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Abstract: *My aim in this paper is limited in scope. I will present Benacerraf's well-known dilemma, 1 offering historical remarks both on its origins and on its influence on the philosophy of language and the philosophy of mathematics of the last fifty years (forty-six, to be precise). I will then consider a suggestion of Charles Parsons to the effect that there is a Kantian analogue of the dilemma.2 I will make some critical comments in order to provide what I believe is an improved formulation of Parsons's suggestion. I will briefly conclude with a presentation of further directions of inquiry based both on this new formulation and on the conception of arithmetical intuition developed in Parsons.3*

Keywords: *Benacerraf's dilemma, I. Kant, mathematical intuition, mathematical knowledge, mathematical objects, C. Parsons, Structuralism, Truth.*

A KANTIAN ANALOGUE OF BENACERRAF'S DILEMMA: PRELIMINARY COMMENTS ON A SUGGESTION OF CHARLES PARSONS

1. Philosophers of the second half of the twentieth century and of the early part of the twenty-first have discussed two theses. Not all them have, of course, and quite a number of them have been busy pondering over quite different issues, but a great number of them in the analytic tradition have nevertheless either advocated or rejected them in one form or another. Here is a brief and unpolished version of these insights. The first is that language contacts reality through quantifiers. The second is that the semantic interpretation or value of the sentences of a language is to be understood in terms of their truth conditions. The philosophers who were concerned with such insights have then considered these two claims with various mathematical languages in mind. By way of an application of the insights to, say, the language of arithmetic, or to the language of set theory, or to the language of some other selected mathematical theory, they have gone on to either defend or attack the view that these languages contact mathematical reality, or some particular portion of it (numbers, sets, etc., as the case may be) through quantifiers, and that the semantic interpretation or value of their sentences or formulae had to be understood in terms of conditions of their truth.

Among those who felt uncomfortable with the two insights, some complained that it is mysterious how we know anything abstract, in particular the abstracta that appear in the truth conditions of the sentences of mathematical languages. The lesson they drew was that they had to meet a challenge. The challenge, of course, was to give an account of quantification and truth conditions that would be compatible with an explanation of the acquisition of mathematical knowledge, i.e. of true justified mathematical beliefs.

Various arguments and techniques have been called to the rescue to show that the compatibility could indeed be obtained and, in one particular case, to show that it wasn't needed anyway (more on this point later on in this section). My purpose here isn't to be exhaustive and critical, let alone to go into the many details of the arguments and techniques that have been provided, but to give a general picture of the situation thereby generated. This is important because the challenge has somehow determined a model of philosophical inquiry for philosophers concerned with *the philosophy of the language of mathematical theories*, a model of the kind of thing that might be done, or that at least might prove to be worth trying, when addressing the compatibility question. Little by little, the model has become "too obvious for words" to adopt Charles Taylor's phrase,[4] the need to argue for the compatibility of semantics and epistemology becoming an organizing principle for these philosophers' practice.

One may distinguish two kinds of agendas or programs addressing the challenge. The first kind pertains directly to quantification, the second pertains directly to truth.

Quantification. The first kind of agenda may take on two forms.

A. One *reinterprets mathematics* entirely so that *no abstracta* such as numbers, functions, sets and the like appear among the values of the variables bound by the objectual existential quantifier, but only — rather typically[5] — physical objects, linguistic expressions or mental constructions, i.e. items which (supposedly) do not qualify as causally inert entities. The problem remains, of course, to construe them as causally active. It is far from obvious that *types* of physical objects or linguistic entities — as opposed to tokens — will qualify, not to speak of mental constructions, whether they happen to be those of the idealized Brouwerian creative subject or those of a naturalized knower of mathematics. The point here, in any event, is to secure *concreta* in the course of values of the bound variables, so that sentences of the form "$(\exists x)$…" read as "There is at least one object…," where objects are placed within the reach of means of human cognition *not* involving a direct grasp of abstracta.

B. The other option is to *reinterpret the quantifiers* and to allow only the *substitutional* interpretation. (One may describe this move somewhat more drastically by saying that one thereby adopts another *variety* of quantification altogether, thus discarding the familiar objectual kind.) Instead of formulating existence claims with the objectual quantifier "$(\exists x)$," one formulates them with the substitutional quantifier "(Σx)." The point here is that the bound variables range over *names* instead of objects so that sentences of the form (Σx)…" read as "There is at least one true substitution instance of…." Names, or at least particular inscriptions or instances of them do count here — as it were by definition or qua linguistic items — as being indeed within the reach of human cognition *not* involving a direct grasp of abstracta.

Truth. The second kind of agenda also has two forms.

A. One accepts the notion of mathematical truth, but constrains it by provability, either in principle or effective, so that it is garanteed by the very nature of the case that we are able to know that provability conditions — as opposed to truth conditions unfettered — are satisfied whenever they indeed are. The point here is that the only bona fide notion of mathematical truth is one on which the truth of mathematical sentences or formulae *may not* transcend their assertability or verifiability by us, either in principle or, more stringently in case one surrenders to finitistic inclinations, effectively, say in polynomial time.

B. One proposes a substitute to the notion of truth, namely conservativity, so that our use of mathematical existence assertions gives us no grounds whatsoever for believing them to be true *under any reading of "true,"* i.e., say, whether or not truth might be transcendent with respect to provability, or whether or not "it is true that *p*" is merely a meta-linguistic variant of *p*. The idea here is than an assertion containing no expressions that might be part of the non logical ressources of a mathematical theory isn't a consequence of a set of similar assertions plus some mathematical theory unless it is already a consequence of that set of assertions without the mathematical theory. In other words, the conclusions we get at when applying mathematics aren't genuinely new for they are already derivable without recourse to mathematics *taken at face-value*, albeit in a more long-winded or cumbersome fashion. I think it is fair to say that in this Fieldian perspective,[6] one in some important way *abandons* the original challenge. Or, if one addresses it still, it is only insofar that one strives to show that (*i*) truth isn't at stake anymore as far as semantics is concerned and that (*ii*) the difference between someone who knows mathematics and someone who doesn't is explained away in terms of abilities to carry out inferences — e.g. in physics[7] — without arriving at anything genuinely new that coudn't be obtained without the mathematics anyway. This isn't an authentic way of reconciling mathematical truth with mathematical knowledge, but indeed a way of surrendering to a substitute for truth, given that mathematical knowledge is now knowledge *how* rather than knowledge that. A satisfactory account of mathematical knowledge how doesn't prima facie require an explanation of how we manage to acquire true mathematical beliefs that nicely "reflect the facts"[8] about remote abstract entities.

2. Benacerraf's dilemma has played a key role in the development of these arguments and strategies and, consequently, in providing a model for what I've called the organizing principle of the practice of philosophers concerned with the philosophy of the language of mathematics.

The dilemma amounts to this: either we have a truth conditional semantics for the language of mathematics, or we have a reasonable epistemology that accounts for mathematical knowledge, *but not both* (in the first instance, "reasonable" might be understood in the ordinary sense of "fair," "plausible" or "sensible"). Here is the relevant passage where Benacerraf makes this plain:[9]

> It is my contention that two quite distinct kinds of concerns have separately motivated accounts of the nature of mathematical truth: (1) the concern for having a homogeneous semantical theory in which semantics for the propositions of mathematics parallel the semantics for the rest of the language*, and (2) the concern that the account of mathematical truth mesh with a reasonable epistemology. It will be my general thesis that almost all accounts of the concept of mathematical truth can be identified with serving one or another of these masters *at the expense of the other*. Since I believe further that both concerns must be met by any adequate account, I find myself deeply dissatisfied with any package of semantics and epistemology that purports to account for truth and knowledge both within and outside of mathematics. For, as I will suggest, accounts of truth that treat mathematical and nonmathematical discourse in relevantly similar ways do so at the cost of leaving it unintelligible how we can have any mathematical knowledge whatsoever; whereas those which attribute to mathematical propositions the kind of truth conditions we can clearly know to obtain, do so at the expense of failing to connect these conditions with any analysis of the sentences which shows how the assigned conditions are conditions of their *truth*.
> * I am indulging here in the fiction that we *have* semantics for "the rest of language," or, more precisely, that the proponents of the views that take their impetus from this concern often think of themselves as having such semantics, at least for philosophically important segments of the language.

This is how Benacerraf presented the dilemma in Atlanta on December 27, 1973 at a symposium on Mathematical Truth jointly sponsored by the American Philosophical Association (Eastern Division) and the Association for Symbolic Logic. Among the historical details that the unnumbered footnote in Benacerraf[10] provides on the previous readings of various segments of the original version written between 1967 and 1968, it is worth noticing that Hartry Field and Mark Steiner feature are among those who commented on these early unpublished versions read in the mid-sixties at Harvard and Princeton (among other universities). Benacerraf's article is only mentioned in an endnote in Field 1980,[11] Field remarking in his last chapter that although it has "overtsepped the bounds of first-order logic," his nominalism nevertheless "saves us from having to believe in a large realm of otherwise gratuitous entities […] which are very unlike the other entities we believe in (due for instance to their causal isolation from us and from everything we experience) and which give rise to substantial philosophical perplexities because of this difference [e.g. Benacerraf's dilemma, as the endnote makes clear]."[12] Before that, Steiner, addressing the challenge in a most direct way (as opposed to Field's way *out* of the dilemma by way of a substitution of conservativity for truth) has defended a naturalistic approach to mathematical knowledge according to which our cognitive apparatus, equiped with the relevant perceptual and introspective resources, is able to generate true intuitive mathematical beliefs without requiring any kind of access to remote and abstract mathematical objects.[13]

In section II of an unpublished version of the paper dating from 1968, entitled "The problem" and which corresponds quite closely to section II of the published 1973 version entitled "Two conditions," Benacerraf points out that:[14]

> The interests I have in mind are two and these: A) Any account of mathematical truth must be recognizably an account of *truth*. […] [T]here must be some general view of truth on the basis of which the property attributed to mathematical propositions when they are said to satisfy the conditions set down by a candidate for an account of truth is indeed truth. I will argue that we have only one such general account, Tarski's […]. […] My second requirement on accounts of mathematical truth presupposes that we have mathematical knowledge, and that such knowledge is no less knowledge for being mathematical. Since we are capable of knowing truths, an account of mathematical truth, to be acceptable, must be consistent with the possibility of having mathematical knowledge: the conditions of the truth of mathematical propositions cannot be such that it is impossible for humans to know that they are satisfied. This is not to argue that there cannot be unknowable truths — only that not all truths can be knowable, for we do know some. The minimal requirement, then, is that a satisfactory account of mathematical truth must be consistent with the possibility that some such truths be knowable. Actually, I will make a stronger requirement: that B) Any account of mathematical truth must be useful as part of an explanation of the existence of particular bits of mathematical knowledge. […] [I]n mathematics, it must be possible to link up what it is for *p* to be true with my knowing that *p*. Though this is extremely vague, I think one can see how condition B tends to rule out accounts which satisfy condition A, and to admit those ruled out by A.[*] For a typical account satisfying A (at least in the case of number theory or set theory) will depict truth conditions in terms of conditions on objects whose nature, as normally conceived, renders them inaccessible to the better understood means of human cognition (e.g. sense perception and the like). The "combinatorial" accounts, on the other hand, usually arise from a sensitivity to just this fact and are hence almost always motivated by epistemological reasons. Their virtue lies in providing an account of the nature of mathematical truth based on the procedures we follow in justifying truth claims in mathematics: proof. It will therefore come as no surprise that *modulo* such an account of mathematical truth, there is little mystery about how we can obtain mathematical knowledge. We need only account for our ability to produce and survey proofs. However, squeezing the balloon at that point apparently makes it bulge on the side of truth: the more nicely we tie up the concept of proof, the more closely we link the definition of proof to combinatorial (raher than semantic) features, the more difficult it is to connect it up with the truth of what is being thus "proved" — or so it would seem.
> [*] I see possible exceptions: for example, the class of views on which all of Mathema-tics is metamathematics and on which every mathematical sentence receives an interpretation via a truth definition. Views on which mathematics consist simply in turning a generative crank on a black box that prints out meaningless symbols are not even in the ballpark we are considering, for ["There are at least three prime numbers between 17 and 43"] would, on such views, either not be a mathematical statement, or would, at any rate, lack a truth-value.

The origin of the dilemma may be traced back to Benacerraf's dissertation, written under the supervision of Hilary Putnam and defended in Princeton in May 1960. Its concluding paragraph is telling in this respect:[15]

> I conclude then that Logicism is mistaken. What I have termed its second thesis is certainly wrong, and, one might argue, so is its first thesis. Such an argument would hang on a determination of the line which marks the outer boundary of logic, a line I do not care to draw, for reasons already expounded elsewhere. This leaves us with the problem of giving an account of the precise nature of the relation between logic and mathematics or, if one prefers, between set theory and the rest of mathematics. I have done my best to indicate that it is not the part-whole relation. We are also left with the problem of accounting for the nature of mathematical truth, if indeed such an animal exists. There is a sense in which we would still be left with that problem even if we had accepted Logicism as fundamentally correct. To say that mathematics is really logic in disguise merely pushes the problem off onto logic. If logic includes set theory, the problem is particularly difficult. I don't even know of an adequate answer to the question when limited to the propositional calculus and quantification theory. I suspect that the animal in question (the nature of mathematical truth) will turn out to be a many-headed monster; it will have to be slaughtered and appropriately butchered into pieces which are sufficiently manageable to lend themselves to fruitful dissection. This, at least is what I have tried to suggest throughout.

The first thesis is that mathematics is reducible to logic or, in a broader form, that the reduction of arithmetic to logic provides arithmetic with a foundation.[16] The second thesis is that mathematical propositions are true in virtue of the definitions of the concepts involved in them, or more specifically, that the analyticity of mathematical propositions is due to the explicit definability of mathematical concepts in terms of logical concepts, logical propositions being themselves analytic.[17]

It is worth noticing that Benacerraf has left the discussion of these two logicist theses on the side in the 1973 paper. He considers them only indirectly when discussing Quine's criticism of the notion of truth by convention because it is then clear that if all mathematical truths are definitional abbreviations of logical truths, mathematics is indeed true by convention.[18] He remarks in 1973 that, the accounts of mathematical truth and mathematical knowledge being many, his twin restraints or strictures that an account of mathematical truth should follow Tarskian lines and that an account of mathematical knowledge should follow causalist lines "are intended to apply to them all."[19] They should then apply to logicism as well, or at least to the Hempelian version favoured by Benacerraf in the dissertation. He does discuss logicism direcly in the 1968 version of the paper, though, mentioning Russell *en passant*.[20]

To put an end to these historical remarks on the legacy of Benacerraf's particular way of understanding the epistemological challenge to platonism, let us note that causal inefficacy has quite generally been understood as the key problem faced by platonists.[21] It is thus deemed mysterious "how we concrete beings can know abstracta,"[22] or "utterly inert numbers."[23] The emphasis is sometimes on the social and the dynamic: it is then judged puzzling how we, "evolving social organisms in space-time," could have access to "beasties," for "[t]hey toil not, neither do they spin."[24] Or again: "there is no interchange of energy-momentum between [mathematical entities] and the material world [which includes us]"[25]. It isn't just that many anthologies mention this problem. The Benacerraf and Putnam anthology does, of course,[26] but also Dale Jacquette's.[27] Some take it indeed as a starting point and claim that most of philosophy of mathematics is an attempt at solving the dilemma. Thus Hart:[28]

> Benacerraf's dilemma is not the only philosophical problem about mathematics, but it is certainly basic to metaphysical and epistemological concerns about mathematics. The dilemma gives us a perspective from which to organize many, especially contemporary, philosophical discussions of mathematics. For if the dilemma is as real as it seems, and if the ontology of platonism is incompatible with the epistemology of empiricism […], then consistency demands that at least one horn of the dilemma yield. So one question to ask about an essay on the dilemma is which horn it seeks to blunt, and how.

3. One way of looking at the matter is to *deny* that there is anything mysterious about the knowledge of the abstracta that feature in the truth conditions of the sentences of mathematical languages and that the consistency requirement, so construed, is misguided. One may then stick to the two insights we started with and look for an account of mathematical knowledge which does not rely on causal relations but still strives to explain how we acquire our mathematical beliefs and to account for their truth.

One possibility is to explain how mathematical knowledge is obtained and developed through intuition, as opposed to the so-called "better understood means of human cognition" favoured by causalist and reliabilists accounts.

There are of course many different construals of the notion to be found in the literature. I'll be looking at Kant's exclusively and only in relation to Parsons' suggestion. (Note that Benacerraf considers a different account of intuition when rejecting Gödel's thesis that we have a mathematical intuition of the objects of transfinite set theory. He assumes in this instance that Gödel, as a realist, is aware that a standard or Tarskian account of mathematical truth must be connected both with an interpretation of the referential apparatus of the theory and with an account of the connection between the objects known and our human cognitive resources, criticizing Gödel for the obscurity and superficiality of the analogy with sense perception, an analogy which provides no ground for a positive and convincing account of what we would call a mathematical intuition *de re* of the objects of transfinite set theory.)[29]

For Kant, the only kind of intuition we have as humans is sensory or sensuous intuition[30]. We only have intuitions of objects which are given to us, either through the perception of the senses (sight, typically), or in the imagination. But we also have pure or specifically mathematical knowledge. Since "[t]houghts without content are empty, [and] intuitions without concepts are blind,"[31] a concept and an intuition of an object must converge or be combined in order for us to obtain mathematical knowledge:[32]

> To be sure, a few principles that the geometers presuppose are actually analytic and rest on the principle of contradiction… yet even these, athough they are valid in accordance with mere concepts, are admitted in mathematics only because they can be exhibited in intuition.

Or again:[33]

> Even from a priori concepts, as employed in discursive knowledge, there can never arise intuitive certainty, that is [demonstrative] evidence, however apodeictically certain the judgment may otherwise be.

Kant is able to reconcile the view that intuition is of one kind, i.e. sensory, with the view that we have pure mathematical knowledge by pointing out that sensory intuition exemplifies the concept or instantiates it. Intuitions are singular representations that relate to objects immediately; concepts are general representations that relate to objects mediately, i.e. through or with the help of intuition.[34] For Kant, mathematics isn't about suis generis objects, but about instantiations of pure mathematical concepts, or at least, about possible instantiations of them. So it would seem that the problem we've started with cannot be one at all from the Kantian point of view for at least two reasons. First because there aren't any causally inert objects remote from ordinary sense experience to begin with, as indeed there are in the platonist picture. Moreover, since Kant also denies that we have intellectual or non sensory intuition, i.e. any special kind of faculty which would as it were come into play only when we are engaged in doing mathematics, he also implicitely denies that we have a special kind of de re intellectual intuition of what we've called "abstract objects" all along should such abstracta, *per impossibile*, exist.

Parsons nevertheless proposes a reading of Kant's puzzle about intuition and of Kant's solution to it which connects them to Benacerraf's dilemma. The puzzle is that we cannot intuit *both* spontaneously [*ursprünglich*] *and* a priori because "an intuition is such a representation as would immediately depend on the presence [*Gegenwart*] of the object."[35] Parsons argues that:[36]

> Kant's puzzle is related to the dilemma about mathematical truth posed by Paul Benacerraf in 'Mathematical Truth' […]. According to Benacerraf, our best theory of mathematical *truth* (Tarski's) involves postulating mathematical objects, while our best account of *knowledge* requires causal relations of the objects of knowledge to us; but mathematical objects are acausal.
>
> One can present Kant's problem as a similar dilemma: mathematical truth requires applicability to the physical world. But our best account of mathematical knowledge makes it rest on intuition, which requires the prior presence of the object. But this contradicts the a priori character of mathematics.
>
> This is of interest because it is a form of the dilemma that does not require that the semantics of mathematics involve mathematical objects […]. But of course it depends on other assumptions, in particular that mathematics is a priori.

One could be ungenerous with Parsons and complain that a puzzle which doesn't require that the semantics of mathematical languages involve quantification over abstract objects *may not be* a genuine variant of the original dilemma. The interest of the analogy, if any, must therefore lie somewhere else. What philosophers who take Benaceraff's dilemma seriously have done is to take abstracta into consideration by what David Lewis has called the "Way of Negation."[37] They have defined or identified such objects as those that *lack* the features possessed by paradigmatic concrete objects, i.e. objets which we ordinarily think of as "material" or "physical." Three features are usually taken into consideration in this respect: spatiality, temporality and causal efficacy. Abstract objects are exactly those which do not occupy any region of space, of time, or of space-time, and make nothing happen. By doing so, these philosophers have looked at objects which are, by their very nature, abstract, if only for negative reasons, and *not* at possible empirical instantiations of mathematical concepts, as Kant does. Prima facie, then, the truth *vs.* causal inefficacy divide isn't quite similar to the applicability *vs.* aprioricity divide. In other words, the thesis that what we're committed to via semantics (abstracta) is incompatible with what some desideratum epistemology must satisfy (a causal or reliabilist account) — which is exactly what Benacerraf's dilemma amounts to —, is quite distinct from the idea that what we're committed to via semantics (applicability) is incompatible with what some desideratum epistemology must satisfy (an account of a prioricity) — which is what Kant's puzzle is about.

I wish to argue that despite this, the dilemmas or puzzles are indeed similar in the sense that in both cases something we wish to preserve, namely the idea that mathematics taken at face value yields truths or consists in a body of truths, is in conflict with some epistemological constraint: an empiricist, either causalist or reliabilist in Benacerraf's case, a transcendental one in Kant's case. It might not be entirely preposterous, then, to consider the puzzles *conjointly* and claim that, should we wish to preserve truth, we would end up either with abstracta we cannot access or with the presence of objects which can't be known a priori. There is, in this sense, a Benacerraf-Kant dilemma according to which a link must indeed be provided betwen what it is for a mathematical proposition to be true and our recognizing that it is true, so that either our true mathematical beliefs reflect the facts about mathematical entities or are causally connected to them (under the causal or reliability constraint), or our intuition doesn't rely at all on the existence or actuality of the objects known (aprioricity constraint). On the view that there are indeed mathematical truths, the Kantian rejoinder to Benacerraf's incompatibilty claim is that the attribution to mathematical propositions of truth conditions we can clearly know to obtain when they do *succeeds* to connect these conditions with an analysis of the propositions which shows how the assigned conditions are conditions of their *a priori truth*. If this rejoinder is acceptable, the link between our cognitive faculties and the interpretation of the referential aparatus of mathematical theories which is severed in Benacerraf's original dilemma, is restored in the Kantian solution to the Kantian version of the puzzle suggested by Parsons.

On the epistemological horn of Benacerraf's original dilemma, we have the kind of causal theory of knowledge developed by Goldman,[38] Skyrms,[39] and Harman,[40] along with Grice's causal theory of perception[41] and, subsequently, Pitcher's.[42] Taken together and in a nutshell, these accounts of knowledge and perception yield the claim that for us to know that *p* (or that *p* is true), there must exist some causal relation between us and "the referents of the names, predicates and quantifiers of [*p*]"[43] such that the very objects with which we are thus causally related are involved in the generation of our perceptual belief states in an appropriate causal way (this last part coming from Pitcher[44] and, ultimately, from Grice[45]).

The causal theory of reference is sometimes added[46] so that we have the following schema: *S* knows that *p* (or that *p* is true) if and only if there is a causal relation between *S* and the referent of the names, predicates and quantifiers of *p* such that: (*a*) these referents are involved in the generation of *S*'s knowledge (or justified belief) that *p* and (*b*) *(i)* the reference of the names, predicates and quantifiers is originally fixed by perception, and *(ii)* further uses of these linguistic items for referential purposes are all linked by a causal chain stretching back to the original fixing.

On the epistemic horn of Kant's puzzle, we have an account of intuition as being of one kind, i.e. sensory, which therefore requires the prior presence of the objects so that they may be given to us, either through sense perception, or by recourse to our imagination. In Benacerraf's dilemma, what would make mathematical knowledge both possible and reliable, i.e. causal interactions with the truth-conditions of mathematical existence assertions, is precisely what we're denied if we also hold that such assertions are true. We have a contradiction in terms, more than a challenge. In Kant's puzzle, what would make that knowledge possible, i.e. intuition, is what we're denied if we also argue that such assertions (or the propositions expressed by them) are a a priori.

4. Let us look at the Kantian solution in more details. Kant gives his solution to the puzzle about the possibility of a priori intuition in §9 of the *Prolegomena*. He also develops the solution in the first Critique, in the Transcendental Aesthetics where he begins by saying that there is intuition only insofar as objects affect our mind [*das Gemüt*], but since §9 is the passage Parsons relies on let us begin with it:[47]

> Therefore in one way only can my intuition [*Anschauung*] anticipate the actuality of the object, and be a cognition a priori, viz.: if my intuition contains nothing but the form of sensibility, antedating in my subjectivity all the actual impressions through which I am affected by objects.

"It is a nice question, Parsons remarks, just what this does to the characterization of intuition that gives rise to the puzzle."[48] What it does, clearly, is this: under the assumption that mathematics is a priori, the (alleged) causal or material dependence of our intuition on the objects, or on their presence, either by means of sense perception or in the imagination, *has to go*. What we have is knowledge by intuition without any causal action on us (either on our sensory apparatus or on our minds) on the part of anything *we* (not Kant) would call an abstract mathematical object.

It would be unfair at this point to complain that an account of mathematical knowledge in terms of an intuition that contains *only the form of sensibility* typically depict[s] the truth conditions of mathematical statements "in terms of conditions on objects whose nature, as normally conceived, places them beyond the reach of the better understood means of human cognition (e.g. sense perception and the like)."[49] Time, as a pure form of sensory intuition and as an a priori condition of all phenomena in general[50] may not be the kind of thing that could ever fall under sense perception, for it is, on the contrary, what makes the reality of phenomena possible.[51] If the Kantian claim that all we need in order to be able to add units is the inner sense of time[52] is correct, we do indeed have a solution to the original puzzle, at least for the limited case of the arithmetic of natural numbers. The point here is that it would be misguided to argue that such an inner sense doesn't fit in the perceptual, causalist or reliabilist model, for it does provide what Benacerraf has claimed all along is missing from accounts of arithmetical truth, namely an explanation of how our justification for the truth of first order arithmetical claims involving natural numbers is obtained. It still is possible, of course, to criticize Kant's proposal and to reject the Kantian solution. My point here is only that it would be unfair to complain that Benacerraf's challenge or puzzle has not been properly addressed.

Although, as Parsons correctly remarks, Kant doesn't explicitely express a view about the intuition of mathematical *objects*, or about the referential apparatus of mathematical theories taken at face-value, an improved formulation of Parsons' suggestion which nevertheless remains true to Kant's idea that mathematical truth requires both applicability and a prioricity must insist that the appeal to a priori conditions and to pure forms of sensory intuition is compatible with an account of mathematical truth (as opposed to an account of mathematical provability or derivaility). It would be unfair to complain at this point that it is compatible with it only provided that the candidate for an account of truth be one for *a priori truth*. What the dilemma or puzzle requires is an account of the knowability of mathematical propositions and this is just what the Kantian account proposes.

Parsons' suggestion in Parsons[53] nevertheless reverts to a non Kantian notion of intuition. Parsons[54] favours a view of arithmetical intuition which relies on ordinary perception at the most basic level. We start with a language containing a basic symbol '|' and we go on with arbitrary strings containing occurrences of this symbol in order to obtain the well-formed expressions of the language. We perceive by ordinary means a string of stroke-tokens: |, ||, ||| and so on, which is isomorphic to the natural numbers. At the next level up, we have singular propositions such as "|| is the successor of |." Such singular propositions are about types. Parsons construes the propositional knowledge *that* || is the successor of | as being justified by a single unique intuition.[55] It is also a general proposition, but only insofar as it has implications for *any* token. So we go from intuitions *of* to intuitions *that* because we take any instance of both the kind of situation and of the kind of assertion that correspond to it as being paradigmatic.

We also have general propositions about types, such as "Each string of strokes can be extended by one more," and such general propositons "have in their scope indefinitely many *different* types."[56] No actual perception or sensory imput is available here, which would act as a warrant for the proposition. As Parsons notes, the idea that we have an intuition of types “faces serious objections because of the timelessness, acausality or incompleteness of types as abstract entities.”[57] What we have to do in this case is to imagine an arbitrary string of strokes either as a vague object, or in such a way that its internal structure is entirely irrelevant to our new concern about types. Parsons remarks that such imaginings or *Gedankenexperimente* count as warrants (“verification” is the word he uses in that respect) of the general statement about types. Obviously they do if and only if certain conditions as to *how* an arbitrary string of strokes *must* be imagined are met, namely in this case, either vaguely or in such a way that the internal structure is “seen” or “understood” or construed in some way as irrelevant.

Parsons grants, at this point, that the problem about the timelessness of types is by nature epistemological. It is mysterious how we may justify truths about types through a perception of their tokens, i.e. truths which would hold for *any* token. We may have an intuiton of the tokens but not of the types because types belong to the category of objects which fail to occupy a determinate region of space-time. It is striking, of course, how *un*-Kantian is the proposal. At the most basic level, our arithmetical knowledge relies on a kind of intuition which crucially depends on the prior presence of the objects. At the level of general propositions, we’re left with objects characterized as abstract by the Way of Negation.

Parsons’ proposal is of course quite different from, say, Maddy’s. (Maddy argues that we can acquire perceptual beliefs about sets of physical objects by construing the belief that, say, there are three physical objects at a given location (three eggs in a box) as a belief about a s*et* of physical things and not about a *physical* aggregate.)[58] We do not have such direct intuition of abstracta (e.g. sets) in Parsons’ analysis. What we have in Parsons’ case is what he calls a “moderate position” to the effect that “intuition gives objects which form a model of arithmetic,” this model being “as good as any, both for the foundations of arithmetic and for applications.”[59]

It is clear, on the Kantian side, that the limits of what we are able to establish as true in mathematics is determined by subjective conditions which are proper to us, as human beings. We are limited to that which can be represented a priori in intuition, i.e. space and time and change in time. We may then ask the following question: What would determine such limits according to theories which hold that we perceive mathematical objects directly so that the perception contains something contentual, utterly different from the *form* of sensibility? Such limits must also be linked to our particular cognitive constitution. But they must be so in a radically different way than the one envisaged by any transcendental philosophy.

Consider again the abstract object stroke-string-type. What we have here as warrants for the general propositions about types are intentional properties of the abstract object. The object is abstract because, although it might be instantiated, it cannot be located anywhere. It possesses properties such as vagueness or lack of internal structure insofar as it is an object of our *intuition* (through the imagination). One might say that it *necessarily* possesses them *as intuitions*, in the sense that we may not intuit the object otherwise. In other words, the stroke-string type is arbitrary or vague or without structure insofar as it is untuited in this way by us. It isn't intrinsically so.

According to this picture, then, there is a link between the way in which we justify our claims about tokens by means of ordinary sense perception and the *Gendankenexperimente* we are legitimately appealing to when justifying claims about timeless types of such tokens. What one then needs, then, is an explanation of how such means of justification are related. It may furthermore be asked, of course, whether the tiered account is compatible with an account of arithmetical truth, but the question about the articulation of kinds of warrants must certainly be answered first.

Notes

1. Paul Benacerraf, "Mathematical Truth," unpublished draft of January 1968, 54 pages, typed with handwritten corrections, 1968; "Mathematical Truth," in *Philosophy of Mathematics - Selected Readings*, 2nd ed., ed. Paul Benacerraf and Hilary Putnam (Cambridge: Cambridge University Press, [1973], 1983), 403-420.
2. Charles Parsons, "Mathematical Intuition," in *The Philosophy of Mathematics*. Oxford Readings in Philosophy, edited by W. D. Hart (Oxford: Oxford University Press,1996), 95-113.
3. This paper is based on various versions of a first segment of a much longer paper read at the Foundations of Mathematics and the Origins of Analytic Philosophy workshop held at the university of York in october 2010. The paper was extensively revised and a new version read at the *Institute for Social and Political Research* (ICSP) of the West University of Timisoara and at the *Institute for Philosophical Research* of the University of Sofia in may 2013. I wish to thank Michael Beaney, Ioan Biris, Boris Grozdanov, Lilia Gurova, Dan Isaacson, Marco Panza, Vesselin Petrov, Octavian Repolschi and Anguel Stefanov for their comments and criticisms. I gratefully acknowledge the generous financial support of the *Centre National de la Recherche Scientifique*, of the *Institut d'Histoire et de Philosophie des Sciences et des Techniques*, of the University of York and of the West University of Timisoara. For Parsons, see "Ontology and Mathematics," *Philosophical Review,* Vol. 80 (1971): 151-176, and the article mentioned in note 2.
4. Charles Taylor, "Philosophy and its history," in *Philosophy in History: Essays in the Historiography of Philosophy*, ed. R. Rorty, J. B. Schneewind and Q. Skinner (Cambridge: Cambridge University Press, 1984), 20.
5. See Hartry Field, *Science Without Numbers - A Defence of Nominalism* (Princeton, New Jersey: Princeton UP, 1980), 1.
6. See Field, *Science Without Numbers - A Defence of Nominalism*.
7. Field, *Science Without Numbers - A Defence of Nominalism;* chapters 7 and 8.
8. Field's phrase in Field, *Realism, Mathematics and Modality* (Oxford: Basil Blackwell, 1989), 26.
9. Paul Benacerraf, "Mathematical Truth," in *Philosophy of Mathematics - Selected Readings*, 2nd edition, ed. Paul Benacerraf and Hilary Putnam (Cambridge: Cambridge University Press, [1973] 1983), 403-404.

10. Benacerraf, “Mathematical Truth,” in *Philosophy of Mathematics - Selected Readings,* 2nd edition, 403.
11. Field, *Science Without Numbers - A Defence of Nominalism,* note 66 at page 126.
12. Field, *Science Without Numbers - A Defence of Nominalism,* 92, 98.
13. see Mark Steiner, “Platonism and the Causal Theory of Knowledge,” *Journal of Philosophy*, vol. 70, n°3 (8 Feb. 1973), (1973): 57-66; and Mark Steiner, *Mathematical Knowledge* (Ithaca, New York: Cornell University Press, 1975).
14. Paul Benacerraf, “Mathematical Truth,” unpublished draft of January 1968, 54 pages, typed with handwritten corrections. 1968, 15-18.
15. Paul Benacerraf, *Logicism, Some Considerations* (PhD dissertation, Ann Arbor, Michigan, Princeton University: University Microfilms Inc., Reference: 61-4511, 260 pages, 1960), 255-256.
16. see Benacerraf, *Logicism, Some Considerations* (PhD dissertation, Ann Arbor, Michigan, Princeton University,1960), ch. I.
17. see Benacerraf, *Logicism, Some Considerations* (PhD dissertation, Ann Arbor, Michigan, Princeton University,1960), ch. III.
18. See Willard Van Orman Quine, “Truth by Convention,” in *Philosophy of Mathematics - Selected Readings*, 2nd ed., ed. Paul Benacerraf and Hilary Putnam [with minor corrections] (Cambridge: Cambridge University Press, [1936] 1983), 329-354.
19. Benacerraf, “Mathematical Truth,” in *Philosophy of Mathematics - Selected Readings*, ed. Paul Benacerraf and Hilary Putnam (Cambridge: Cambridge University Press, 1973), 415.
20. Benacerraf “Mathematical Truth,” unpublished draft of January 1968, 54 pages, typed with handwritten corrections. 1968: section IV, at pages 41-44. Section 2 of Benacerraf’s “What Mathematical Truth Could Not Be - I,” in Benacerraf and his Critics, ed. A. Morton and S. P. Stich, 9-59 (Oxford and Cambridge, Mass.: Basil Blackwell, 1996) offers additional relevant historical information, e.g. on Benacerraf’s negative reaction to Quine and Ayer, going as far back as the nineteen fifties when Church and Gödel’s ideological impurity was blatant in an academic world dominated by empiricism and logical positivism.
21. Following Field’s remark that Benaceraff’s challenge is to explain how our mathematical beliefs so well reflect the facts about them (Field, *Realism, Mathematics and Modality* (Oxford: Basil Blackwell, 1989), 29), it is generally assumed that there is no need to restrict our attention to causal mechanisms. Mechanisms that the outmoded causal theory of knowledge wouldn’t coutenance because they would deny the remoteness and acausality of mathematical objects might nevertheless “do the the job” and meet the challenge. Either that, or the antirealist programs are indeed facing equivalent worries, so that the Benacerraf challenge isn’t tied to any particular view of the ontology (or metaphysics) of mathematics. There are important discussions of these two related issues by Clarke-Doane, Liggins, Linnebo and Shapiro. I cannot go into the details here. This is an occasion for another paper.
22. Dale Gottlieb, *Ontological Economy: Substitutional Quantification and Mathematics* (Oxford: Oxford University Press, 1980), 11.
23. Wilber Dyre Hart, edit., *The Philosophy of Mathematics.* Oxford Readings in Philosophy (Oxford: Oxford University Press, 1996), 4.
24. Paul Benacerraf and Hilary Putnam. eds., *Philosophy of Mathematics - Selected Readings*, 2nd edition. (Cambridge: Cambridge University Press, 1983), 31; This is almost a quote from Luke

12: 27 — "Consider the lilies how they grow: they toil not, they spin not [.…]" — in the King James version.
25. H. Field, *Realism, Mathematics and Modality* (Oxford: Basil Blackwell, 1989), 19.
26. See, in particular, sections 6 and 8 of the introduction to *Philosophy of Mathematics - Selected Readings*, 2nd edition (Cambridge: Cambridge University Press, 1983) at pages 21-27 and 30-33, respectively.
27. See Dale Jacquette, *Philosophy of Mathematics - An Anthology* (Oxford: Basil Blackwell, 2002), 2-3.
28. Hart, *The Philosophy of Mathematics*, 5.
29. Benacerraf, "Mathematical Truth," in *Philosophy of Mathematics - Selected Readings,* 2nd edition, 415-416.
30. Immanuel Kant, *Immanuel Kant's Critique of Pure Reason*, English translation by N. Kemp Smith (New York: St. Martin's Press, [1781, 1787] 1965), B75.
31. Kant, *Immanuel Kant's Critique of Pure Reason*, B75.
32. Kant, *Immanuel Kant's Critique of Pure Reason*, B16; [Einige wenige Grundsätze, welche die Geometer voraussetzen, sind zwar wirklich analytisch und beruhen auf del Satze des Widerspruchs;… Und doch auch diese selbst, ob sie gleich nach bloßen Begriffe gelten, werden in der Mathematik nur darum zugelassen, weil sie in der Anschauung können dargestellet werden.].
33. Kant, *Immanuel Kant's Critique of Pure Reason*, A734, B762; [Aus Begriffen a priori (im diskursiven Erkenntnisse) kann aber niemals anschauende Gewißheit, d. i. Evidenz entspringen, so sehr auch sonst das Urteil apodiktisch gewiß seing mag.]
34. Kant, *Immanuel Kant's Critique of Pure Reason*, A320/B376-377, A68/B93.
35. Immanuel Kant, *Prolegomena to Any Future Metaphysics*, English translation of *Prolegomena zu einer jeden künftigen Metaphysik die als Wissenschaft wird auftreten können* by L. W. Beck (Indianapolis: Bobbs-Merrill Publishing Company, 1783 [1950]), §8.
36. Charles Parsons, "Mathematical Intuition," in *The Philosophy of Mathematics*. Oxford Readings in Philo-sophy, ed. W. D. Hart (Oxford: Oxford University Press, [1979-1980] 1996), 99, note 12.
37. David Lewis, *On the Plurality of Worlds* (Oxford: Basil Blackwell, 1986).
38. Alvin Goldman, "A Causal Theory of Knowing," *Journal of Philosophy*, vol. 64, n°12 (June 1967): 357-372.
39. Brian Skyrms, "The Explication of '*X* knows that *p*'," *Journal of Philosophy*, vol. 64, n°12 (June 1967): 373-389.
40. Gilbert Harman, *Thought* (Princeton, New Jersey: Princeton Univeristy Press, 1973).
41. H. P. Grice, "The Causal Theory of Perception," *Proceedings of the Aristotelian Society*, supplementary volume 35 (1961): 121-152.
42. George Pitcher, *A Theory of Perception* (Princeton, New Jersey: Princeton University Press, 1971).
43. Benacerraf, "Mathematical Truth," in *Philosophy of Mathematics - Selected Readings,* 2nd edition, 412.
44. Pitcher, *A Theory of Perception.*
45. Grice, "The Causal Theory of Perception."
46. see Benacerraf, "Mathematical Truth," in *Philosophy of Mathematics - Selected Readings,* 2nd edition, 412.

47. Kant, *Prolegomena to Any Future Metaphysics*, §9.
48. Parsons, "Mathematical Intuition," 99.
49. Benacerraf, "Mathematical Truth," in *Philosophy of Mathematics - Selected Readings,* 2nd edition, 409.
50. Kant, *Prolegomena to Any Future Metaphysics*, §6.
51. Kant, *Prolegomena to Any Future Metaphysics*, §4.
52. Kant, *Immanuel Kant's Critique of Pure Reason*, [B182].
53. Parsons, "Mathematical Intuition."
54. Parsons, "Mathematical Intuition," sect. IV-VII.
55. Parsons, "Mathematical Intuition," 105.
56. Parsons, "Mathematical Intuition," 105.
57. Parsons, "Mathematical Intuition," 109.
58. Penelope Maddy, "Perception and Mathematical Intuition," *Philosophical Review*, vol. 89 (1980): 163-196; republished in *The Philosophy of Mathematics*. Oxford Readings in Philosophy, ed. W. D. Hart (Oxford: Oxford University Press, 1996) 126-131.
59. Parsons, "Mathematical Intuition," 111.

Bibliography

Benacerraf, Paul. *Logicism, Some Considerations*, PhD dissertation, Ann Arbor, Michigan, Princeton University: University Microfilms Inc., 1960. Reference: 61-4511. 260 pa-ges.

Benacerraf, Paul. "Mathematical Truth," unpublished draft of January 1968, 54 pages, typed with handwritten corrections. 1968.

Benacerraf, Paul. "Mathematical Truth." In *Philosophy of Mathematics - Selected Readings*, 2nd edition, edited by Paul Benacerraf and Hilary Putnam, 403-420. Cambridge: Cambridge University Press, [1973] 1983.

Benacerraf, Paul. "What Mathematical Truth Could Not Be - I." In *Benacerraf and his Critics*, edited by A. Morton and S. P. Stich, 9-59. Oxford and Cambridge, Mass.: Basil Blackwell, 1996.

Benacerraf, Paul and Hilary Putnam, eds. *Philosophy of Mathematics - Selected Readings*, 2nd edition. Cambridge: Cambridge University Press, 1983.

Boolos, George. "On the Proof of Frege's Theorem." In *Benacerraf and his Critics*, edited by A. Morton and S. P. Stich, 143-159. Oxford and Cambridge, Mass.: Basil Blackwell, 1996.

Field, Hartry. *Science Without Numbers - A Defence of Nominalism*. Princeton, New Jersey: Princeton University Press, 1980.

Field, Hartry. *Realism, Mathematics and Modality*, Oxford: Basil Blackwell, 1989.

Goldman, Alvin."A Causal Theory of Knowing." *Journal of Philosophy*, vol. 64, n°12 (June) (1967): 357-372.

Gottlieb, Dale. *Ontological Economy: Substitutional Quantification and Mathematics.* Oxford: Oxford University Press, 1980.

Grice, H. P.. "The Causal Theory of Perception." *Proceedings of the Aristotelian Society*, Supplementary Volume 35 (1961): 121-152.

Harman, Gilbert. *Thought*. Princeton, New Jersey: Princeton Univeristy Press, 1973.

Hart, Wilber Dyre, (ed.), *The Philosophy of Mathematics*. Oxford Readings in Philosophy. Oxford: Oxford University Press, 1996.

Jacquette, Dale. *Philosophy of Mathematics - An Anthology*. Oxford: Basil Blackwell, 2002.

Kant, Immanuel. *Immanuel Kant's Critique of Pure Reason*, english translation by N. Kemp Smith. New York: St. Martin's Press, [1781, 1787] 1965.

Kant, Immanuel. *Prolegomena to Any Future Metaphysics*, english translation of *Prolegomena zu einer jeden künftigen Metaphysik die als Wissenschaft wird auftreten können* by L. W. Beck. Indianapolis: Bobbs-Merrill Publishing Company, 1783 [1950].

Lewis, David. *On the Plurality of Worlds*. Oxford: Basil Blackwell, 1986.

Maddy, Penelope. "Perception and Mathematical Intuition." *Philosophical Review*, vol. 89 (1980): 163-196; republished in *The Philosophy of Mathematics*. Oxford Readings in Philosophy, edited by W. D. Hart, 114-141. Oxford: Oxford University Press, 1996.

Parsons, Charles.. "Ontology and Mathematics." *Philosophical Review*, vol. 80 (1971): 151-176.

Parsons, Charles. "Mathematical Intuition." In *The Philosophy of Mathematics*. Oxford Readings in Philosophy, edited by W. D. Hart, 95-113. Oxford: Oxford University Press, [1979-1980], 1996.

Pitcher, George. *A Theory of Perception.* Princeton, New Jersey: Princeton University Press, 1971.

Quine, Willard Van Orman. "Truth by Convention." In *Philosophy of Mathematics - Selected Readings*, 2nd ed., edited by Paul Benacerraf and Hilary Putnam, 329-354 [with minor corrections]. Cambridge: Cambridge University Press, [1936] 1983.

Skyrms, Brian. "The Explication of '*X* knows that *p*'." *Journal of Philosophy*, vol. 64, n°12 (22 June 1967) (1967): 373-389.

Steiner, Mark. "Platonism and the Causal Theory of Knowledge." *Journal of Philosophy.* vol. 70, n°3 (8 Feb. 1973), (1973): 57-66.

Steiner, Mark. *Mathematical Knowledge*. Ithaca, New York: Cornell University Press, 1975.

Taylor, Charles. "Philosophy and its history." In *Philosophy in History: Essays in the Historiography of Philosophy,* edited by R. Rorty, J. B. Schneewind and Q. Skinner. 17-30. Cambridge: Cambridge University Press, 1984.