themes addressed in various parts of the work.

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§1. Things versus facts.[1] Ontology is usually conceived by philosophers as a question about existing things, the "stuff" the world is made of. Realism is often interpreted as the view that such and such objects exist; and the more sorts of objects one thinks there are—the more realist one is. In the extreme case one thinks that there is a domain populated by abstract, human-independent, eternally existing objects; thus one gets to be a Platonist. This way of framing the questions derives from powerful traditional metaphors. I think it is potentially misleading and bound to land us in endless and fruitless debates. The metaphors themselves, like any metaphor, are neither true nor false. A metaphor can be good: useful, illuminating, fruitful, or bad: useless, misleading, and barren. And this depends on the context in which the metaphor is used, and on how it is implicitly or explicitly applied. A fruitful metaphor can become later a dead weight, a guide for tunneled vision.

I shall not advocate this or that position in the foundations of mathematics; what I suggest is a *way of characterizing various positions*. Neither shall I argue head on against the traditional object-oriented conception. In fact, concerns with objects and with the semantic notion of denotation are often an important motive for certain foundational positions; such issues will be addressed in Sections 4 and 5. For the moment let me begin by suggesting an alternative way. Instead of starting with the basic apparently understood notion of *existing object*, let us start with another basic and apparently understood notion: that of a *factually meaningful question*; that is, a question that has a determinate answer, which is independent of our knowing it or our abilities to find it. If the question has a yes/no answer, then its being factual means that some statement is objectively true or false. I think that the wider notion of a *factual question*, for example, asking for some numeric value, "what is the number of . . . ?," is more appropriate in this analysis. But for simplicity, the examples will be mostly of yes/no questions. At this point we need not go into the issue of mathematical truths that are unknowable to humans due to the limitation of human capacities—something that has gained plausibility, in view of the incompleteness results, more recent independence results, and the nonexistence of "short enough" proofs (lower bounds on proof complexity). It is sufficient that the questions (or the statements) are considered factually meaningful by themselves, without this being conditioned on what we can do to settle them.

Note that my proposal concerns both *ontology* and *realism*—in both I propose a shift of focus from "existing objects" to "objective facts" or "objective truth." The fact-oriented approach in ontology implies the same approach in characterizing realism, but not vice versa.[2] The characterization of "ontology" will be discussed in Section 3. For the moment, that is, in the next section, I shall apply the fact-oriented approach to characterize

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[1] Wittgenstein's dictum "The world is the totality of facts not of things" could serve as the title of the section. But Wittgenstein's philosophy of mathematics (in the *Tractatus* as well as in the later writings), which leaves no place for realism, is a subject by itself and any discussion of it would be distracting. I do believe, however, that I share with Wittgenstein the basic intuition about things versus facts, as it is expressed in the opening statement of the *Tractatus*.

[2] Thus, Shapiro (1997, p. 37) proposes a distinction between *realism with respect to existence* and *realism with respect to truth*. The second is a kin to the approach to realism proposed in this paper. Following Dummett, he attributes to Kreisel the point about the importance of objective truth rather than the existence of objects. But this attribution seems problematic; see Footnote 4.

various stronger and weaker realistic positions in the foundations of mathematics. Such an approach to realism has been notably associated with Dummett.[3,4]

Of course, both notions, that of an existing object and that of a factually meaningful question (or a factual statement), can be sized upon and analyzed with great philosophical zeal. Overcoming the temptation, let us take the notion of a factual meaningful question for granted and see how it can be profitably used in comparing various foundational positions in the philosophy of mathematics. Its use in these contexts will also cast light on the notion itself, which the direct approach of putting it under the philosophical microscope is unlikely to do. So here is a list of questions for testing "degrees of realism." If you think the question has an objective true answer, you are a realist with respect to this type of question.

§2. From weaker to stronger realistic positions. Consider the following questions:

(1) What is the largest prime number dividing $2^{2^{43112609}} - 1$?

(2) Are there solutions to the Diophantine equation $p(x_1, x_2, \ldots, x_k) = 0$? (here p is some polynomial with integer coefficients and $x_1, \ldots, x_k$ range over the natural numbers).

(3) Are there infinitely many twin primes? (where twin primes are primes of the form p, $p+2$)

(4) Is ϕ true? (where ϕ is any given sentence in first-order arithmetic).

(5) Are there nonconstructible real numbers?

(6) Is there a nondenumerable subset of real numbers of cardinality strictly less than the cardinality of the whole set? (i.e., is CH, the Continuum Hypothesis, false?)

(7) Are there measurable cardinals?

(1) is used to mark *ultrafinitism*, also known as *strict finitism*—a position according to which (1) has no factual meaning. Except for an extremely small number of ultrafinitists,

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[3] Dummett's paper 'Realism', where this approach is proposed, was first published in 1978 (Dummett, 1978, pp. 145–165); but it was read in a 1963 philosophical meeting. The view is somehow indicated also in Dummett's earlier published work from 1959 and 1969. My own proposal regarding "ontology" (Gaifman, 1975, 1976) is independent of Dummett's work. It was my first foray as a mathematician into general philosophy and I was not aware of Dummett's positions. My papers actually appeared before (Dummett 1978). My proposal was also more radical in that it concerned "ontology," but it forces the implications for "realism."

[4] Dummett (1978, pp. xxvii, 146, 228) attributes to Kreisel the precise quote 'The point is not the existence of mathematical objects, but the objectivity of mathematical truth'. This attribution is, I believe, a mistake. He refers to Kreisel's "review of Wittgenstein" (p. xxvii). But Kreisel's (1958) review of Wittgenstein's *Remark on the Foundations of Mathematics*, published in BJPS (*British Journal of the Philosophy of Science*) contains no such quote and no such dictum. The nearest it comes to something that might suggest the idea is in footnote 1 on page 138: "...Incidentally, it should be noted that Wittgenstein argues against a notion of a mathematical object (presumably: substance), but, at least in places (p. 124,35 or p. 96, 71, lines 5 and 4 from below) not against the objectivity of mathematics..." Neither is the point mentioned in Kreisel's later review of *The Blue and Brown Books* ("Wittgenstein's Theory and Practice of Philosophy" BJPS, 1960). I have searched without success in quite a few of Kreisel's many publications in the relevant period and am inclined to think that the quote is Dummett's own phrasing of an idea that came to him as he read the above mentioned footnote. Dummett's attribution seems to be the only source of the folklore that grew about this matter. I would be grateful to anyone that can point to me the dictum in Kreisel's works. As far as I presently know, my 1975 paper was the first work in print that elaborated such a position, though in the philosophical community the dictum became known through Dummett's 1963 lecture.

mathematicians believe that (1) has a definite answer, though we might not find it due to our limited computational and proof-theoretic resources. Ultrafinitists consider such questions factually meaningless, if the exponents are sufficiently high. I believe the exponential term I chose can be used to represent all variants of the position; if not—increase it. (2) is used to mark *finitism* (Hilbert's position), as well as intuitionistic views. (3) and (4) serve to characterize positions with respect to statements in first-order arithmetic; in particular, the acceptance of (4) as a factual statement characterizes the default position with regard to the natural numbers, held, implicitly or explicitly, by the majority of mathematicians. (5), (6), and (7) are used to indicate positions in higher-order arithmetic and in set theory.

Note that I do *not* claim that positions can be arranged in a total ordering; probably they cannot. In spite of the title of this section, the list should not always suggest an ordering; some logicians, who give a positive answer to (7), may pause when they consider (6). The list is also not intended to mark an overall systematic classification. I have not gone into nice distinctions between finitism, intuitionism, and various brands of constructivism. Neither do I go into the higher reaches of set theory and the very rich material of the last five decades. But as an illustration of the general method the list will do; besides, the items are interesting in their own right. I shall discuss them in their numeric order. The presupposed technical knowledge varies; hopefully, there is enough for the nontechnical readers to appreciate the general idea. I include in Footnote 5 definitions, explanations, and notations regarding the arithmetic hierarchy.[5]

Since some of the discussions are relatively long, the section is divided into five subsections. As far as the rest of the paper is considered Section 2.1, which is devoted to ultrafinitism, can be skipped.

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[5] The first-order language of arithmetic is based on a name for 0, names (function symbols) for the successor function, addition and multiplication, the equality symbol, sentential connectives, and first-order quantifiers. The intended interpretation of the language is the so-called standard model of natural numbers. A very rich class of relations and functions can be defined in terms of this vocabulary. *Bounded quantifiers* are of the form $\forall x \leq t$ and $\exists x \leq t$ (read: "for all $x \leq t$" and "there exists $x \leq t$"), where t is any term; they are interpreted in the obvious way and can be expressed in terms of the original vocabulary (since '$\leq$' is definable); but for many purposes it is convenient to add them, as well as '$\leq$' as basic. A *bounded wff* is one that can be built by using, in addition to sentential connectives, only bounded quantification (such wffs are also classified as Σ_0, as well as Π_0, as well as Δ_0). Σ_1 *formulas* and Π_1 *formulas* are, respectively, of the forms $\exists x_1 \ldots x_k \phi$, $\forall x_1 \ldots x_k \phi$, where ϕ is Σ_0 and where k is any number (if $k = 0$, this is defined to be ϕ, thus, every wff that is Σ_0 is also Σ_1 as well as Π_1). In general, the Σ_{n+1} and Π_{n+1} wffs are $\exists x_1 \ldots x_k \phi$ and $\forall x_1 \ldots x_k \phi$, where ϕ is, respectively, Π_n and Σ_n. A Σ_n-*relation* (Π_n-*relation*) is one definable by a Σ_n formula (Π_n formula). A Δ_n-*relation* is a relation that is both Σ_n and Π_n. It is easily seen that the Π_n relations are exactly the complements of Σ_n relations. This classification is referred to as the *arithmetic hierarchy*. It can be shown that the class of Δ_{n+1} relations properly includes the Σ_n relations as well as the Π_n relations. Moreover the Δ_1 relations are exactly the *computable* relations (known also as *recursive*) and the Σ_1 relations are exactly the c.e. (*computably enumerable*) ones, known also as r.e. relations.

That classification is also significant with regard to the formal theory: roughly speaking, the induction scheme, $\phi(0) \wedge \forall x \, (\varphi(x) \rightarrow \varphi(x+1)) \rightarrow \forall x \varphi(x)$, is stronger if we allow more alternating blocks of unbounded quantifiers in ϕ. Sometimes a superscript '0' is added to indicate that the quantifiers in question are first-order, for example, 'Σ_n' rewritten as 'Σ_n^0'; this is done to distinguish this hierarchy from analogous ones based on higher order quantifiers, for example, a Σ_1^1 formula is of the form $\exists X_1, \ldots, X_k \, \phi$, where the X_i's are second-order variables ranging over all sets of natural numbers, and ϕ involves only first-order quantification.

2.1. Ultrafinitism.   *Ultrafinitism,* or *strict finitism*, is often characterized by the dramatic claim "*t* does not exist," where '*t*' stands for some sufficiently high exponential term; this means that the term is to be regarded as a formal symbol lacking denotation. To be more specific, the ultrafinitist basic position is that the natural numbers are closed under addition and multiplication,[6] but are not closed under exponentiation. The choice of exponentiation, rather than some other fast growing function, seems right. It marks the first crucial big jump: in computer science—from polynomial time to exponential time, and in set theory—from a set to its power set (which is even more striking for infinite sets than for finite ones.[7])

I believe that the exponential in (1) is high enough to take care of possible different versions of ultrafinitism. I have also chosen it because at the time of the writing $2^{43112609} - 1$ was the largest known prime; the proof involved sophisticated number theory and heavy use of computers. Nelson (1986), who argued that 2^{65536} does not exist,[8] will regard the proof as a mere formal manipulation of symbols.

The position originated in the works of Esenin-Volpin (1961, 1970),[9] which are often obscure but contain some striking suggestive ideas. A major step was achieved in Parikh (1971). Dummett (1975) gave an intuitive semi-formal argument purporting to show that ultrafinitism is incoherent; but the argument fails in an interesting way. Nelson's (1986) book, in spite of its faults, is impressive in its systematic rigorous working out of a formal deductive system. Although ultrafinitism is a topic of lively discussion on the internet, Nelson's book is, as far as I know, the only worked out attempt at a full-fledged formal system.

The basis for the ultrafinitist formal system is a weak subtheory of PA (Peano Arithmetic), denoted as $I\Delta_0$ (or as $I\Sigma_0$). It is obtained by restricting the induction scheme to bounded wffs, that is, Δ_0 (or Σ_0) formulas (see Footnote 5). There is a bounded wff, $exp(x, y, z)$, which expresses the equality $x^y = z$, in the sense that in $I\Delta_0$ it is provable that there is at most one z such that $exp(x, y, z)$, and that if the function is defined for x, y it is defined for all $x' \leq x$, $y' \leq y$ and it satisfies the usual inductive clauses for exponentiation; in particular, $I\Delta_0 \vdash exp(x, y, z) \rightarrow exp(x, y{+}1, z \cdot x)$. Interpreting "$x^y = z$" as $exp(x, y, z)$, we can assert in $I\Delta_0$: $\neg\exists z(x^y = z)$, that is, "x^y does not exist." The sentence $\exists y \neg\exists z\, (2^y = z)$ is compatible with $I\Delta_0$.

A common reaction to ultrafinitism is "so what is the largest y such that 2^y exists?" The answer is that there is no such y; the existence of a maximal number in every set bounded from above requires induction that is not provided in $I\Delta_0$. In particular, we need $I\Sigma_1$ (induction over Σ_1 formulas), to guarantee that exponentiation is a total function. Another difficulty that might arise here is that by starting with $2^0 = 1$, we can prove, for each n, $\exists z(2^n = z)$, in a proof that uses repeatedly $\exists z\, (2^y = z) \rightarrow \exists z\, (2^{y+1} = z)$. The

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[6] This is guaranteed if the formal system has the function symbols '+' and '·'. Alternatively, we can have predicates for the three-place relations, $x + y = z$, $x \cdot y = z$, and axioms (or theorems) asserting that for all x, y there is a unique z such that $x + y = z$, and similarly for multiplication.

[7] This is shown by the fact that we need a special axiom, AC (axiom of choice), to guarantee that the power set should inherit from the set the property of having a well ordering, or even the property of having a simple ordering.

[8] Nelson's (1986) argument for this particular claim appeals to physics, namely, the fact that, on the physical theory of the time, it will take 10^{19684} ages of the universe to count from 1 to 2^{265536}, where each count takes 10^{-24} seconds. I think the appeal to physics is misguided in this case, and moreover that the estimate for a nondenoting term should be much higher, but I shall not go into it here.

[9] Though he referred to it as "ultraintuitionism."

ultrafinitist can counter this suggestion by observing that the proof will be "too large" and will not exist in the ultrafinitist universe. He can also rule out proofs that contain nondenoting terms, as mere formal manipulations of symbols.

A model of $I\Delta_0$ in which exponentiation is not total is obtained by using a nonstandard model of PA and taking any initial segment of it that is closed under successor and multiplication, but not under exponentiation (there are infinitely many such initial segments in every nonstandard model of PA). Initial segments of models of PA, also known as *cuts*, are a useful tool in *bounded arithmetic*, the study of subsystems of Peano, obtained by restricting the induction scheme to certain subsets of wffs. Nelson used them as a guideline, although, as infinite structures, they are from an ultrafinitist point of view no more than a heuristic fantasy.

Nelson suggested a well- defined methodology for finding theories suitable for ultrafinitist mathematics. It yields initially $I\Delta_0$, which in itself seems too weak; in a further stage it yields an axiom denoted as Ω_1 (and already known in bounded arithmetic studies), which states that the function $x^{|x|}$ is total: $\forall x \exists y \ (y = x^{|x|})$. Here $|x|$ is the length of the binary sequence that denotes x in binary notation; it is easy to see that $log(x) < |x| < log(x)+1$. While $I\Delta_0$ guarantees the closure of the natural numbers under functions defined by bounded wffs (and it is easy to see that they cannot increase faster than polynomial growth), Ω_1 implies closure under a function that grows as $x^{log(x)}$, which is more than polynomial growth. Nelson's ambitious goal was to get a version of ultrafinitist number theory which is immune to Gödel's second incompleteness theorem, that is, it should prove its own consistency (or something very near it). As Iwan (2000) pointed out, however, various subsequent results in bounded arithmetic show that Nelson's suggested methodology will not achieve such a goal.[10]

The failure of ambitious goals, such as bypassing the second incompleteness theorem (or showing that PA inconsistent) does not mean that ultrafinitism is not a tenable position. Perhaps $I\Delta_0 + \Omega_1$, or some extension of it, is a good enough ultrafinitist theory. All foundational positions have to live with the incompleteness results and ultrafinitism fares no better. The problem is that we miss a good understanding of what the *finite* universe of the ultrafinitist may look like, one that is not some infinite nonstandard model. A suggestion of Esenin-Volpin (1961) can play here a crucial role: there are finite ordered sets that do not have a largest member, for example: the set of heart beats during a person's childhood.[11] A short calculation will show that this number is smaller than 60×10^8; yet if one is a child at the nth heartbeat, one is also a child at the $(n+1)^{th}$ heartbeat. These phenomena underlie vagueness as well as tolerance in engineering; they suggest that an ultrafinitist may consistently insist that some numeric symbols denote natural numbers and others are mere

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[10] Nelson, as it now appears, has continued in a different direction. On September 26, 2011 he announced that he was writing a proof that PA (actually, a much weaker system) is inconsistent. He posted on his website a short outline and 100 pages of work in progress—the beginning of a book entitled *Elements*, which would include the proof and initiate a program of basing mathematics on firm foundations. He also developed a proof checker, qea (*quod est absurdum*), to verify the proof. On September 27, Terrence Tao, a Field medalist from UCLA, posted on Google+ a note pointing to an error in the proof (in a part that has not been checked by qea); the same error has been independently found by Daniel Tausk, from the university of São Paulo. On October 1 Nelson graciously acknowledge the error and withdrew his claim, remarking "the consistency of P remains an open problem." The work in progress is no longer on his website.

[11] Apparently Nelson was not aware of Volpin's works, or if he was, he did not think them sufficiently clear to merit some mention.

notations, without having to assume that there is a largest natural number. Using vagueness in mathematical contexts is a radical move, but no more radical than ultrafinitism itself. So here is the idea. Use truly finite but very large numbers to simulate various aspects of an infinite cut in a nonstandard model. Some terms denote numbers and some do not; but as the denoted numbers become bigger vagueness takes over and there is no sharp cutoff for denoting terms. The soundness of a theory should mean that the sentences that are provable by proofs that are contained in the ultrafinitist world are true; and this becomes vague, as the entities in question grow larger. To make all this precise will take hard work.

So far ultrafinitism is best regarded as an unfinished project, or perhaps a collection of projects with a basic shared view. I believe that it can be developed into a coherent tenable position.

2.2. Finitism and intuitionism.   (2), recall, is the question whether some given Diophantine equation has solutions. It involves quantification over all natural numbers, and this—in systems that are based on classical logic—amounts to an acceptance of actual infinities. It will not be regarded as a factual question by finitists of Hilbert's stripe (cf. Hilbert, 1925), unless the questions can be decided by a proof acceptable to the finitist. Also intuitionists will not accept it, unless the answer can be derived in an intuitionistic system. Both finitists and intuitionists do not apply classical logic where this involves quantification over infinite totalities. The intuitionists allow quantification over all numbers, but they change the logic. Hilbert, who did not want to give up classical logic, restricted severely the quantification. Following Tait's (1981) plausible suggestion, we can formally capture Hilbert's position in PRA, the system of *primitive recursive arithmetic*, which is weaker than intuitionistic first-order arithmetic.[12]

Of course, by substituting particular numbers for the x_i's we can always evaluate the polynomial and, if the equality holds, prove that the answer is 'yes'; in that case the question is settled no matter what system we use. The problematic cases are those in which, classically speaking, the answer is negative—that is, the inequality holds in all instantiations. Sometimes a negative answer can be proved in the system in question (e.g., one can prove in PRA general inequalities of the form $p(x_1, x_2, \ldots, x_k) \neq 0$, where the x_i's are free variables) and also in this case the question has factual meaning. But if one treats the question as having an objective answer, independent of its being provable in this or that system, one adopts thereby a realistic position with respect to questions of this type.

2.3. Realism with regard to first-order arithmetical statements.   While (2) is a question on the first level of the arithmetical hierarchy—it concerns the truth-or-falsity of a Σ_1^0 (or a Π_1^0) sentence—(3) appears to be on the second level—it involves a Π_2^0 sentence:

$$\forall x \, \exists y (\, y > x \, \wedge \, y \text{ is prime} \wedge y + 2 \text{ is prime}).$$

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[12] PRA is based on a vocabulary with names for 0 and the successor function; it has no quantifiers but it allows one to add new function symbols for functions defined by simple induction (these functions can be used in subsequent definitions to introduce further functions). The induction clauses are treated as axioms. The named functions in PRA are all the primitive recursive functions. An instantiation rule allows us to substitute the free variables of any theorem by any terms. The induction axiom appears as an inference rule. Some existential claims can be proved in PRA in a functional form; for example, for a quantifier free ϕ, $\forall x \exists y \phi(\ldots x \ldots y \ldots)$ can be proved by deriving $\phi(\ldots x \ldots f(x) \ldots)$, where f is some defined function. The machinery of PRA can be implemented within first-order intuitionistic arithmetic, so that the theorems of the first become theorems of the second. But PRA is strictly weaker: some wffs of PRA that are provable in the intuitionistic system are not provable in PRA.

(4) involves the full language of first-order arithmetic. To accept all questions of type (4) as factually meaningful is to adopt a realistic position with regard to the standard model of natural numbers. If you want, we can call this Platonism with regard to natural numbers. (It has been lately the vogue to call "Platonist" anyone who believes in the existence of abstract objects. I doubt that this is a good use of the term, but why should I quibble about a name?). We can safely say that the great majority of mathematicians adopt a *de facto* realist position with respect to the natural numbers, and most of those who are interested in foundational questions are also realists *de jure*. The source of the realist conviction is something like the following argument, illustrated here in the case of (3): For any given number n, either n is divisible by a smaller number greater than 1, or not; in principle, the answer can be effectively found by carrying out these divisions. Now going through the sequence of natural numbers we can mark all the cases in which both n and $n+2$ are prime; in this process either the finding of new twin primes will never cease—meaning that there are infinitely many of them, or from some point on no twin primes will be found—meaning that their total number is finite. It is either the one or the other. There seems to be no wiggle place for another possibility. In the case of Π_1^0 (or Σ_1^0) sentences the intuition is even stronger: running through all the natural numbers (or rather through all m-tuples of natural numbers, for some fixed m) we either find or do not find a counterexample to a universal hypothesis whose instances can be effectively checked. Using the MRDP theorem (Matiyasevich, 1970), any Π_1^0 sentence is equivalent to a sentence stating that a certain Diophantine equation has no solution.[13] The extension of such a view to all sentences in first-order arithmetic appeals to a sort of metainduction: *If, for each n, the satisfaction or nonsatisfaction of φ(x) by n is an objective fact, then, going through all the natural numbers, either we find some n for which φ(n) is objectively true, or all of them are objectively false; hence the truth-or-falsity of ∃xφ(x) and ∀x¬φ(x) are objective facts.*

Does the last heuristic argument imply that realism with respect to natural numbers is inevitable? No. Finitist and intuitionist positions deny that the picture of "running through all the numbers" is a basis for treating the sentences in question as factually meaningful. These are viable conceptions, but their ontological parsimony comes at a considerable price: One cannot discuss some simple metatheoretical questions, such as the provability of a given sentence, or the consistency of a given formal theory. These discussions of the formal system employ classical logic and they presuppose that the questions discussed have factual meaning. But to ask whether ϕ is provable is to ask whether there is a proof of ϕ, and to say that a theory T is consistent is to say there is no proof in T of a contradiction. Now proofs, as well as the other syntactic entities of a formal system can be effectively coded into natural numbers; and vice versa, the natural numbers (and the basic arithmetic functions) can be coded into the syntax, provided that the syntax—based on strings of symbols, or trees, or what have you—is minimally expressive. Thus, 'ϕ is provable' and 'T is consistent' are Σ_1^0 and Π_1^0 sentences. Those who make use of these concepts—which are common in philosophical discussions of formal systems—are therefore committed to realism, at least vis-à-vis the first level of the arithmetic hierarchy.

Philosophers, whose approach is usually object oriented, might deny the significance of the back-and-forth encodings; they might argue that the objects in question, numbers and proofs, are not the same. Proofs might appear safer since they are more "concrete," but

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[13]   For a more detailed explanation of the reduction to Diophantine equations, see Section 6.3

this is an illusion; proofs are abstract objects, and if one's objection to quantifying over numbers derives from their being abstract, then one cannot appeal to the very notion of provability.

In some contexts the quantifier is implicitly bounded and therefore involves no such commitment; for example, when Nelson speaks of the possibility that in PA there is a proof of a contradiction, he is constrained by his own position to some finite set of proofs. Also if someone says that he or she will try to prove some conjecture, this should not involve quantification over all proofs; one can reasonably restrict the quantifier to proofs that a human being can produce within reasonable time. But in general, talk of consistency or provability is intended to cover *all* proofs.

Abraham Robinson, who was a finitist, or something very near to it, realized the seriousness of the limitations that his position implied with regard to syntactic concepts that required quantification over infinite domains. In Robinson (1965) he addressed it, providing an account that accepted the metatheoretic limitations that his position implied. He is, as far as I know, the only philosopher of mathematics who addressed this problem in detail. More recently, Field (1998) appreciated the problem, and used it to argue for the need to secure the standard concept of a natural numbers (or, equivalently, the concept of a finite set). Unfortunately, he appeals to physics to provide the required modeling, in a way that involves a vicious circle. I shall argue in Section 5.2 that his proposal fails to secure any additional objective ground for the concept 'finite'.

If, like most mathematicians, we take the standard model for granted and interpret the quantifiers in every sentence accordingly, we get a realistic commitment for all sentences in first-order arithmetic. This is not exactly the intuitive metainductive argument I outlined above; the argument indicates that there might be a real advance in the commitment as we move from one level to the next—a possibility we may want to keep open.

The use of a stronger theory, for example, ZF, in order to define the standard model, only shifts the question to the credentials of the stronger theory. In my view the concept of *finiteness*, or of repeating some elementary step *a finite number of times*, is more fundamental than the concept of *set* that is captured in ZF.

Perhaps limited commitments, in particular commitments to the first arithmetical level only, might emerge under the pressure of some very strong independence results, which might undermine our confidence that we have sufficient understanding of the standard model. Consider, for example, the following radical scenario:

> (RS) A proof is found that the twin prime conjecture is independent of ZFC (if ZFC is consistent),[14] and this proof does not give us any indication as to the truth or falsity of the conjecture in the standard model.

Presently this seems a remote possibility; the result would be truly sensational. One way in which this can happen is the construction of two models of ZFC in which the twin prime conjecture has different truth values; in both the natural numbers are not standard (i.e., these are not ω-models), and the construction yields no indication as to what happens in the standard model. Assuming (RS), I do not think that we shall give up the concept of a unique standard model, yet there might be some who will entertain the possibility of two legitimate concepts of natural numbers, in one the twin prime conjecture is true and in the other it is false. Note that no such scenario can undermine realism with regard to the first level: the truth of a Π_1 sentence in any model of PA implies its truth in the

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[14] ZF is Zermelo-Fraenkel set theory. ZFC is ZF augmented with the axiom of choice.

standard model. This is related to the well-known observation that the independence of a Π_1 sentence implies its truth, which will be discussed in section 6.2

Leaving aside the possibility of (RS), there are several tenable views that do not subscribe to a realist conception of the standard model. Finitism and intuitionism are such views. Another possibility has emerged from the multiverse conception proposed by Hamkins (2011). On this view, the model of natural numbers depends on the set-theoretic universe containing the model. What it comes to is that we have a clear enough conception of models of PA, or of some extensions of PA, but we have no clear distinction between standard and nonstandard models. Perhaps Abraham Robinson, the founder of nonstandard mathematics, who was not a set theoretician, would agree to that.

2.4. Higher order arithmetic and set theory.

The move from (4) to (5) is a major jump that involves quantification over all subsets of the natural numbers (or equivalently, over all real numbers). (5) belongs to the second-order hierarchy, and it is there on the third level; the question amounts to asking whether a certain Σ_3^1 statement is true. (By Shoenfield's absoluteness theorem, a question whose answer may depend on whether the sets are restricted to the constructible universe cannot arise on a lower level.)

(6)—the truth or falsity of CH—is on the second level of the second-order theory of reals, or equivalently the third-order theory of arithmetic, that is, Σ_2^2. (7) puts us in a different perspective altogether, beyond higher order arithmetic: that of "Cantor's universe." The questions though are often related: for example, a positive answer to (7) entails a positive answer to (5) (but nothing regarding (6)).

I shall not enter here into the question of realism in set theory, which is a wide area by itself. I shall only remark that the key notion, which plays an analogous role to that of the standard natural numbers is the notion of the *power set* of a given set; in particular, $\mathcal{P}(N)$, the set of all subsets of N, and $\mathcal{P}(\mathcal{P}(N))$; here N is the set of natural numbers. The standard natural numbers are characterized by a *minimality* condition: N is the *smallest* set closed under successor; it forms also a model of PA, and every model of PA has a unique initial segment isomorphic to it (under a unique isomorphism). On the other hand $\mathcal{P}(N)$ is characterized by a *maximality* condition: *every* subset of N belongs to it; and similarly for $\mathcal{P}(\mathcal{P}(N))$. This, it turned out, makes a huge difference. Something like (RS) happened in set theory. The methods of constructing and extending models allowed us to switch back and forth the truth value of CH by repeated extensions of a given model. This and the fact that CH remains undecided in the presence of large cardinal axioms have convinced some top set theoreticians, for example, Shelah (2003), that there is no unique answer; more generally, the view is that there is a plurality of legitimate set theories (some better than others, but no "best" one). On this view the situation resembles the split that took place in Geometry upon the discovery of Non-Euclidean geometries. Other top set theoreticians, for example, Woodin (2001), think that the concept of the power set of a given set is sufficiently clear to warrant a realistic conception, according to which CH has a definite answer. What is sometime known as the California school has engaged in a research program, whose aim it to find axioms that will lead to the right answer.

I believe however that there is much more to the story than that. Even under the pressure of radical scenarios such as (RS) we would be much less likely to adopt a pluralistic view with regard to the natural numbers. But this is a subject for a separate work.

2.5. Further observations.

Those who do not accept some questions as factually meaningful can be further divided into deniers, who hold that in the final account the questions are factually meaningless, and agnostics who prefer to suspend judgment. The

big historical debates at the turn of the last century were between affirmers and deniers. Agnosticism can express a sincere personal attitude. But as a philosophical position it matters only to the extent that it implies some distinction between those mathematical statements whose factuality is accepted and those whose factuality is either denied or left open.

Now one can reject the factual meaning of various mathematical statements, yet accept the use of the theory in which they figure, as an instrument for discovering theorems that *do* have factual meaning. This was Hilbert's position with respect to theories involving actual infinities. His project aimed at showing how these infinities can be treated as idealized entities, which are very useful in proofs and will never lead us to contradiction. Hilbert's goal is famously known as a finitist consistency proof for classical mathematical systems; such a proof will imply that we can safely use them. It is also not difficult to show that such a proof would have yielded an effective translation of ordinary mathematical proofs, of sentences acceptable by a finitist, into finitist proofs of the very same sentences.[15] Thus it will show how the instrument can in principle be eliminated. After Gödel we know that this is impossible. Yet, finitism is a viable position.

We may at the same time wonder, what is the source of the often encountered belief that the "instrument" is reliable? Since there seems to be no point in working within a theory in which 0 =1 is provable, consistency is taken for granted. But this cannot constitute a *reason* for taking it so. There is also more to it than the gross empirical observation that so far the "instrument" has worked well; we trust it not as we trust a black box with a good record of outputs. An expert will know a lot about the internal mechanism of the box. Often, but not always, the expert's intuition is guided by a semantic picture—an interpretation of the formalism in some sort of structure, or family of structures—which implies that the formalism does not lead to contradiction. But apart from this the expert knows something about the patterns of reasoning associated with a formal system. Hilbert, in spite of his finitism, was enthusiastic about set theory, the paradigmatic theory devoted to actual infinities; its beauty and unifying power were obvious to him: "No one shall drive us out of the paradise which Cantor has created for us" (Hilbert, 1925).[16]

Expert intuition which is a product of intensive practice, and which is continuously confirmed and developed by new results, applies across the board. This includes areas where there is no semantic picture to guide us, such as the study of formal systems from a purely combinatorial perspective. Getting good insight into certain patterns of reasoning, the expert can become more confident that they will not lead us to contradiction. All of this *and* the accumulated record is the source of our belief. Expert intuition can be also the ground for believing that some theory is likely to be contradictory. Nelson, who invested considerable effort to prove that PA is inconsistent, had philosophical motivation; but he relied on some intuitive technical ideas how the contradiction could be obtained.

§3. **Ontology and Quine's criterion.**   One's position with regard to questions of the types exemplified in the list raises basic issues concerning the status of mathematics: What is the source of mathematical validity? By virtue of what are mathematical statements true? What kind of necessity appertains to them? Broadly speaking these are ontological questions, provided that we interpret "ontological" in the broad sense that is appropriate

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[15] This point is almost always ignored. Historically, Hilbert was probably motivated by the set theoretical paradoxes, which suggested that established methods might hide unsuspected contradictions, hence, the justified need to establish that the instrument we are using is safe.

[16] For a more detailed discussion of Hilbert's position see Section 6.

to this type of investigation. That is, not through a direct answer to the traditional question "what things exist?"—a question that is likely to trigger endless unilluminating debates, where the sides appeal to different intuitions and clashing pictures about what merits the status of an "existing object." The discussions can easily degenerate into a mixture of philosophy and lexicography about the right use of terms. In "On what there is" Quine (1948) provides an entertaining illustration of what can happen in such exchanges.[17] The ontological question should be rather approached by taking into consideration the working of the theory, or the framework, as a whole. Quine's proposal of using the values of quantified variables, in order to characterize the ontological commitment of a theory, is a move in that direction; it shifts the focus from the denotations of names, to the broader perspective of the values of quantified variables. At the same time the move, which focuses on objects, preserves the traditional object-oriented picture.

Quine's criterion is useful for marking the great divide between first-order and second-order arithmetic; or, more generally, between the nth and the $(n+1)^{th}$ orders. But his move is not sufficient and the criterion is not helpful at all when it comes to distinctions within the first-order systems. Consider for example Hilbert's finitist position, which accepts the totality of all natural numbers as a potential but not an actual (or "accomplished") infinity. As noted above, finitism remains a viable option. It shuns quantification over all natural numbers, but provides instead a powerful mechanism of inductive definition and inductive proofs, sufficient for defining all primitive recursive functions and for establishing, by induction, a host of theorems.[18] It is also sufficient for defining the syntax of the language and the notions of proof and computation. The language does not have quantifiers, but one can prove in it many universal claims—by proving a wff with free variables, and many existential claims—by defining the appropriate function and proving that its values satisfy a certain condition. Thus one can prove that there is a computation whose output is the answer of (1). It would be absurd to claim that finitism is not committed to "the existence of all natural numbers";[19] it is only the more recent ultrafinitism that can be thus described. The bound variables criterion is also a poor guide in most of the cases arising within set theory, for example, (7).

Larger units of meaning—that is, units represented by larger syntactic constructs—should serve as a basis for the analysis. Many years ago I suggested that ontological

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[17] "However, Wyman, in an ill-conceived effort to appear agreeable, genially grants us the nonexistence of Pegasus and then, contrary to what *we* meant by non-existence of Pegasus, insists that Pegasus *is.* Existence is one thing, he says, and subsistence is another. The only way I know of coping with this obfuscation of issues is to *give* Wyman the word "exist". I'll try not to use it again; I still have "is". So much for lexicorgraphy;" Quine (1948, p. 23).

[18] The extent to which Tait's proposal reflects the historical Hilbert does not matter for my argument. The point remains that Quine's criterion is no help at all in distinguishing a view that is based on the distinction between potential and actual infinity.

[19] Quine (1948, p. 34) tries to use his criterion in order to explain the differences between realism, intuitionism, and finitism. The result is a brief passage consisting of statements that are either inaccurate or plainly false. He conflates intuitionism with conceptualism—the view that only defined classes exist—which is flatly wrong. And he explains Hilbert's position as motivated either by the wish to preserve classical logic or by the rejection of abstract entities ("even in the restrained sense of mind-made entities"). While it is true that Hilbert aimed at preserving the logic used by mathematicians, his main motive was a philosophical rejection of actual infinities. And the suggestion concerning abstract entities is wrong. Hilbert's idealized finite strings of strokes are hardly "non-abstract," especially when they form an unending sequence. Hilbert refers to them as "objects of thought."

questions should be construed as questions about the commitment of a conceptual framework to statements which are considered objectively true or false.[20] A similar view for characterizing realism had been stated and developed Dummett. My position was however broader: the ontology itself, to which a theory is committed, should be characterized in terms of facts (which, in a language-based analysis, correspond to declarative sentences), rather than objects. Nowadays I would consider the category of factual questions as the more appropriate one; true-or-false statements correspond to questions with yes/no answers, and not every question can be reduced to that form. (1) is an example of the more general use of questions. Similarly, one can consider, instead of (6), the broader question: What $\aleph_\alpha$ is the cardinality of the continuum? Philosophers who truck in ontology continue however to be wedded to the traditional object-oriented ontology.

§4. On the so-called "Ontology/Ideology" distinction. A misleading distinction, which is often used in discussions of ontology, is the distinction between "ontology" and "ideology." It derives from the traditional metaphor of the world as made of properties-carrying primordial "stuff." Applying the tools of formal logic, the stuff—that is, the "ontology"—is identified with the objects (indicated by the quantifiable variables) and the properties—that is, the "ideology"—is identified with the bunch of properties and relations (marked by the predicates). Quine (1969) modified his approach, adopting a holistic theory-oriented perspective; he ended by completely relativizing "ontology" to theories, which, I think, defuses much of the interest in ontological questions.[21] Quine is not my present concern, but his observation (Quine, 1969, p. 63) is relevant here:

> Ontology is internally indifferent also, I think, to any theory that is complete and decidable. Where we can always settle truth values mechanically, there is no evident internal reason for interest in the theory of quantifiers nor, therefore, in values of variables. These matters take on significance only as we think of the decidable theory as embedded in a richer background theory in which the variables and their values are serious business.

The observation, I think, is essentially correct.[22] Quine was motivated here by the truth-or-falsity perspective and the feeling that if truth values can be settled by a mechanical procedure, then no real ontological content is involved. Yet it goes against the very gist of object-based ontology. If ontology is determined by the values of bound variables, then surely the fact that the theory is complete and decidable is purely an epistemological

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[20] Gaifman (1975, 1976). See also Footnotes 3, 4.

[21] Quine leaves no place for a serious comparison and evaluation of different ontological views. One chooses one's overall theory and then one is stuck with whatever ontology it implies. The rest is pragmatics or a matter of taste. I think however that ontological questions can be discussed from a very general point of view, taking *objective* or *factual question*, as primitive concepts. I do not have to be an intuitionist in order to understand the intuitionist's motivation, or an extreme realist with respect to sets in order to appreciate that position. A serious dialogue between adherents of different views is possible. The discussion need not end by convincing one of the sides (it usually does not). Suffices that it lead to better understanding.

[22] In certain contexts, however, one may have to consider also the complexity of the decision procedure: how long does it take to decide the truth value of a given sentence? If ontology has to do with "truth beyond knowledge" then the degree of "beyond knowledge" has to be taken into account.

matter. It means that we are in the happy situation in which we can settle, by a mechanical procedure, questions of truth and falsity. Why should this have any ontological implications at all?

Possibly, Quine was led to his observation by classical examples of decidable theories. The earliest example is probably Presburger arithmetic—the theory of the standard natural numbers, with addition (and with the successor function) but without multiplication. Mojźesz Presburger provided a decision procedure for that theory: some computer program can compute, given any sentence in that language, the sentence's truth-value. If we add multiplication to the model (say, as a three-place relation) we find ourselves in the deep waters of Gödel's theorems, the problems of unknowable truths and the problem of realism vis-à-vis the standard model. Yet nothing has changed as far as the objects are concerned, or the quantifiers. No new numbers have been added. There are dozens of such examples. Here is a very neat one. Consider the first-order theory of real numbers, under addition and multiplication; we can also assume that the language has names for 0 and 1—hence also names for all natural numbers—and for the ordering relation, since these are definable in terms of the original vocabulary. By Tarski's decidability result this theory is complete and decidable. Add merely one monadic predicate 'x is a natural number', which is true exactly of the natural numbers, and all hell breaks loose: the system is now equivalent to full second-order arithmetic.

Quine qualifies his observation by noting that the original theory can be ontologically significant in the context of a larger theory in which the values of the variables are "serious business." One could argue that the possibility of extending first-order real number theory by additional predicates, such as 'x is a natural number', is what endows the real numbers with their ontological significance. I can grant this, for it only shows that the nontrivial ontology that comes with the real numbers is realized only when they come with additional relations. In short, the ontology inheres in the combined setup "objects+relations." This is not to deny the asymmetric roles that objects and relations play. A given relational structure (model) can be augmented by adding more objects—which leads to an *extension* (in the narrow sense of "extension"), or it can be augmented by adding relations, in which case we get an *enrichment*—a different kind of augmentation. In general, we find it useful to regard a theory as being about such and such objects, and to cluster theories according to the objects they are about. But at least in mathematics and in certain examples from the sciences, the difference is *logical*—in the broad sense in which "logic" has to do with the basic organization of thinking. The ontology does not reside in the objects by themselves, or in the relations, but in their combination.

This last point was appreciated (at least implicitly) in the nominalistic foundational program for mathematics, proposed in (Goodman and Quine, 1947). Their aim was to dispense with abstract entities, which entailed more than using only concrete individuals in modeling the natural numbers:

> By renouncing abstract entities, we of course exclude all predicates which are not predicates of concrete individuals or explained in terms of predicates of concrete individuals. Moreover, we reject any statement or definition–even one that explains some predicates of concrete individuals in terms of others-if it commits us to abstract entities. For example, until we find some way of construing 'is an ancestor of ' in terms of 'is a parent of ' other than the way the ancestral of a relation is usually defined in systems of logic, the relationship between these predicates remains for us unexplained.

The impossibility of defining the ancestor relation as a predicate that belongs to the language of concrete individuals brought their nominalistic program to its end. The failure attests to the seriousness of their attempt; in a way it gives them credit.

§5. "Concrete" modeling of mathematical structures.   Various foundational views have been motivated by the need to anchor mathematical structures in concrete items. The naïve formalist account, which identified numbers with numerals, and the whole of algebra with a system of rules for manipulating inscriptions, arises from attempts to secure such concrete anchoring.[23] It is a persistent tendency, which had and is continuing to have its effects in the philosophy of mathematics. One attempted way to anchor mathematics in something "concrete" is to base it on physics.

5.1. Can mathematical objectivity be secured through physics?   I am not going to answer this general difficult question. I shall only discuss what I take to be a failed attempt to secure the objective status of the concept of finiteness (hence also that of the standard numeric progression) through a physical-based modeling. I think the failure illuminates an important aspect of the relation between mathematics and physics.

Field (1998) proposed a definition of finiteness, which is based on physical discrete time, or, to be more precise, events ordered by time. Consider some events arranged in temporal order, $e_1, e_2, \ldots, e_\xi$, where e_1 and e_ξ are first and last members, in which any two events are temporally separated by at least one second (the choice of lower bound makes no difference). Such sets are to serve as templates for defining finiteness and I shall refer to them by that name. A set (or the extension of a predicate) is defined to be finite if it can be embedded into some template by a one-to-one mapping.

In order to guarantee the existence of arbitrarily large finite sets, we assume the cosmological hypothesis that time is infinite (say, for every event there is another event at least one second later). This of course will not suffice, since we have to make sure that our templates are indeed *finite* (the concept we have to secure). To ensure this we make another cosmological assumption: time is Archimedean. Now the definition of "Archimedean" makes full use of the mathematical concept of *finite*, and to say that time is Archimedean is just to rule out nonfinite templates. Field claims that, if the two cosmological hypotheses hold, the definition extends the determinacy of physical concepts to the mathematical concept of finiteness. (He uses "determinate" to characterize objective truth values of sentences, but he also uses it characterize the objective status of concepts or objective senses.) One may perhaps smell something fishy, since the concept of finiteness has been invoked in the very hypothesis of Archimedean time. Field is aware of the point and he is not very happy about the circularity; but he ends by arguing that, in general, we can start by using in our reasoning any concept whose determinacy has not been secured, a determinacy that can be later either confirmed, or undermined, by further analysis or further discoveries; otherwise we could never start at all. This is undoubtedly true. But it does not save the proposal from the circularity of appealing to physics, whose objectivity presupposes the objectivity of mathematics.

The trouble with the definition is not the need for initial determinacy, but the need for intelligibility: the Archimedean hypothesis is simply meaningless, unless we make full use of the mathematical distinction between finite and not finite. Any indeterminacy and

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[23] Accounts of this type are quoted and ridiculed by Frege (1884). See, for example, Schröder's "Axiom of Symbolic Stability" p. xx in Austin's translation.

any unclarity of that distinction is fully inherited by the Archimedean hypothesis. The two remain locked together by the very definition of 'Archimedean'. The concept *Archimedean* is mathematical; it can be applied to ordered sets with a metric (actually something weaker than a metric is sufficient). And it is defined in terms of 'finite'. Given an infinite ordered set, which is Archimedean, we can define, using it as a fixed parameter (and using one-to-one embeddings), the mathematical concept of *finite*. This is all pure mathematics. Field assumes that time, or events ordered by time, give us such an Archimedean ordered set, and he bases on it the concept of *finite*. This is as circular as it can be. Part of the circle is provided with a physical modeling, but this makes no difference to the circularity involved in securing the objectivity of mathematics by appeal to a physical hypothesis whose objective meaning presupposes the objectivity of mathematics.

Note that it is possible to discover that time is not Archimedean; that is, to get experimental results that will justify the choosing of a non-Archimedean model in our physical theory. But this possibility makes no sense without the mathematical finite/not-finite distinction.

A physicalistic view may incline one to identify "being objective" with that which can be defined in physical terms. If the point of the proposal is to achieve this type of objectivity, then it achieves it by a mere formal trick: a mathematical concept is defined in terms of itself, in a closed loop that has been partly "physicalized."

One final point: if we can use the concept of finiteness in the physical modeling (as the proposal does) then we do not need the Archimedean hypothesis. For we can separate between "Archimedean" and "non-Archimedean" points, and then use only the former: Fix some event e_1. Call any later event Archimedean (relative to e_1) if it is not the end event of any infinite template whose first member is e_1. Now proceed as before using only Archimedean events.

5.2. Hilbert's modeling of formal systems and of natural numbers. In Hilbert (1904)—his earliest outline of formal systems—he speaks of '1' and '=' as simple "thought objects," whose combinations are "thought objects" as well. The "combinations" are parsed strings, where the parsing is indicated by parentheses (the parentheses are *not* thought objects). For example:

$$((11)(1)(=))(==)$$

This is essentially a finite labeled tree—a common structure of formal languages and a basic tool in linguistics. In Hilbert (1925) only the finite strings of 1's remain and the story shifts to the elimination of actual infinity. When he speaks of formal systems he speaks and gives examples of 'signs', which 'serve to convey information' or 'to make assertions' without further analysis. Only in the case of natural numbers do we get something more to guide us: a picture of an endless sequence of strings: 1, 11, 111,..., *and so on*.

Hilbert's finite strings appear "concrete," but their forming an unending sequence undermines their concreteness. We might say that the sequence represents the very "essence" of natural numbers—the possibility of *repeating an act an indefinite number of times* (the act here is: to add 1). Now the distinction between the potential and the actual infinite had been well known since the time of Aristotle, if not earlier. Hilbert could hope to avoid actual infinities by regarding them instrumentally. But this was the most he could hope, since to abolish the potential infinite is to abolish the natural numbers themselves, and also to abolish the potentially infinite collection of sentences generated in the formal language. Besides, a rejection of potential infinity seems to imply the existence of a largest natural

number. But if n is that number, what is to prevent one from going on to $n+1$? (This issue has been addressed in the section on ultrafinitism.)

§6. Realism in mathematics.   Let me recap: a realist attitude is a belief that certain questions have objectively true answers, which are independent of our abilities to know them. I toss a fair coin and, without noting the side it landed, I toss it again. Nobody knows and nobody will be able to know the outcome of the first toss. My act has "created" a fact beyond knowledge, namely, that the coin landed X (where X is 'heads' or 'tails'). Such irretrievable little pieces of information are a matter of daily occurrence (there is no need to imagine the proverbial falling of the tree in the remote forest). Yet people have no problem in believing that there is a true answer to the question "which side did the coin land?" And the belief is not based on sophisticated speculations on the possibilities of retrieving information from minute changes in the state of the world. Of course, the event is specified by using a rich conceptual apparatus that is sufficiently well understood, which covers "coin tosses," "landing of coins," and all the background knowledge of everyday physics. The inaccessibility of the fact is easily explained by appeal to physical factors (the enormous size and complexity of the physical world, our limited access, etc.).

Since mathematical practice is not physical in the way coin tossing is, and there is no physical metaphor to appeal to—no kicking of stones, no causal chains, and since, moreover, mathematics is supposed to be a discipline of "pure reason"—the problem of realism in mathematics is more difficult.

In my view mathematics is concerned with modes of organization, by which we organize our world and our own activities in it. (Presumably this is essentially a Kantian view, but I am not concerned here with the question of interpreting Kant). Mathematical reasoning derives from a capacity to recognize these modes as *generic structures*, and here I use "generic structure" broadly, as a primitive term, which can be clarified and analyzed, using the many examples that mathematical history provides, but which cannot be defined. It is a big subject that merits considerable treatment by itself; I cannot go into it here, and for the purpose of the present discussion I do not need to (I hope to address it elsewhere).

Skepticism about a mathematical theory always derives from suspicion that the theory involves unclear grasping, doubtful analogies, and error-creating thinking. And there is no shortage of examples to show that illusions and errors do occur. It is for this very reason that the methodology of validating mathematical claims by proofs is necessary. Proofs are needed not only as arbiters between conflicting claims in the mathematical community. They are also needed by the single mathematician in order to check the correctness of a "proof" in his or her head; it is even needed to organize one's thinking and to clarify one's own basic idea to oneself. Of course, the proofs are formalized only to the extent that is needed to settle the issue. But the formalization can be carried out all the way down to the level of a proof in a formal language.

The major debates at the turn of the last century involved logicists, who subscribed, in this way or other, to set theory with all its infinities, and deniers who saw it as a confusion based on misleading analogies;[24] some of the latter also rejected the use of classical logic

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[24] Russell's "no class" theory has no problem with a set-theoretic approach; it legitimizes it by translating class talk into propositional-function talk.

in nonfinite domains. Hilbert, who recognized the beauty and fruitfulness of set theory, intended to defuse the issue by showing that set theory is safe. To this he added the claim that in mathematics everything can be known:

> ... After all, one of the things that attract us most when we apply ourselves to a mathematical problem is precisely that within us we always hear the call: here is the problem, search for the solution; you can find it by pure thought, for in mathematics there is no *ignorabimus* ["we will not know"] Now, to be sure, my proof theory cannot specify a general method for solving every mathematical problem; that does not exist. But the demonstration that the assumption of the solvability of every mathematical problem is consistent falls entirely within the scope of our theory. (Hilbert, 1925)

Here "method" and "solvability" are rather vague. But the gist of Hilbert's claim is obvious: to abolish, in principle, the gap between mathematical truth and its knowability. Under 'problem' Hilbert included set-theoretic problems, such as CH (the continuum hypothesis), the first on his problem list from 1900. The second part of Hilbert (1925) is devoted to an outline of a (mistaken) proof of CH. The question posed by CH is arguably the most natural question of set theory; for it is about the size of the first gap between familiar infinities: that of the natural numbers and that of the reals. Hilbert believed that questions that arise in mathematical practice can "in principle" be solved; if the mathematicians work hard enough, the conjecture will be either proven or refuted. If also the desired consistency proof of Hilbert's program is achieved, the solutions will provide unique answers (for conflicting answers will imply a contradiction).[25] The mathematician could then apply 'true' and 'false' across the board: conjectures arising in mathematical practice are true if they can be proven, false—if they can be refuted, and it is either the one or the other. The appeal to finitist reasoning is needed in order to guarantee consistency and base mathematics on firm foundations. But once this is done, the problem of mathematical realism becomes moot.[26]

Gödel's incompleteness results upturned Hilbert's view on both counts, that of safeness and that of knowability: No, you can never be safe, except by appealing to a stronger theory whose safety is, again, not guaranteed. And no, there will be always unanswerable questions within your formal framework. The first point is a clear and uncontroversial consequence of the second incompleteness theorem. The issue of realism has to do with the second point: the existence of undecidable statements (those that can be neither proven nor refuted within a given formal theory). The statements are undecidable provided that the theory is consistent, and although the consistency cannot be derived in the theory itself (if the theory is indeed consistent), we can, for the purpose of our analysis, take the

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[25] Hilbert did not specify precisely the system whose consistency is to be established. Van Heijenoort observes (footnote of p. 383) that by "consistency of arithmetic axioms" Hilbert sometimes meant the axioms for the real numbers and sometimes the axioms for arithmetic. But obviously, the general project would be to prove by finitist methods the consistency of any system that we use. To retain Cantor's paradise "[It] is necessary to make inferences everywhere as reliable as they are in ordinary elementary number theory, which no one questions and in which contradictions and paradoxes arise only through our carelessness" (Hilbert, 1925, p. 376).

[26] The point is somewhat a kin to Quine's observation, quoted in Section 3.1, that ontology is "internally indifferent" in the case of a complete decidable theory.

consistency of our adopted theory for granted (see the end of Section 2.5 for a discussion of the grounds for the consistency assumption).

But the implications of the undecidability results require further analysis. At the beginning of the section I have argued that mathematical knowledge requires proofs, which can be fully formalized as proofs in some formal theory (I shall elaborate on it in Section 6.1) Therefore, as long as we are working within a given formal theory, no matter how powerful, there will be either unknowable truths or statement that have no objective truth value. One might still imagine that by switching repeatedly to stronger theories, mathematicians could "in principle" solve any given problem. I shall argue in Section 6.2 that we should not lay too much store on this speculation.

How does this relate to realism? If an independence result indicates that some mathematical truths outstrip our capacities of knowing them, then it points to knowledge-transcendent truth, hence to realism. But an independence result can cut both ways, and sometimes it cuts the other way: this or that is unknowable because there is nothing to know. What an independence result implies is quite sensitive to the way it has been proven and its place in the overall theory. In set theory independence results are proven semantically—by constructing well-founded models of ZFC in which the independent statement has different truth values. A realist will have to rule out some of these interpretations (or all of them) as "deviant," that is, not in accordance with the concept of *set*. The issue has been shortly discussed in Section 2.4.

By contrast, Gödel's undecidability results concern arithmetical statements, and their proofs do not involve different interpretations of formal arithmetic. If one's realism with regard to arithmetic statements derives from one's conception of the standard model, Gödel's results should not undermine it. They should, on the contrary, force one to take one's realism seriously. They should also force us to consider seriously the possibility of mathematical truth beyond human knowledge (I hope to make this clear in Sections 6.1 and 6.2.) Yet the possibility of the radical scenario (RS), described in Section 2.4, has worried some logicians. For example, Franzén (2006) tries to reassure us that we should not infer by any means that the conjecture in question is unknowable.

6.1. The implications of Gödel's results.   Gödel's theorem, and the flood of undecidability results that followed, derive their significance from the following two facts: Mathematical statements are validated by deriving them from axioms, using some logical machinery. And second, any mathematical theory can, in principle, be fully formalized so that informal proofs are translated into formal ones; and the latter are well-defined finite constructs whose correctness can be effectively checked. Note that this also applies to computer-aided proofs, since computations, as entities generated by mathematical machines, can be accommodated within the general framework of proofs. It requires a language that incorporates some model of computation, for example, a version of the λ calculus, or a language that includes terms and rules for handling inductive definitions.[27] For simplicity I shall not pursue this possibility further and assume some traditional notion of a deductive system, for example, a Hilbert-type system. Nothing hinges on this restriction.

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[27] The actual use of computers relies however on physics; our confidence that the mathematical results are valid stems from our confidence that the physical computer realizes the mathematical machine. The same goes for probabilistic algorithms, which can be seen as proving statements of the form "ϕ holds with probability $\geq p$" and which rely on randomization techniques.

Now the very notion of *formal proof* implies that we have an effective way to verify that an alleged proof is indeed a proof—a way of checking each step. This means that the set of theorems of a given theory is *computably enumerable* (c.e.) — also known as *recursively enumerable* (r.e.). Let T be any theory, such that a sufficient fragment of PA (Peano Arithmetic) is derivable in it. Then, using the fixed-point technique, we can construct a sentence, such that, if T is consistent, neither the sentence nor its negation are provable in it.[28] In accordance with Gödel's terminology, call such sentences *undecidable* in T.[29] These sentences are also known as *independent* of T.

While the original use of the fixed point technique yields particular sentences that are undecidable, the results imply that there is an infinite number of undecidable Π_1 sentences, and there is no effective way of recognizing them.[30] There are natural ways of strengthening T, which can be used repeatedly, leading to stronger and stronger theories. In these theories, some previously undecidable sentences can be decided. But no matter how we get new theories; as long as the set of theorems is c.e., and the theory is consistent, we will find ourselves in the situation just described.

These are very robust results pertaining to any formal system of certain minimal strength (PA is considerably more than we need). The claims and their proofs do not address semantic issues; they are concerned only with the deductive formalism.[31] The undecidable sentences are about numbers but, as remarked in Section 2, syntactic items and numbers are birds of a feather. Having established the incompleteness result, we can view the situation semantically, using the concept of *truth*.[32] It will then follow that, for each undecidable pair, ϕ, $\neg\phi$, one of the two is true but unprovable; hence the gap between provability and truth. And since, in mathematics, proofs—which in principle can be formalized—are required in order to certify knowledge, we get also a gap between *knowability* and truth. This line of thought explains the gap as a result of our epistemic limitations. Stated in full it is the following: *Mathematical knowledge requires certification by proofs, which, in principle, can be formalized, so that we can effectively test if a proof is correct. But formalized proofs have their price; they leave many statements undecidable.* A fuller explanation should address the basic epistemic questions, how we come to know mathematical truths and the role of proofs in the process, which are beyond the scope of this paper.

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[28] The technique of constructing self-referential sentences underlies Gödel's original proof. Here we apply it to Rosser's variant. Actually Gödel did not state the fixed-point theorem. The first to infer the general theorem from Gödel's (1931) work was Carnap (1934)—a fact not generally known. Gödel (1934) credits him with it.

[29] Gödel referred to them as *formally undecidable propositions* [Sätze]. ('Undecidable' is currently used ambiguously; 'undecidable theory' means a theory for which there is no decision procedure.)

[30] The set of these sentences is not computable. Otherwise we could partition the Π_1 sentences into three computable sets: the undecidable, the provable, and the refutable. (This is done as follows: given a Π_1 sentence ϕ, first compute and find if ϕ is undecidable; if it is not, try in parallel to prove ϕ and to prove its negation.) It is known however that the set of Π_1 sentences that are derivable in T is not computable, and similarly for the refutable ones. This holds also for Σ_1 sentences.

[31] Arguably, ω-consistency, which turns up in Gödel's original theorem introduces a certain semantic aspect. I do not think it matters to the point I am making here. In any case, Rosser's variant eliminates even this semantic vestige.

[32] Historically Gödel arrived at his theorem probably by going from the semantics to the formal system: from the Liar and Richard's paradox to the syntactic analogy; see Gaifman (2006). The heuristics of the discovery does not affect my point. He was fully aware of the importance of removing semantic elements from the proof.

The explanation of the gap is further supported by noting that limitations on computational power give rise to similar gaps in finitary situations. Let PRS be Presburger Arithmetic, the theory of natural numbers under addition. As noted in Section 3.1 this theory is decidable: there is an algorithm that, given any sentence ϕ in the language of PRS, answers correctly the question, is ϕ true (in the standard model)? But any such algorithm will in general take no less than $2^{2^{c \cdot n}}$ steps to decide whether a given sentence of length n is a true; here $c > 0$ is some constant. This result has been proved in a ground breaking work by Fischer & Rabin (1974). Its philosophical interest lies in the fact that the proof uses diagonalization, which amounts to scaling down Gödel's technique to the finitary case; it can be seen as a construction of a sentence that says, "I am not decidable by less than $2^{2^{c \cdot n}}$ steps." The technical work consists in showing how very long computations can be described by much shorter sentences of PRS.

The algorithms in question are nondeterministic, that is, the computation allows nondeterministic choice of steps, like the choice of steps in a proof. Proof construction is indeed a particular case of nondeterministic algorithms. Using this fact, Fischer and Rabin restate their result in terms of proofs. The following is a slight reformulation of their statement. Here a set of axioms for PRS is any set of sentences, such that the true sentences of the language are exactly those provable (in first-order logic) from that set.

> Let "axiomatization of PRS" mean any set, AX, of axioms for PRS, for which there is a polynomial-time algorithm for recognizing if a given sentence is in AX. Then there is a constant $c > 0$, such that for every axiomatization, AX, of PRS there is an integer n_0, so that for every $n > n_0$ there is a theorem of PRS of length n whose shortest proof from AX is longer than $2^{2^{c \cdot n}}$.

Surely, there is no mystery in super-exponentially long proofs (unless you are an ultra-finitist). But if our resources restrict us to less than super-exponential computation, then some truths must remain unknown.

The incompleteness results show that realism is for real. The issue cannot be bypassed as Hilbert once hoped. These results are about arithmetical statements, and their proofs do not give us any reason we did not have before to suspect that there is something wrong with our conception of the standard model of natural numbers. Hence they force those who rely on that conception (the great majority of mathematicians) to take realism seriously, that is, to accept, *at least as a serious possibility*, that the answers to some simple mathematical questions—for example, that some Diophantine equations have no solution—are beyond what we can know; and this is due to our epistemic limitations. When it comes to physical events such a possibility is easily acceptable, for we can see how our epistemic limited resources can give rise to it. Also in mathematics we have an explanation that appeals to epistemic limitation—our need to use proofs, which form a c.e. set. Yet in mathematics, which is devoid of any contingency and which is pictured as the "fruit of human intellect," the situation may appear strange.

6.2. Unknowability and its price.   It is often observed that the original Gödel sentence, which "says of itself" that it is unprovable in T, is indeed not provable, assuming, as we do, that T is consistent. (Here T is our presupposed strong enough formal system.) The Gödel sentence can be equivalently written as a Π_1 sentence (and so can the Rosser sentence), and the observation just made generalizes to all such sentences: an undecidable Π_1 sentence

must be true; because a false Π_1 sentence is refutable in T.[33] Hence, if we have proven that the consistency of T implies the undecidability of a Π_1 sentence, then we know that the sentence is true, in as much as we know that T is consistent. It is not the same as proving the sentence in T but it is knowledge enough. This is the reason that, for any particular Π_1 sentence A, a proof that A is independent (in some acceptable theory) reveals its truth.

In the radical scenario (RS) of Section 2.4 we chose a Π_2 sentence; we could not have chosen a sentence on the first level.

We can extend the above line of thought further. Let Con(T) be the statement that T is consistent, and define:

$$\mathrm{Con}^{(1)}(T) = \mathrm{Con}(T),\ \mathrm{Con}^{(2)}(T) = \mathrm{Con}(T + \mathrm{Con}^{(1)}(T))),\ \ldots,$$
$$\mathrm{Con}^{(n+1)}(T) = \mathrm{Con}(T + \mathrm{Con}^{(n)}(T)),\ \ldots$$

Say that A is derivable from *plausible consistency assumptions* if for some finite n it is derivable from $\mathrm{Con}^{(n)}(T)$. We might recognize that A is derivable from plausible consistency assumption, without having to use formally a stronger theory.[34] We may then argue that as long the Π_1 sentence is derivable from plausible consistency assumption we know that it is true. This consolation is short lived however. Using a more sophisticated application of the fixed-point technique, we can construct a sentence A (which is not a Π_1 sentence) that refers to itself and involves also an additional sentence C, so that the consistency of T implies that A is undecidable; but, assuming $\mathrm{Con}^{(2)}(T)$, the truth value of A is the same as that of C. Choosing as C an open conjecture, we get an undecidable sentence (assuming that T is consistent), whose truth value is unknown; finding A's truth-value is equivalent to proving or disproving C.[35] Of course, we do not know that A's truth-value is *unknowable*, since we do not know that C's truth-value is unknowable.

The more troubling implication of Gödel's result is not a particular case we know to be undecidable, but an uncertainty that affects *any* open conjecture—the uncertainty that the conjecture may, for all we know, be undecidable in the background theory whose resources we employ. We know for sure that, if the theory is consistent, there are an infinite number of undecidable Π_1-sentences, which we cannot effectively recognize. More sophisticated uses of the fixed-point technique yield undecidable sentences at higher levels of the arithmetic hierarchy, but considerations of Π_1 sentences are sufficient to make the present point. These statements include several of the famous number-theoretic open conjectures, for example, the Goldbach conjecture (every even number $>$ 2 is the sum of two primes), and

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[33] That $\forall x_1,\ldots,x_n \phi(x_1,\ldots,x_n)$ is false means that $\phi(x_1,\ldots,x_n)$ is false for some instantiation of the x_i's. If ϕ is bounded, the instantiation yields a bounded sentence. Such a sentence is convertible into an equivalent quantifier-free sentence, by replacing the parts governed by bounded quantifiers by finite conjunctions and disjunctions; all these moves can be done working within our theory T. It is easily seen that if a quantifier free sentence is false its negation is provable in T. Hence if $\forall x_1,\ldots,x_n\ \phi(x_1,\ldots,x_n)$ is false, then, for some numerals $a_1,\ldots,a_n$, $T \vdash \neg\phi(a_1,\ldots,a_n)$; hence $T \vdash \neg\forall x_1,\ldots,x_n\ \phi(x_1,\ldots,x_n)$.

[34] We can continue by defining $\mathrm{Con}^{(\omega)}(T)$ as the union of all preceding theories and go on. As long as we are going through recursive ordinals we get c.e. theories. But then we would do better to switch to the formal stronger theory. The point of the definition is to provide some margins of recognition without having to make the formal switch.

[35] Such a construction was posted by Torkel Franzén on the FOM network ("Re: Question on hard core independence," November 1, 2003), in a discussion initiated by me under the name "hard core independence." Independently, Harvey Friedman emailed me another construction that achieves this effect.

the conjecture that all perfect numbers are even (where a number is perfect, just when it is equal to the sum of all its smaller divisors, e.g., 28); the Riemann hypotheses, usually stated in terms of the Riemann function, is equivalent to a Π_1 statement. And, as noted, any Π_1 sentence is equivalent in PA to a universal sentence stating that a Diophantine equation has no solutions.

The stronger the theory T is, the more significant is the fact that a statement is undecidable in it. Right now the natural theory of choice is ZFC. For it turns out that ZFC is more than sufficient for the purpose of formalizing proofs in the nonlogic areas of mathematics, as well as many parts of mathematical logic; using a rough safe guess, this should cover 99% of mathematical activity. It is the reason that undecidability in ZFC is taken to mark the unsolvability of the problem in common mathematical practice. As noted, any theory, T, can be systematically strengthened to T+Con(T) and so on. In the case of ZFC such extensions are minute compared with the very strong extensions that have been intensively investigated in the last 60 years. But as long as the theorems of our theory form a c.e. set, there will be an infinite set of undecidable Π_1 statements, hence unknowable truths at that stage.

Does it mean that certain mathematical truths are "unknowable by humans"? The question has bothered some logicians and philosophers including, most notably, Gödel (1951). As long as mathematicians are working in a system with a c.e. set of theorems the answer is yes. We may consider however a process of repeated extensions, say by adding new axioms, which in the limit will cover all mathematical truths. And we may hypothesize that an idealized mathematician, or an idealized community, can "in principle" converge to such a limit. The more one reflects on this the more it becomes less clear what the hypothesis is. Is it a conjecture on what the human brain, in its present capacity, can do? The alternative hypothesis, namely that the brain is constrained to produce some c.e. sequence of theories—though we may never identify the very complex algorithm that does it (a possibility discussed in (Gaifman, 2000))—seems more plausible. Or is it a conjecture on possible human evolution? I think that we will do better to focus on the present state and the foreseeable future. And we would do better to admit, at least as a serious possibility, that some Π_1 sentences, including possibly some simple conjectures, or some simple arithmetical conjectures on the higher level of the hierarchy, are "in principle" sealed for humans.

That fact has some uncomfortable implications for mathematical practice. No one likes to be in a situation of trying to do the impossible—not impossible as in "mission impossible," but impossible mathematically. Yet mathematicians must take risks. One may try to prove a conjecture, when the conjecture is in fact refutable—this is ordinary bad luck. Hopefully, when some people try to prove and others try to refute, the issue eventually will be decided. Sometimes the same person tries both ways. Much worse is the case where all these efforts must fail, because the conjecture is undecidable in the background theory. This is extraordinary or metabad luck. Hilbert's "no *ignorabimus*" thesis has powerful motivation, and the passage quoted at the beginning of Section 6 makes this motivation explicit. No wonder that the vast majority of mathematicians hate to entertain the possibility that the problem they work on has no solution because it is independent of ZFC. I once heard Paul Erdös say in a lecture on number theory, "hopefully this kind of problem does not fall prey to the monster of independence."

Others are more hopeful. They believe that there is no extraordinary bad luck, or that there should not be, if one is careful enough to choose the right problems in one's area of research. On this view, a mathematical research area generates its own *natural*

questions, and in certain areas not directly related to set theory such questions will not fall prey to independence. There have been some notable counterexamples, such as Whitehead's problem in group theory—a well known question posed by Whitehead in 1950 which was proved by Shelah in 1974 to be independent of ZFC; or Kaplansky's conjecture on Banach algebras, whose independence follows from the works of Solovay, Woodin, and others. There have been independence results in other areas, but not enough to undermine the natural-question view. The independence, in all these cases is independence of ZFC, and it derives from the fact that set theory can have different interpretations. The natural-question view is a position about the relation of formalized set theory (the combination of formal logic with set theory) to other mathematical disciplines. On the whole it is folklore (and a powerful one), which is very common among mathematicians who are usually, but not always, nonlogicians.[36] It goes without saying that the notion involves inevitable vagueness, yet it leads to a basic problem concerning the interplay of ontology and epistemology.

6.3. On Nonnatural questions in number theory. Consider the sentence Con(ZFC), which says that ZFC is consistent, where this is expressed in first-order arithmetic, via Gödel numbering. Con(ZFC) is a true Π_1 sentence (assuming of course that ZFC is consistent), which is unprovable in ZFC, due to Gödel's second incompleteness theorem. The Π_1 statement can be reduced to a claim that a certain Diophantine equation has no solutions. This independence is purely syntactic, it is obtained solely through Gödel's techniques. But the consistency of ZFC follows from the set-theoretic axiom that there is a strongly inaccessible cardinal.[37] Call the axiom IN. Then the claim about the Diophantine equation is a theorem of ZFC+IN, but not of ZFC. In this way it becomes an independence result that derives from a different interpretation of set theory—one that restricts the universe to V_α, where α is the first strongly inaccessible cardinal; in *that* universe IN fails. (Note however that IN is needed only to give us Con(ZFC) and that the second incompleteness theorem does all the work.)

The relevance of additional set theoretic axioms to number theoretic statements involving Diophantine equations was noted first (as far as I know) by Gödel (1951, p. 307). Apparently he gave it considerable thought.[38]

The Diophantine equation whose unsatisfiability is equivalent to Con(ZFC) is quite long but still manageable. One can obtain it as follows. The Gödel numbers of the theorems of ZFC form a c.e. set. It is easy, though tedious, to write a program that outputs one after another the members of this set. By applying the techniques of Diophantine sets, which

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[36] Angus Macintyre argued for the natural question view in a lecture he gave at Cuny several years ago and, in particular, in an exchange I had with him after the lecture. He quoted Atiyah as saying that a certain conjecture cannot be formalized in set theory. As Macintyre glossed it—"it cannot be formalized as a living question." Some such position is implied by *Bourbaki*, but *Bourbaki* is an ideological package that contains also many other things.

[37] If α is a strongly inaccessible cardinal, then V_α is a model of ZFC.

[38] Gödel uses a Π_2 sentence in which the quantifier free part is a Diophantine equality. He observes that for each of the new set theoretic axioms (of which he spoke in his lecture) there is a Diophantine polynomial $P(\vec{x}, \vec{y})$, such that $\forall \vec{x} \exists \vec{y} (P(\vec{x}, \vec{y}) = 0)$ is decidable via this axiom, but is undecidable if the axiom is dropped. He goes so far as to estimate that the degree of P need not be higher than 4. Later Tarski used the above observation about the existence of strongly inaccessible cardinals to point out to mathematicians (in oral exchanges) the relevance of large cardinal axioms to number theory. He used a Diophantine equation that involved variables as exponents; this was before the Matiyasevich proof.

have been developed since Matiyasevich (1970), we get a polynomial with integer coefficient, $q(y, x_1, \ldots, x_n)$, such that $\{y : \exists x_1, \ldots, x_n \, q(y, x_1, \ldots, x_n) = 0\}$ is the set of outputs of the program. Let k be the Gödel number of '0=1', and let $p(x_1, \ldots, x_n) = q(\text{k}, x_1, \ldots, x_n)$, then:

$$(\dagger) \quad \text{Con(ZFC)} \Leftrightarrow \neg \, \exists x_1, \ldots, x_n \, (p(x_1, \ldots, x_n) = 0)$$

Some results in the area of Diophantine sets imply that n, the number of unknowns in the equation, need not exceed 11. We can also get simpler expressions by allowing a system of equations (i.e., a finite conjunction of equations) instead of a single one. Our system of equations should be comparable in length to systems that appear in the study of Diophantine sets—an area that falls within the framework of general number theory, as it is usually conceived in mathematics.

The question "are there solutions to $p(x_1, \ldots, x_n) = 0$?" where p is the polynomial in ($\dagger$), is *not* a natural number theoretic question for the following reason. The way of getting p makes it extremely sensitive to the fine details of the Gödel numbering, many of which are arbitrary. The significance of the equation can be recognized only by recognizing that the negated existential statement encodes the claim that ZFC is consistent. As an *equation* it plays no role in number theory, or any other nonlogic area of mathematics. And in logic its significance comes only from its encoded linkage with the consistency claim.

It goes without saying that questions whether this or that mathematical question is "natural" need not always have clear cut answers, and that the answers may vary with time, according to the shifting interests in mathematical communities. As an example, consider the further development of the idea of getting number theoretic results via additional set theoretic axioms, which was already indicated by Gödel (1951) (see Footnote 38). A straightforward application of relative consistency claims, Con(ZFC+A) $\Rightarrow$ Con(ZFC+B), where A and B are large cardinal axioms, yields the corresponding implication between the nonexistence of solutions of the corresponding equations—but this would not be a candidate for a natural result. Harvey Friedman refined and developed the strategy, in order to get a series of more interesting theorems about natural numbers (under additional systems of functions); they follow from some large cardinal axioms, but do not follow in ZFC if the axioms are omitted. By now he has developed a considerable body of research, under the title Boolean Relation Theory, or BRT. Do the theorems of BRT answer natural questions of number theory (in the broad sense used by mathematicians)? The answer might be in the eye of the beholder, one who should also have expertise in BRT and number theory, which I do not.

The fact remains that the questions they answer have not been previously stated in number theory, or for that matter, in mathematics. The appeal to what has been published does not settle the question. Some problems that have been published are no longer regarded as natural (e.g., Whitehead's conjecture). And sometime the posing of a new conjecture is by itself an important step that advances the area. Yet, it seems to me that BRT is better described as setting up a new area that studies certain number-based structures, which is guided by large cardinal axioms. It does not yield independence results for known number theoretic questions. But post factum, it may constitute a new area with its own interesting questions.[39]

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[39] Allegedly, some number theorists, who had some theorems explained to them, agreed that the questions are natural.

6.4. Ontology versus epistemic organization. The problem of natural questions has to do with the distinction between mathematical ontology and the epistemic organization of mathematical material. This is best explained with the help of an elementary example. Consider $\mathbb{R}$, the field of reals, and its extension to $\mathbb{C}$, the field of all complex numbers, $x + y \cdot \sqrt{-1}$, x, $y \in \mathbb{R}$. Initially the imaginary numbers, $y \cdot \sqrt{-1}$, were considered "non existing" entities (as their name indicates); they were mere devices for facilitating the computation. Only in the nineteenth century did they acquire first citizen status. As far as the ontology is concerned the difference between $\mathbb{R}$ and $\mathbb{C}$ is trivial, for we can regard the complex numbers as pairs of reals, (x, y), and define the arithmetical operations in the well known way:

$$
\begin{aligned}
(x, y) + (x', y') &=_{DF} (x + x', y + y') \\
(x, y) \cdot (x', y') &=_{DF} (x \cdot x' - y \cdot y', x \cdot y' + x' \cdot y)
\end{aligned}
$$

The old real numbers now appear as pairs of the form $(x, 0)$. Since we have multiplication for complex numbers we can define z^n, where $z \in \mathbb{C}$, as iterated multiplication; using standard power series methods of analysis, we can extend other functions from the reals to the complex numbers. Every statement about complex numbers has an obvious translation into a statement about the reals. And vice versa, every statement about the reals can be translated into a statement about the complex numbers—one that is relativized to a subdomain consisting of all pairs $(x, 0)$. The translation extends to richer structures, obtained by adding a metric over the complex numbers, notions of continuity, differentiability, and so on and so forth. If the translated statement is true, or false, or problematic, so is the original one. There is no ontological increase in the extension of $\mathbb{R}$ to $\mathbb{C}$.

But real analysis and complex analysis, though related, are very different disciplines with different lines of research, and different natural questions. The first is guided by the total ordering of the real line, the second—by the topology of the plane, and, in particular, by curves and planar regions. $\mathbb{C}$ is not merely $\mathbb{R}^2$; one crucial difference is that in $\mathbb{C}$ we have multiplication, which makes it a field, and in $\mathbb{R}^2$ we do not. The study of real valued functions over Cartesian products, $\mathbb{R}^k$, which naturally leads to functional analysis, is altogether different from complex analysis—the theory of functions defined over $\mathbb{C}^k$. Yet, *ontologically* speaking, that is—as far as questions of factual truth are concerned— the systems are translatable into each other. There is no ontological difference.

Retrospectively we can see that the big advances in mathematics have been mostly in epistemic organization. For example, the following systems are all intertranslatable. Statements made in any of them have easy translations into statements made in another.

   (i) The positive integers, under $+, \cdot$, and the partially defined subtraction and division,

  (ii) The natural numbers, obtained by adding 0,

 (iii) The integers, with totally defined subtraction,

 (iv) The rational numbers, with total division except for division by 0.

Yet the move from (i) to (ii), that is, the introduction of 0, is a major advance in mathematical practice. And so is the move from (ii) to (iii), that is, the introduction of negative numbers, and the move from (iii) to (iv). To be sure, this is not a faithful historical picture; the actual history is a twisted affair. But one can see how major advances correspond with moves that, post factum, organize the material in a different way, preserving the same ontology. The big ontological jump is from the rational numbers to the reals, that is, from first-order theories to second order theories.

And then came set theory. In principle, we can translate statements of the numerous disciplines and subdisciplines of mainstream mathematics into a language based on one

binary relation, ∈, and we can derive the translation of any theorem from the axioms of ZFC. It is also overall accepted that ZFC is consistent and that derivations in ZFC are legitimate. By "main stream" I except set theory itself in its different variants. Think of differential geometry as an example.

We can see how the translation will go, because we know how to model bottom-up various structures within set theory: starting from ordered pairs, natural numbers, real numbers, Cartesian products, groups, functions, operators, operator spaces; the whole gamut. And we are sufficiently convinced that the translated theorems are provable in ZFC for similar reasons: we are acquainted with the informal proofs employed in mathematics, the basic steps and the combining of shorter steps into longer ones, to be sufficiently convinced that the informal proof can be first converted into an informal proof in ZFC, which can be then fully formalized. There is of course a lot of "in principle" in all this, and some hand waving. But it is not mere speculation; it is the outcome of half a century of research in logic, set theory, and formal systems.

For someone working in differential geometry, that translation, or the possibility of it, is completely useless. Even the initial steps of the modeling destroy the basic organization that gives the mathematician a grasp of the subject. The translation renders the statements practically meaningless. It is in this sense that the reported saying of Atiyah, about a statement that cannot be formalized in set theory, can be understood (see Footnote 36).

The mathematician also trusts his or her expert grasp of structures and of their interrelations—an understanding that is based on natural talent developed by intensive training. This is not mere intuition or "feel." Mathematicians adheres to full rigor, taking trouble to check carefully the steps of a proof (an informal one), and breaking long proofs into smaller components in case of doubt. Taking a de-facto realistic attitude—itself derived from expert understanding of the relevant structures—the mathematical expert in a field is confident that a natural conjecture has a yes-or-no answer. A set theoretic independence result will imply that the mathematician's understanding is flawed: the structures can be crucially different in different models of set theory. To settle the question, he or she may try to find what the "right" set theoretic model is, but this is not a welcome proposition to a differential geometer.

Yet no working mathematician can ignore an independence result in the area he or she works. If, and this is quite hypothetical, a conjecture in differential geometry were proven to be independent of ZFC, mathematicians will stop working on it (they will cease their attempts to prove it, and they will cease their attempts to refute it). They would be foolish not to. And unless they are interested in set theory, their reaction would be to declare that the problem was not the correct one.[40]

6.5. Gritty realism, you cannot "find it by pure thought."   Hilbert's "you can find it by pure thought" meets another big obstacle, besides that of independence results: the necessity to rely on computers. Many long computations are, in principle, long mathematical derivations, consisting of a large number of little repetitive steps. Each step is obvious, and sometimes the whole computation can be done on paper; but, as a rule, it is beyond human capacity. Some computations have depth and complexity of their own, an organization that requires high expertise in programming. And some also require an additional level of computer work: a verifier that checks that the original program satisfies the necessary conditions which are written in a higher level specification language.

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[40] I am relying here on personal knowledge and anecdotal evidence.

In applied mathematics the use of computers is an old story, and it is also an old story when it comes to discovering large primes and other long range numeric patterns.[41] Some number theorists and some mathematicians working in combinatorics consult routinely computer outputs, as a guide in their research. A more dramatic entrance of computations into pure mathematics took place in 1976, when Appel and Haken proved that every planar map is colorable by four colors; this confirmed a famous graph theoretic conjecture stated more than a hundred years earlier. The proof made heavy use of computers, which rendered part of it inaccessible to human inspection. In the mathematics community it was resisted on two grounds: some wanted to preserve the existing standards of proof (call this the purists' objection), and some were concerned about possible mistakes due to the handling of many details and possible programming errors (call this the pragmatic objection). Eventually the result was accepted; it was supported by a computer-aided proof from 1995 which was more transparent, and by an interactive theorem prover—which reduced the likeliness of programming error.[42] It appears that in graph theory such proofs will become part of the norm. The purists lost and the pragmatists got sufficient assurance.

The four-color problem is undoubtedly a natural problem and the use of computers—which renders parts of the proof opaque to humans—poses a prima facie problem for any view that grounds mathematical knowledge on mathematical understanding of structures. In fact, the explanation in terms of human understanding is at hand. Computer-aided proofs are accepted by graph theorists because they understand the main claims of the proof, concerning graphs, and the proof's general structure, and also because they have some general understanding of algorithms and how a computer works; they may also have some programming experience. In addition they trust the programmers, who have their own understanding of how the program works, and they trust the physical computers. Putting aside the last item, we can see that here understanding involves two kinds of mathematical structures—those of the subject matter (the graph theory, the problem itself), and those of the "meta-subject" (algorithms, mathematical machines, and computations that prove claims about graphs).

A much more dramatic event may be currently in the process of realization. This involves Hales' (officially uncertified) proof of the Kepler conjecture and the launching of the FlysPecK project in 2003. For the sake of completeness here is a short account of the story.[43] The conjecture was stated by Kepler in 1611. It claims that the most efficient way to fill a volume by very small equal spheres is to use a repetitive pattern based on planar hexagons (that pattern is often used in stacking oranges in a store). The efficiency

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[41] Some claimed that long computations are only calculations, not proofs or not proof-components. This appeal to linguistic usage, which distinguishes between "proofs" and "calculations," is misguided. It is not a question of everyday usage and the position is untenable. For it introduces a distinction between mathematical statements that are proved by proofs and mathematical statements that are proved by calculation, for example, that $243112609 - 1$ is prime. Besides, any calculation can be formally transformed into a proof in which the induction scheme is repeatedly applied.

[42] The original paper is (Appel & Haken, 1977); it is the first part of a paper whose second part has also Koch as a co-author. (Appel & Haken, 1989) is a book which contributed to a wider appreciation of the proof. The simpler and easier to verify proof is by four authors (Robertson *et al*., 1996). The verification by means of a powerful proof checker is described in (Gontier, 2008). The excellent Wikipedia entry on the subject is recommended.

[43] The Wikipedia entry on the subject is excellent; but the entry—at the time of writing this paper— does not cover the most recent information given in Hales *et al*. (2010),

is measured by the asymptotic ratio, as the spheres' diameter approaches 0, of the volume covered by the spheres to the total volume. Gauss proved it under the assumption that the spheres are arranged in a regular lattice, and in 1900 Hilbert included the question as part of his 18th problem. Hales, following an earlier known suggestion, found that the answer could be derived by minimizing a function of 150 variables, and that it would prove the conjecture if certain lower bounds could be found for each of 5000 configuration of spheres. This could be done by solving 100000 linear programming problems. The final proof consisted of 250 written pages, and of 3 gigabytes of code, data, and results. The editors of *Annals of Mathematics* agreed to publish it, if it were accepted by a panel of 12 experts.[44] In 2003, after 4 years of work the referees, though sympathetic, decided that they could not go on. The chairman said that they were "99% certain" that the proof is correct, but they could not certify the correctness of all calculations. In 2006 *Discrete and Computational Geometry* devoted an issue to a paper in which Hales and his student Ferguson presented the proof; and in 2009 they received for it the Fulkerson prize for outstanding papers in discrete mathematics. The proof remains uncertified.

In 2003 Hales launched a big project, called *FlysPecK* (the capitals standing for "Formal Proof Kepler") whose aim is to create a fully formalized proof and to verify it by computer. In 2008 he published on the internet a so-called *blue print edition* which is supposed to serve as a blueprint for the construction of the proof in full formalization. It has also been restructured in a more transparent way, and contains additional detail and many simplifications. It turned out that the 2006 version did contain some errors, mostly minor, except one gap that was fixed after some work.

In Hales *et al*. (2010) the formal proof that is to be constructed is described thus:

> A formal proof is a proof in which every logical inference has been checked, all the way back to the foundational axioms of mathematics. No step is skipped no matter how obvious it may be to a mathematician. A formal proof may be less intuitive, and yet is less susceptible to logical errors. Because of the large number of inferences involved, a computer is used to check the steps of a formal proof.[45]

And the methodology is this:

> The first stage is to expand the traditional proof in greater detail. This stage fills in steps that a mathematician would regard as obvious, works out arguments that the original proof leaves to the reader, and supplies the assumed background knowledge. In a final stage, the detailed text is transcribed into a computer-readable format inside a computer proof assistant. The proof assistant contains mathematical axioms, logical rules of inference, and a collection of previously proved theorems. It validates each new lemma by stepping through each inference. No other currently available technology is able to provide levels of certification of a complex mathematical proof that is remotely comparable to that available by formal computer verification.

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[44] The journal is among the most prestigious mathematics journals in the world.

[45] "A formal proof may be less intuitive" is an extreme understatement. It will not be intuitive at all, except perhaps to people who have been working on it for years.

Hales estimates that by now the project is "about half way complete," and since in another place he says that it is a 20-year project (presumably he counts his initial work as part of it), this means that he expects to have the final product within the next 10 years.

The success of the project will make it a milestone in the history of proving mathematical claims. It will probably give rise to heated debates in the mathematical community, but the very existence of such a project has philosophical implications. For one thing it shows that talk about reconstructing and formalizing big proofs in formal deductive systems is much more then hand waving. And it gives us a good feel for what this formalization might involve.

How is all this related to realism? The "natural question" view maintains that natural questions are decidable. Thus it is a version of Hilbert's no-*ignorabimus* position, which restricts Hilbert's position to natural questions. The four-color hypotheses and Kepler's conjecture are natural questions *par excellence*. Since they are decidable they are not counterexamples to the "natural question" view. But they go against the very spirit of it: the belief that the thought that gave rise to good mathematical problems must also be able to solve them. It strengthens a realistic position by revealing the gritty aspect of mathematical reality.

For many mathematicians the strong appeal of mathematics lies in the fact that it combines beauty with truth. The beauty is undeniable, and the use of beautiful proofs to discover truths is highly gratifying. And so is the fact that the beauty of a theorem often derives from the deep connections it reveals between apparently different phenomena. All this is well known. But there is a price to be paid for truth, namely, that it can be messy. Not everything is subject to the revealing power of the human mind. Very often there is nothing we can do but to use brute force, to sift through numerous pieces of data. And sometimes we need a computer to show us the beautiful consequences of our beautiful axioms. If, besides beauty, the target of mathematics is truth, then the mathematicians will often bite a big chunk of it, more than they can chew.

§7. Some final summing up observations. Let me try to sum up and bring together some of the points made in the paper. The theme that underlies the work is the proposed way of characterizing ontological positions in the philosophy of mathematics: One's ontological commitment is determined by the kind of questions that have, according to one's position, objective answers. The more a position is ontologically committed, the more realistic it is. Section 2 exemplifies this approach through a list of examples, some of which represent main schools in the philosophy of mathematics.

The philosophical discussion of Sections 3 and 4 make the general point that the ontology inheres neither in the objects, nor in the bunch of relations defined over them, but in the combination of both.

The import of the independence (or undecidability) results is one of the main themes of the paper. I am mostly concerned with independence results for first-order arithmetical statements. My general view is that these results do not affect our conception of the standard natural numbers. Hence, they reinforce any realism that is based on this conception. They do so by raising the serious possibility that there are mathematical truths that are inaccessible to us due to our epistemic limited resources. This is surely knowledge-transcendent truth. The source of our epistemic limitations is the fact that mathematical knowledge must be certified by proofs, which means that the set of theorems is computably enumerable. This by itself gives rise to undecidability results.

I believe that independence results in set theory have a potential of undermining the belief in a unique standard model of higher order arithmetic, say second order or third order. But this requires further analysis, and the paper does not go into it.

Another theme of this work is the lesson to be drawn from finitary versions of the undecidability results, which appear as computational lower bounds on the time (or space) it takes for an algorithm to solve a problem. This shows that analogous phenomena arise in finitary situations, where we cannot get the desired knowledge, if the price is too high. It is also related to the use of computers in proving the four-color conjecture, or in Hales' project for proving Kepler's conjecture. Here we can get knowledge, but we can do so only by paying the price of using a device that operates outside the purview of human "pure thought." The link between the axioms and their simple and beautiful conclusions must pass through numerous opaque computations.

The problem of the "natural questions" of a given area in mathematics has to do with the relations between ontological and epistemic factors. The major advances in the history of mathematics have been made by reorganizations of the epistemic framework, which made it more transparent, hence more efficient. On the other hand the translation of a mathematical theory into set theory is an ontological reduction that destroys the epistemic organization of the area. It is of no use at all to the mathematician who works in that area. Natural questions depend on epistemic organization, and the undecidability of such a question shows to the mathematician that his or her grasp of the area was flawed: what appeared as a well-stated question that has a unique solution turns out to have more than one answer, depending on some version of set theory. Speaking metaphorically, it is an undermining of the epistemic by the ontological. (The later appears here as set theory, to which the given theory is being reduced.) Hilbert believed that the two can go together. The success of Hilbert's program would guarantee that we can safely work in a given area, pretending that the actual infinities that figure in our reasoning are real; thus pretended realism is safe. And the slogan "in mathematics there are no *ignorabimus*" meant that our epistemic apparatus is able to solve any mathematical problem that arises in our research. After all, the problem was not forced on us by the physical world. It arose in the domain of pure thinking; therefore pure thought can handle it. The undecidability results, and the need to use messy data processing in order to arrive at beautiful theorems, introduce a wedge between the epistemic and the ontological; pure thought is not enough.

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