On Benacerraf's Dilemma, Again

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Despite its enormous influence, Benacerraf's dilemma admits no standard, unanimously accepted, version. This mainly depends on Benacerraf's having originally presented it (Benacerraf 1973) in a quite colloquial way, by avoiding any compact, somehow codified, but purportedly comprehensive formulation. But it also depends on Benacerraf's appealing, while expounding the dilemma, to so many conceptual ingredients so as to spontaneously generate the feeling that most of them are in fact inessential for stating it. Apart from the almost unanimous agreement on the fact that, despite Benacerraf's appealing to a causal conception of knowledge throughout his exposition of the dilemma, this is, in itself, independent of the adoption of such a conception, there have not been, however, and still there is no agreement about which of these ingredients have to be conserved so as to get a sort of minimal version of the dilemma, and which others can, rather, be left aside (or should be so, in agreement with an Okkamist policy).

My purpose, here, is to come back to the discussion on this matter (section 1), with a particular attention to Field's reformulation of the problem, (especially in Field 1989*a*), so as to identify two converging and quite basic challenges, respectively addressed by Benacerraf's dilemma to a platonist (section 2) and to a combinatorialist (in Benacerraf's own sense) philosophy of mathematics (section 3). What I mean by dubbing these challenges 'converging' is both that they share a common kernel, which encompasses a crucial conundrum for any plausible philosophy of mathematics[1], and that they suggest (at least to me) a way-out along similar lines. Roughing these lines out is the purpose of the two last sections of the paper (sections 4 and 5).

1. Field's Reformulation of Benacerraf's Challenge to a Platonist: Is the Problem Really Concerned with Truth and Knowledge?

Unquestionably, Benacerraf's purpose was to keep the reader's attention on a supposed contrast between two sorts of philosophical concerns about mathematics,

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[1] This last challenge is possibly not generalisable to any sort of a priori knowledge or beliefs, as the original version of the dilemma is, instead (as recently shown, for example, in Thurow 2013, sect. 2).

one doubtlessly epistemological, the other apparently semantic, though possibly rather ontological, in nature. Having been originally written for a presentation at a symposium on mathematical truth, the paper appeals to this very notion already in its title, and, from its very first lines, declares its interest for mathematical knowledge, explicitly presented as a notion depending "on how truth in mathematics is properly explained" (Benacerraf 1973, p. 661). Still, under some readings, the dilemma appears to be eventually independent of both the notions of (mathematical) truth and (mathematical) knowledge, by being rather concerned with the connection between mathematical beliefs, or possibly their formation process or justification, and the subject matter of mathematics, that which mathematics is to be taken to be about, if it is to be taken to be about something.

This is at least, what is suggested by Field's reformulation of the problem. This takes the form of a challenge to "mathematical realism" or "platonism", conceived as "the view that there are mathematical entities and that they are no way mind dependent or language dependent" (Field 1989*a*, p. 228), "bear no spatio-temporal relation to us[, and][…] do not undergo any physical interactions […] with us or anything we can observe" (Field 1989, p. 27), in few words, they are both mind and language independent, and abstract[2].

One of Field's explicit purposes is just that of adapting the challenge to this characterisation of platonism, which is, as such, independent of any appeal to truth and knowledge. According to his picture, a mathematical platonist is not mandated to take mathematical statements to be true in some sense of 'true' "more loaded" that a mere "disquotational" sense (Field 1989*a*, pp. 228-229; cf. also Field 1988, pp. 62-63). He or she is merely required to maintain that "his or her own states of mathematical beliefs, and those of most members of the mathematical community […] are highly correlated with the mathematical facts" (Field 1989*a*, p. 230; cf. also Field 1988, p. 62), namely the "facts about [the] mathematical entities" (Field 1989*a*, p. 232) that he or she takes to there be. Once this is admitted, the challenge can be freed, not only from any appeal to any non-merely-disquotational conception of truth, but also from "any theory of knowledge", and, then, from "any assumption about necessary and sufficient condition for knowledge" (Field 1989*a*, pp. 232-233). In short, it "can be put without

[2] Two clarifications are in order. Firstly, though Field writes "mind dependent or language dependent", it seems clear to me that the 'or' counts here as an 'and', as this is confirmed by many other formulations by Fled himself (for example in Field 1989, p. 27). Secondly, though Field's preference is for labeling this view 'mathematical realism', I prefer using the term 'platonism' and its cognates, since the former term is, more often than the latter, also used in literature to refer to other views openly concerned with mathematical truth, or, more generally, with the truth-value of mathematical statements. I also dislike the term 'entity' to denote that which, according to a platonist, mathematics is about, since using this term suggests that platonism is quite vague about the logical status of what mathematics is about. This is why, except in quotes, I shall later use, instead, the (logically much more precise) term 'object'.

use of the term of art 'knows', and […] without talk of truth" (Field 1989*a*, p. 230; cf. also Field 1988, p. 62).

What Field means when he says that, for a platonist, the states of mathematical beliefs of most members of the mathematical community are highly correlated with the mathematical facts is that "for most mathematical sentences that you substitute for '*p*', the following holds: If mathematicians accept '*p*', then *p*" (Field 1989*a*, p. 230; Field 1988, p. 62; cf. also Field 1989, p. 26). The challenge consists, then, in asking a platonist for an appropriate explanation of such a "systematic correlation" or "general regularity" (Field 1989*a*, p. 231), "an explanation of how it can have come about that mathematicians's belief states and utterances so well reflect the mathematical facts" (Field 1989*a*, p. 230; Field 1988, p. 62). It seems plain that accepting '*p*' is here taken to be the same as believing that *p*, and mathematicians's utterances are taken as content-transparent expressions of mathematicians's states of belief. This suggests rephrasing the challenge as follows: how can a platonist explain that, at least in the great majority of cases, a mathematician has the mathematical belief that *p* only if it is a (mathematical) fact that *p*?

From Field's perspective, the question is rhetorical, of course, since, according to him, "there seems *prima facie* to be a difficulty in principle in explaining the regularity" (Field 1989*a*, p. 230-231). Later in the paper, this *prima facie* seeming difficulty becomes a principled impossibility, and the challenge transforms in a negative precept (Field 1989*a*, p. 233; cf. also Field 1989, p. 26):

> […] we should view with suspicion any claim to know facts about a certain domain [namely mathematics, in the case at issue] if we believe it is impossible in principle to explain the reliability of our beliefs about the domain.

Two things might appear to be strange in this way of putting the problem: that it relies on a quite controversial and loaded epistemological notion as that of reliability, which the previous considerations do not take into account, at first glance; and that the notion of mathematical knowledge is appealed, which seems to contradict Field's initial proposal.

A way to answer the former worry is to observe that the reliability that Field is here evoking is not that of some justifications or of any sort of belief formation process, but rather that of the relevant beliefs themselves. Of course, one might stipulate that a belief is reliable if and only if this is so for its formation process. But this does not seem to be what Field is meaning. He rather seems merely to take the reliability of mathematical beliefs to be the same as their reflecting mathematical facts in the sense of the aforesaid systematic correlation or general regularity[3].

[3] This reading is quite openly suggested by Field himself, when he writes that "one would have to formulate more clearly the claim that our mathematical beliefs are 'reliable', or 'reflect the mathematical facts' " (Field 1989, p. 26), and, among others, by Burgess & Rosen (1997, pp. 41-42) and Linnebo (2006, pp. 548-549).

A way to answer the latter worry is to observe that mathematical knowledge only enters the matter *a fortiori*, so to say, and independently of any specific view on it: what Field seems to mean is that, if this correlation or regularity is not explained, there is no room, for a platonist, to provide an appropriate account of mathematical knowledge, whatever his or her conception of knowledge might be[4].

The challenge does not seem, then, to change its nature; the crucial point is the same as before: how can a platonist explain that, in the great majority of cases, a mathematician has the mathematical belief that *p* only if *p*?

But, one could retort, is a platonist really required to maintain that, in the great majority of cases, a mathematician has the mathematical belief that *p* only if *p*?

Even if it is admitted that the reference of the term 'mathematician' is clearly enough fixed, the answer still largely depends on what is meant by 'in the great majority of cases' and 'mathematical belief'. If one takes a mathematical belief to be any belief that can be expressed though a statement using a mathematical vocabulary (or a vocabulary widely recognised as a mathematical one), or even using only such a vocabulary (together with appropriate logical constants), and the majority of cases to be the majority of mathematical beliefs that mathematicians have, have had, or will have, the answer seems to be negative. Leaving logically possible cases of collective hypnosis or hallucination apart, it remains that many mathematicians have, have had, and presumably will have opposite beliefs so expressed, since much of these beliefs are, were, and will presumably be grounded on no proof, or, at least, on no widely accepted proof, or depend, depended, and will presumably depend on methodological, philosophical, aesthetic, or even mystical attitudes or convictions[5]. In other terms, much of what mathematicians believe, believed, and will presumably believe on what

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[4] This reading is suggested by Liggins (2006, p. 137), and seems to be confirmed by two other formulations of what Field takes to be the "key point" of Benacerraf's dilemma, which he offers in (Field 2005, pp. 77 and 81):

> […] our belief in a theory should be undermined if the theory requires that it would be a huge coincidence if what we believed about its subject matter were correct. But mathematical theories, taken at face value, postulate mathematical objects that are mind-independent and bear no causal or spatiotemporal relations to us, or any other kinds of relations to us that would explain why our beliefs about them tend to be correct; it seems hard to give any account of our beliefs about these mathematical objects that doesn't make the correctness of the beliefs a huge coincidence.

> The Benacerraf problem […] seems to arise from the thought that we would have had exactly the same mathematical […] beliefs, even if the mathematical […] facts were different; because of this, it can only be a coincidence if our mathematical […] beliefs are right, and this undermines those beliefs.

[5] Remark that I'm not referring, here, to mere conjectures having a conditional or dubitative content, since one could argue that these are not expressions of genuine beliefs. I rather refer to unquestionably-genuine beliefs expressing by apodictic statements, like '$2^{\aleph_0} \neq 2^{\aleph_1}$', or 'the real part of any non-trivial zero of the Riemann zeta function is 1/2'.

they take, took, and will presumably take to be the subject-matter of mathematics, even of pure mathematics, is, has been, and will presumably be open to controversy within the mathematical community itself. There is, then, no reason for thinking that someone who considers that there are mathematical objects, and that they are mind and language independent and abstract have also to maintain that, in the great majority of cases, a mathematician has the mathematical belief that *p* only if *p*, if this claim is so intended.

Field's point appears, instead, much more plausible if the range of mathematical beliefs is restricted to beliefs somehow secured within purely mathematical theories widely accepted within the mathematical community. For short, call these beliefs 'mathematical theory-tied beliefs'[6].

That the challenge is, in fact, restricted to these beliefs is something that Field himself suggests. He remarks that "as mathematics has become more and more deductively systematized, the truth [disquotationally understood, I suppose] of mathematics has become reduced to the truth of a smaller and smaller set of basic axioms", with the result that what a platonist needs to explain is only the alleged (by him or her) circumstance that "for all (or most) sentences '*p*' […][,] if most mathematicians accept '*p*' as an axiom, then *p*", or better, that "either *p*, or [most] mathematicians don't take '*p*' as an axiom" (Field 1989*a*, p. 231). So conceived, the challenge is echoed by R. Heck Jr., while rephrasing Benacerraf's dilemma, in turn (Heck 2000, p. 128):

> […] we lack […] an explanation of how we come to know the axioms, be these the axioms of some developed mathematical theory or those propositions which are, in a less developed theory's present state, typically assumed without proof. More precisely, it is not obvious why there should be any relation at all between our belief that the axioms are true and the facts of mathematics as the platonist conceive them, why our beliefs should reliably reflect how things stand with the sets or the numbers, or whatever.

Field and Heck clearly consider that it is easy to meet the challenge for whatever mathematical theory-tied belief, if it is met for the axioms of the relevant theories. Still, this is so only if all these theories consist of formal axiomatic systems, and they are all sound in an appropriate sense, that is, they are such that if their axioms reflect the mathematical facts, so do their theorems. Even if it were taken for granted that the former condition is met, it would still remain to explain that also the latter is so: it

[6] Remark that this is not the same as arguing that Field's challenge is to be intended as the request that a platonist explain the reliability of the process that is supposed to secure the relevant beliefs. What I mean is rather that this challenge appears as a plausible one only if it is intended as the request that a platonist explain how it happens that a mathematician has the mathematical theory-tied beliefs that *p* only if *p*? Though the difference between the two formulations could appear quite slight, at first glance, I take it to be essential. My further developments should make clear my reasons for that.

would remain to explain how it happens that the deductive rules of the relevant theories fit with the way mathematical facts are related to each other. For this reason, and also for taking into account other sorts of theories, either formal or not, that do not consist of axiomatic systems, I think it would be more appropriate to generalise what Field and Heck say on mathematical axioms to any sort of liminal assumptions of the relevant theories, certainly including axioms, but also other sorts of stipulations or presumptions (either explicitly governing formal deductions, like deductive rules, or entering informal but widely accepted proofs).

Still, if it is admitted that meeting the challenge for these liminal assumptions makes easy to meet it for any mathematical theory-tied beliefs, why should one restrict its formulation to the former? This does not make it easier to meet. For sake of all-inclusiveness, I prefer, then, to state it for mathematical theory-tied beliefs, in general.

Finally, if the content of these beliefs is taken to be stable under the variation of the cognitive subjects that have them, and of the cognitive context in which they do (as Field and Heck seem to admit), the challenge can be stated without any appeal to mathematicians as the bearers of theses same beliefs: all what is relevant are the beliefs themselves[7].

In the end, the question seems then to be the following: how can a platonist explain that mathematical theory-tied beliefs reflect the mathematical facts (in the sense specified above)?

It remains, however, that not appealing to mathematicians as the bearers of the relevant beliefs while stating the challenge is not the same as taking it to be independent of what mathematicians do, namely of their providing justifications of these beliefs. By definition, a belief is, indeed, a mathematical theory-tied one only if it comes together with a consensual epistemic practice that secures it: typically a consensually accepted justification for it, or, at least, a consensual admission that it has an acceptable justification, namely a widely accepted proof, or another sort of (direct or indirect) ground as those usually appealed for supporting mathematical axioms or other liminal assumptions of mathematical theories.

Hence, though neither Field's nor Heck's formulations of the challenge appeal to the justification of the relevant beliefs (either under the form of a proof or of any sort

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[7] Things would be different if the challenge were taken as being concerned with the reliability of the process that is supposed to secure the relevant beliefs. For one could easily admit that, if there is something special that makes this process reliable, then mathematicians have it available. Considering mathematicians as the bearers of the relevant beliefs would, then, be a way for focusing on this special reliability-maker, rather than on the beliefs themselves. Still, I do not think at all that the problem with Benacerraf dilemma, however understood, is that of identifying such a special reliability-maker for mathematics (for example something as what it is often allegedly referred to with the term 'mathematical intuition', which is for me hardly understandable without further specifications). I will explain me, later, on this point. For the time being, it is merely of order to observe that this is a first reason for taking the two formulations considered in footnote (6) as different to each other.

of suitable ground), and despite Field's own claim that "Benacerraf's challenge […] is not […] a challenge to our ability to justify our mathematical beliefs, but […] a challenge to our ability to explain the reliability of these beliefs" (Field 1989, p. 25), the justification for the relevant beliefs seems to be an indispensable ingredient of it[8].

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[8] This has already been remarked by Burgess & Rosen (1997, p. 42), but cf. also Linnebo (2006, p. 571, note 4). Liggins (2006, pp. 139-140) has, instead, insisted, that the two projects of "explaining how our beliefs come to be justified; and […][of] explaining how our beliefs come to be reliable" are "distinct" and "quite separate" (and they would be so, even if "being justified" were conceived as being the same as "being formed by a reliable process", since the former project should then involve an explanation of such a conception of being justified, whereas the latter should not), and that Field's argument "has nothing to do" with the former project, but rather pertains to the latter (cf. also Liggins 2010, p. 73). It is not clear to me what Liggins means by reliability of mathematical beliefs. Still, his point seems to be that one thing is wondering whether some beliefs count as justified or not, and another is wondering what makes, in general, a belief formation process reliable (or, possibly, what, in general, makes a belief reliable, if the latter question were considered as different from the former). Though I fully agree on this distinction, I disagree that Field's point concerns the question of establishing what makes reliable the formation process of mathematical beliefs, provided it be different from the question of explaining how the relevant beliefs reflect the mathematical facts, having, instead, nothing to do with the way the relevant beliefs are justified. Indeed, it seems clear to me that: *i*) for Field, claiming that our mathematical beliefs are reliable merely means the same as claiming that they reflect the mathematical facts; *ii*) Field's point cannot plausibly be done with respect to any mathematical belief, but is to be restricted to theory-tied ones. Now, under these circumstances, separating the explanation of the reliability of the relevant beliefs from any consideration of their justification seems to me quite artificial, unless this depends on arguing that mathematical theory-tied beliefs are not necessarily justified. Field has suggested something like that, by arguing that "many of our beliefs and inferential rules in mathematics, logic, and methodology" are such that "we must be, in a sense, entitled to them by default" (Field 2005, pp. 81-82), and that "our being default-entitled to them" is not to be regarded as a "mysterious metaphysical phenomenon", since what happens "it's basically just that we regard it as legitimate to have these beliefs and employ these rules, even in the absence of argument for them, and that we have no other commitments that entail that we should not so regard them". According to him, a reason for considering that, in the case of mathematical such beliefs—namely mathematical axioms or other sorts of liminal assumptions of mathematical theories—, "the need for justification doesn't seem as pressing", is that in mathematics it does not seem to there be "genuine conflict between alternative theories", for "it's natural to think that different mathematical theories, if both consistent, are simply about different subjects" (*ibid*., pp. 82-83). But, as he also observes, this might not "lessening the need for justification", but merely entail "that the justification for consistent mathematical theories comes relatively cheap: by the purely logical knowledge that the theory is consistent" (*ibid*., p. 83). The latter option (according to which, mathematical axioms or other sorts of liminal assumptions of mathematical theories are merely justified by proving their consistency, or, at least, by arguing for it) is perfectly fitting with what I shall say later (though my point also applies in the case we admit more substantial sorts of justification for them). Under the former option (according to which, we are merely entitled by default to mathematical axioms or other sorts of liminal assumptions of mathematical theories, so that they have no justification at all), something I shall say in what follows would not apply, instead. But the conclusion I shall come to, regarding what I take to be the crucial

Field's insistence on the idea of a systematic correlation or a general regularity suggests, then, that the required explanation could be offered only insofar as it were identified a stable (i.e. regularly and/or systematically operating, under the variation of *p*) connection between the (mathematical) fact that *p* and the consensually accepted justifications for the belief that *p*. Taking an identification of this connection to be an essential ingredient of a theory of mathematical knowledge, or preferring to avoid any appeal to a so loaded term as 'knowledge' for describing the problematic setting that is here at issue, depends, to my mind, more on a terminological than on a substantial option. What is important is that, according to Field, a platonist could be hardly credited with a decent epistemology (broadly understood as a conception of the virtues that mathematics has for us) if he or she were not able to answer this challenge[9].

2. Another Way to Understand Benacerraf's Challenge to a Platonist

When Benacerraf's dilemma is seen under this light, its original formulation seems to depend on the requirement that the connection to be explained (which in the original settings takes the form of a "connection between the truth conditions of *p* […] and the grounds on which *p* is said to be known": Benacerraf 1973, p. 672) hinge on a "causal relation" (*ibid.*, p. 671) between the epistemic subjects having the relevant beliefs and the constituents of the relevant facts, namely the objects that these facts

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challenge addressed by Benacerraf's dilemma to a platonist, could be easily restated for it to apply also under this option. More on this in footnote (14).

[9] Field (Field 1989*a*, pp. 233-239; Field 1988, pp. 62-67) has considered the possibility of trivially meeting the challenge by observing (in Linnebo's words: 2006, p. 557) that "the correlation to be explained has no counterfactual force", since mathematics is necessary, and a mathematical fact obtains, then, in any possible word. He has offered different arguments against this line of response, and other scholars have come back on some of them, or offered other arguments to the same purpose. I'm not interested in entering this discussion here, since it seems to me there is a quite simple way to block a similar response, even by taking for granted (which, in fact, I'm personally not ready to make) that mathematics is necessary in an appropriate way, and that, then, a mathematical fact obtains in any possible word. The point is simply that, even if it were admitted that a mathematical fact obtains in any possible word, this would in no way entail that the mathematical theory-tied beliefs were the same in any possible word. It is not hard at all to imagine, indeed, a possible word in which these beliefs include, for example, the belief that 5+7=13, though what happens, there, as in any other possible word (under the granted assumption) is that 5+7=12. So, in this setting, a platonist should still explain how it happens that in our actual word, mathematical theory-tied beliefs include the belief that *p* only if *p*, even if this not so in any possible word (also under the assumption that a mathematical fact obtains in any possible word). One could say that, if it is granted that the fact that *p* obtains in any possible word, requiring an explanation of this is not requiring an explanation of a genuine correlation. Still, far from solving the problem, this purely terminological remark would leave it perfectly intact.

are supposed to be about[10]: a relation allowing one to account for the justification of these beliefs by admitting that these subjects are causally affected by these objects, and that the justification just results from this.

Dropping this requirement is not only worthy, since, as famously observed by Hart (Hart 1977, pp. 125-126), "superficial worries about the intellectual hygiene of causal theories of knowledge are irrelevant […] and misleading […], for the problem is not so much about causality as about the very possibility of natural knowledge of abstract objects". It is also indispensable for avoiding begging the question by charging a platonist—admitting, as in Field's picture, that mathematical objects are mind and language independent and abstract and, then, that mathematical facts are facts about mind and language independent *abstracta*—with a burden which is, in principle, not in his or her power to meet.

But there is more: Sereni's meticulous argumentation, in his paper included in the present volume (pp. ???), suggests that any plausible requirement about the nature of the relevant connection fall into the risk either of begging the question, too, or of making the challenge lose any specificity.

Should we conclude, as Sereni, that the challenge that Benacerraf's dilemma addressees to a platonist, even if understood in the minimal form I have described above, following Field, is either ill-posed or unspecific?[11]

[10] It is not necessary here to better specify which relation mathematical facts are supposed to bear to mathematical objects. Taking this relation to be one of a certain or another nature rests on different ontological views whose specific nature should not be taken to affect the points that are here under discussion. What is important is that mathematical facts are taken to depend (in a way or another) on the way mathematical objects are or stand to each other. Following Field's jargon (cf. Field 1989*a*, p. 232, quoted above, section 2), I use here the preposition 'about' to indicate this unspecified relation, namely I say that some facts are about some objects to mean that the former depend on the way that latter are or stand to each other.

[11] Sereni's conclusion is, in fact, assimilable to this one only under some specifications. What he argues for is that, when addressed to a platonist, Benacerraf's dilemma faces, as it were, a meta-dilemma structurally quite similar to itself: for it to be recovered so as to avoid begging the question, and being, in this sense, ill-posed, "it should nor rely on notions so robust as to make the corresponding challenge to the platonist prejudicial"; for it to be recovered so as to avoid being confused with other, already well-known charges to any sort of platonist view about any subject, or, even, with any satisfaction condition for any philosophical account of mathematics, and being, in this sense, unspecific, "it should not be so general that no novel or dedicated threat is raised for mathematical platonism"; still, though "both requirements are desirable and can be defended on their own[…][,] it is unclear whether they can be satisfied together" (this volume, p. ???). Sereni's suggestion is, clearly, that they cannot. My point is, rather, as I shall try to explain in what follows, that the former requirement can be fully satisfied in such a way as to address to mathematical platonism a challenge, which, for the very form in which it is stated, specifically addresses to mathematical platonism, though encompassing, as I have said at the begging of my paper, a crucial conundrum for any plausible philosophy of mathematics. That the same challenge could be seen also behind other current arguments in philosophy of mathematics is, then, another question: this is certainly true, but, far from undermining the problem, it rather testifies as deep it is.

I do not think so. I rather think that for the challenge to be a non-ill-posed and specific[12] one, it is merely enough to stay away from any requirement about the nature of the connection between the mathematical theory-tied belief that *p* an the (mathematical) fact that *p*, by merely requiring that the justification of the former (namely the consensual epistemic practice that secures it, and makes, then, it be a mathematical theory-tied belief)[13] be a justification that the latter obtains.

To see the point, let us reflect a little more on Field's picture.

This picture does not only depend on the characterisation of a platonist as someone that maintains that there are mathematical objects, and that they are mind and language independent and abstract. It also depends on the admission that, according to such a platonist, the relevant mathematical beliefs have a propositional content, and that this content is that a mathematical fact, namely a fact about these very objects (presumably consisting in their being ones to each others in certain relations) obtains. In other terms, this picture seems to take for granted that a (mathematical theory-tied) belief that *p* is just the belief that the (mathematical) fact that *p* obtains, that is, that some appropriate (mathematical) objects are (ones with respect to each others) in a certain way.

This is quite natural to admit, from a platonist perspective, and does not seem, as such, to ask for any further explanation (that is, for any explanation beside those relative to the notions of a mathematical object and of a mathematical fact, and, possibly, to the conception of existence, that are here at issue). What is by far less natural to admit, and actually requires further explanation, is that a justification of a mathematical theory-tied belief that *p* be a justification that the mathematical fact that *p* obtains, that is, a justification that the relevant mathematical objects are (ones with respect to each others) in a certain way. Taking a justification of a mathematical theory-tied belief to meet this requirement is already to make a very strong assumption: an assumption that a platonist cannot merely take for granted, but has rather to account for.

Here is, in my view, the basic challenge that Benacerraf's dilemma addresses to a platonist: it asks him or her to explain how can a justification of a mathematical theory-tied belief, namely an argument supporting an axiom or another sort of liminal assumption of a mathematical theory, or a proof within such a theory, be a justification that a mathematical fact, conceived as a fact about the mathematical objects, obtains. The crucial question is then, according to me, not concerned with that which, according to a platonist, could ensure that the relevant mathematical beliefs reflect (in Field's sense) the facts about mathematical objects, but rather with

[12] Cf. footnote (11) above for the sense in which I deem the challenge specific.

[13] In all what follows, by speaking of a justification of a mathematical theory-tied belief, I refer (not to any possible argument, belonging to some abstract domain of arguments, and which one could take as a justification of this belief, but) to this consensual epistemic practice, that is, to an actual justification occurring within the relevant theory (or theories), or in relation to it (or them).

the very possibility of taking the justifications of these beliefs to be justifications that these facts obtain.

Consider a quite simple example, namely the belief that 5+7=12. It should be plain that this belief is both mathematical and (as opposed, for instance, to the belief that 5+7=13) theory-tied. One might have many different ideas about what should count as a justification of it, but it should also be plain that most of us would spontaneously consider that a proof that 5+7=12, or of '5+7=12', within an accepted version of arithmetic, is such a justification (and, even a suitable and reliable one, if it were admitted that this belief could also have unsuitable or unreliable justifications). Now, there is no doubt that such a proof justifies a belief. The point is whether a platonist can, in agreement with the spontaneous pronouncement of most of us, take this belief to be the very belief that 5+7=12.

According to Field's picture, for a platonist, this last belief is the belief that: *i*) there are the numbers 5, 7, and 12; *ii*) they are mind and language independent abstract objects; *iii*) they stay to each others in the additive relation that the statement '5+7=12' expresses. Hence, if this picture is admitted, the point is whether a platonist can take a proof that 5+7=12, or a proof of '5+7=12', within an accepted version of arithmetic, as a justification that facts (*i*)-(*iii*) obtain, or, at least—supposing that it has been previously justified, on other grounds, that facts (*i*)-(*ii*) obtain—that fact (*iii*) also obtains, rather that, merely, as a justification that it is a theorem of this version of arithmetic that 5+7=12, or that '5+7=12' is such a theorem.

To avoid any aside worry, let us focus, for the sake of the argument, on categorical versions of arithmetic, for example on PA2. Following a platonist, let us also take for granted—for the sake of the argument, again—that the singular terms of PA2, or of whatever other categorical version of arithmetic, have the same reference as the corresponding numerical terms we use, both in our ordinary informal arithmetic and in our every-day language, if these terms have a reference at all. It would be a crucial challenge for a platonist, as described by Field, that of explaining how it can happen that a proof within such a suitable accepted version of arithmetic justify that the facts (*i*)-(*iii*), or, alternatively (under the mentioned condition), the fact (*iii*), obtain.

A similar concern also applies to other sorts of mathematical theory-tied beliefs, namely those pertaining to the axioms or other liminal assumptions of mathematical theories. Take again a quite simple example, the axiom of unicity of the successor for natural numbers, in whatever of its (formal or informal) formulations, which, for the sake of the argument, anew, we take as being all about the same objects, if they are about some objects at all. According to Field's picture, for a platonist, believing this axioms is believing that: *i*) there are the natural numbers; *ii*) they are mind and language independent abstract objects; *iii*) any such number has a single successor. Now, many grounds have been offered for this axiom, and all of them indubitably justify a belief. What is far from clear is whether these grounds are justifications that facts (*i*)-(*iii*) obtain, or, at least—supposing that it has been previously justified, on other grounds, that facts (*i*)-(*ii*) obtain—that fact (*iii*) also obtains, rather than merely justifications that such an the axiom is a suitable axiom for a suitable version of arithmetic. Again, an obvious challenge for a platonist, as described by Field, is to explain how it can happen that the former option holds, rather than the latter.

The two cases are not equivalent. In the former, the justification is so regimented as to make any doubt about its internal correctness or accurateness immaterial. In the latter, the justification is either essentially informal, and then either open to doubts or plausible scepticism, or regimented within a meta-theory (as it happens for justifications based on proofs of completeness or categoricity), and then openly conform only to the second of the two options considered (since a proof within a meta-theory possibly justifies that some facts about the relevant theory obtain, but certainly not that some facts about that which this theory is about do). Still, this difference does not seem to me to affect the point that I want to make, since this point is not concerned with the accurateness or faultlessness of the justification, but rather with that which it is a justification of, and stands on its own feet even if it is conceded, for the sake of the argument, at least, that our formal and informal mathematical argumentations are all about the same objects, if they are about some objects at all. I take, then, the challenge to be the same in the two cases, and to generally apply to any mathematical theory-tied belief: it is the challenge of explaining how a justification of such a belief that *p* can be a justification that the fact that *p* obtains[14].

When it is put in this form, the challenge comes apparently close to the one to which Burgess and Rosen have ultimately reduced Benacerraf's dilemma: "granted that belief in some theory is justified by scientific standards, is belief in that theory justified?" (Burgess & Rosen 1997, p. 48). This reduction goes through many intermediary stages (which I shall account here only partially: one finds both a trustworthy succinct account, and a stringent criticism in Hale 1998, pp. 162-163). Burgess and Rosen begin with their own way to put Field's challenge to a platonist: if it is true that "when mathematicians believe a claim about mathematicalia, then that claim is true", then this deserves explanation (*ibid.*, pp. 41-42). They then admit (with

[14] Of course, if the second option considered in footnote (8) holds, to the effect that we are merely entitled by default to mathematical axioms or other sorts of liminal assumptions of mathematical theories, so that they have no justification at all, this challenge does not apply to our beliefs pertaining to these axioms or liminal assumptions. It is still easy to see how to restate the challenge in this case: what is to be explained becomes how can we be entitled by default to the belief that a mathematical fact obtains, rather than, merely, that it is appropriate, or even only legitimate to admit a certain axiom or liminal assumption. After all, under this option, the relevant entitlement by default reduces, as claimed by Field himself, to the mere circumstance that "we regard it as legitimate to have these beliefs […] and that we have no other commitments that entail that we should not so regard them" (cf. footnote (8)). Stating the challenge this way would clearly require changing something in what I shall say later about it. Still, it should not be hard to see how this could be done.

Field) that this explanation should not be required to concern "what follows logically or analytically from what", and restrict the issue to "axiomatic beliefs, ones that are not believed simply because they follow logically or analytically from other, more basic beliefs" (*ibid.*, p. 45). Next, they appeal to the possibility of rephrasing all mathematics within set theory (and to other connected considerations that we can leave aside here), to further restrict the challenge to the following: "granted that belief in standard set theory is justified by scientific standards, is belief in the truth of standard set theory justified?" (*ibid.*, p. 47). Finally, they argue, in agreement with what Benacerraf himself, together with Putnam, writes in the introduction to (Benacerraf & Putnam 1983, p. 35), that the problem is not "peculiar to mathematics" (Burgess & Rosen 1997, p. 47)[15], and remark, in agreement with Field, that truth is here to be intended disquotationally, so to arrive to their last formulation, which is that quoted above.

This long detour already suggests, however, that the way Burgess and Rosen look at their version of the challenge is, in fact, quite different from that I look to mine. This becomes quite palpable from the way they comment the former (Burgess & Rosen 1997, p. 48):

> Once it is put in this last form, it becomes clear that the […] challenge presupposes a 'heavy duty' notion of 'justification'—one not just constituted by ordinary commonsense standards of justification and their scientific refinements […]. To put the matter another way, once it is put in this last form, it become clear that the question or challenge is essentially just a demand for a philosophical 'foundation' for common sense and science—one that would show it to be something more that just a convenient way for creatures with capacities like ours to organize their experience—of the kind that Quine's naturalized epistemology rejects.

[15] Burgess and Rosen consider, indeed, that the last formulation quoted above "is in fact Benacerraf's (writing with Putnam in Benacerraf and Putnam (1983: editorial introduction § 9)". What they allude is possibly the following passage of Benacerraf and Putnam's introduction, where a similar question is raised (Benacerraf & Putnam 1983, p. 35):

> But why should the simplest and more conservative system (or rather, the system that best balances simplicity and conservatism, by our lights)[that is, the theory we prefer and adopt] have any tendency to be *true*? […] It is hard enough to believe that the natural word is so nicely arranged that what is simplest, etc. by *our* lights is always the same as what is *true* (or, at least, *generally* the same as what is true); why should one believe that the universe of sets […] is so nicely arranged that there is a preestablished harmony between *our* feelings of simplicity, etc. and *truth*?

This is even closer to an alternative way of posing the challenge that Burgess and Rosen also suggest (Burgess & Rosen 1997, p. 47):

> […] there is a *connection* which has not been explained. It is the connection between set theory's being something that creatures with intellectual capacities and histories like ours might, given favourable conditions for the exercise of their capacities, come to believe, and set theory's being something that is true.

It is then clear that what Burgess and Rosen see in the challenge is the request of an ultimate legitimisation for our theories, both mathematical and scientific, depending of what actually there is and how things actually are, a request that openly violates naturalism (cf. Liggins 2010, p. 73).

Though I do not see any reason for a philosophy of mathematics should conform to naturalism, I do not see the challenge this way. I rather see it as the request of a philosophical account of mathematics capable of explaining how mathematical theories, which are got and selected according to our standards, can speak of what they speak according to a platonist, and how, then, the justifications which are proper to them are justifications for what, for a platonist, they are beliefs that[16]. This is just what I'm asking when I ask for an explanation of how a justification of the mathematical theory-tied belief that *p* can be a justification that the fact that *p* obtains[17].

I do not see why, when it is so understood, the challenge should beg the question for a platonist described as in Field's picture. If it begged the question, what would beg the question would be the very requirement that a justification of a mathematical

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[16] I'm not sure whether Holland means something like this when he retorts to Burgess and Rosen's understanding of the challenge that this "is not a demand for an external justification of science; rather, it is a demand for the justification of the scientific character of belief formation about abstract mathematical entities" (Holland 1999, p. 239; cf. also Linnebo 2006, p. 552).

[17] Notice that this question cannot be appropriately answered by what Linnebo calls 'internal explanation': an explanation according to which "mathematicians' tendency to accept as axioms only true sentences is adequately explained by pointing out that the historical process that led to the acceptance of these axioms is a justifiable one according to the standards of justification implicit in the mathematical and scientific community" (Linnebo 2006, p. 561). Possibly, Linnebo is right in claiming that this explanation is "undefeated", if it intended to respond to Field's challenge (*ibid.*, p. 563). But it simply does not answer my version of the challenge, since what I'm asking to explain is just how can happen that the justifications issued by this historical process (that is, the arguments selected through it, in support of mathematical axioms and other liminal assumptions of mathematical theories, and the proofs within these theories) be justifications that some appropriate mathematical facts obtain. An internal explanation in Linnebo's sense no more answers Field's version of the challenge, at least insofar as this is understood as a demand of an explanation of how happened that this historical process lead mathematicians to (justifiably) have a mathematical (theory-tied) belief that *p*, just in the case that the fact that *p* obtains. Things go possibly differently with Linnebo's "external explanation", an explanation of "*what makes it the case* that the process is reliable", that is, of "why […] [mathematicians's] methods are conducive to finding out whether […] [mathematicians's] claims are true" (*ibid.*). According to Linnebo, in the case of perceptual knowledge, such an explanation should explain the correlation between claims "about physical objects outside of people's sensory surfaces" and methods used "for deciding whether to accept such claims", relying on "the verdicts of […][people's] senses" (*ibid.*, p. 564). This might suggest that what Linnebo is asking here is just en explanation of the correlation between mathematicians's justifications and mathematicians's claims, or possibly (theory-tied) beliefs (cf. *ibid.*, p. 569). If this is so, Linnebo comes here close to my version of the challenge, though he suggests, then, a way to meet it that is quite different from what I shall later suggest.

theory-tied belief do not be a justification of something else than what, for such a platonist, this belief is a belief that. In other terms, what would beg the question for such a platonist would be the very demand of offering whatsoever epistemology for mathematics that be both plausible and compatible with his or her views on what mathematics is (about).

When it is so understood, the challenge that Benacerraf's dilemma addressees to a platonist is concerned neither with the reliability of the relevant justifications, not with any other possible condition that these justifications could be required to meet (beside that of just being justifications of the relevant beliefs)[18]. This cuts short a number of questions, arguments, and counterarguments that are often evoked in discussions allegedly concerned with Benacerraf's dilemma. Let us leave, then, these questions, arguments, and counterarguments apart, and wonder which challenge, if any, the dilemma addressees to a combinatorialist (in Benacerraf's sense).

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[18] A way to make it clear is by remarking the difference between the setting which underlies this challenge and that which underlies a counter-example *à la* Gettier to the tripartite conception of knowledge. One could argue that no such a counter-example is possible for mathematical knowledge, but if one is possible, it should go as the following (an alleged counter-example for the case of logical knowledge has been suggested in Besson 2009, pp. 2-4). Suppose a character, Archy, which reads this, in a text-book of number theory, written (a little bit too hastily) by a distinguished mathematician: "a prime number is a natural number having no divisor other than 1 and itself". Suppose also that this text-book does offer no definition of natural numbers, taking for granted that these are the well-known numbers 0, 1, 2, etc., and that the just mentioned definition occurs at page 2, while at page 3, a perfectly usual and universally acceptable definition of the order relation SMALLER OR EQUAL TO on natural numbers is offered, and that Archy, after having read it (and before going head in his reading, up to come upon the definition of the strict-order relation SMALLER THAN, which is offered at page 4, and then before realising the difference between an order and a strict-order relation), draws from what he has learned until then, through a simple and perfectly correct deduction (that he completely accepts and trusts), that 1 is a prime number and, then, that there is a prime number smaller or equal to 2. Though there is such a prime number, actually, one could hardly admit that Archy knows it. A way for arguing that this is not a suitable counter-examples *à la* Gettier could be by observing that the source of Archy's justification, namely the definition he founds in the text-book he has at hand, is not an admissible source for a mathematical justification. But, were this criticism in order or not, what is relevant here is not whether the counter-example is well-taken, but rather that the setting which underlies it is essentially different from that which underlies the challenge that, in my view, Benacerraf's dilemma addresses to a platonist. In the former, there is no question whether the relevant justification is a justification of what the relevant belief is (taken to be) a belief that. This is simply taken for granted. What is questioned is rather whether the relevant justification is suitable for transforming the relevant true belief in knowledge. In the latter, things just go in the other way around. There is no question whether the relevant justification is suitable for transforming the relevant true belief in knowledge. Indeed, the question is simply not raised, since no appeal is done to the notions of truth and knowledge. What is questioned is rather whether this justification is a justification of what a platonist takes the relevant belief to be a belief that.

3. Benacerraf's Challenge to a Combinatorialist

A simple way to answer the former challenge is by denying that the mathematical fact that *p* is distinct from the fact that it is a theorem or a liminal assumption of an accepted mathematical theory that *p*. Insofar as it seems quite implausible (since contrary to the evidence coming from mathematical practice) that the latter fact reduces to the former, this depends on admitting that there is nothing like a mathematical fact that *p* other than the mere fact that it is a theorem or a liminal assumption of an accepted mathematical theory that *p*: for example, there is nothing like the mathematical facts that 5+7=12, or that any natural number has a single successor other than, respectively, the mere facts that it is a theorem of an accepted mathematical theory that 5+7=12, or that it is a suitable axiom for a suitable version of arithmetic that any natural number has a single successor.

According to Field's picture, this solution is not open to a platonist, however, since it is incompatible with the view that there are mathematical objects and that they are mind and language independent and abstract, unless our platonist were ready to admit that mathematical facts are not facts about these objects (which would make this view quite immaterial, as a basis for a philosophical account of mathematics). It is rather perfectly in line with the views about mathematical truth that Benacerraf calls 'combinatorial', according to which "the truth conditions for arithmetic [but one could in general say 'mathematical'] sentences are given as their […] derivability from specified sets of axioms", provided that such a derivability is broadly intended, or that the requirement of completeness (understood as the requirement that a truth value be assigned to each statement of the language of the relevant theorem) is abandoned, so as to avoid the difficulty depending on Gödel incompleteness theorem (Benacerraf 1973, p. 665). According to Benacerraf, the "leading idea" of these views is, indeed, "that of assigning truth values to arithmetic [but, again, one could in general say 'mathematical'] sentences on the basis of certain (usually proof-theoretic) syntactic facts about them" (*ibid.*).

The claim that a mathematical fact that *p* reduces to the fact that it is a theorem or a liminal assumption of an accepted mathematical theory that *p* does not rely on any non-disquotational notion of mathematical truth. Advancing this claim seems then a natural way for rendering the combinatorial views in a weakened setting where any such notion is dismissed, like in the setting of Field's reformulation of the challenge that Benacerraf's dilemma addresses to a Platonist. This suggests that the challenge the dilemma addresses to a combinatorialist could be restated, within such a weakened setting, as a challenge for a supporter of this claim.

Though in his original paper, Benacerraf does not insist too much on this matter, he quite clearly observes that combinatorial views are in need not only of a combinatorial account of mathematical truth, but also of a "new *theory of truth theories*" capable of relating combinatorial truth for mathematics to usual truth for "referential (quantificational) languages" (*ibid.*, p. 669). The challenge he addresses to a combinatorialist seems, then, that of providing such a new theory of truth theories.

Hence, if the notion of truth (for any sort of languages) is appropriately weakened, or even merely let out from the setting, a natural way for rephrasing the challenge is this: a combinatorialist should account for the way an analysis of a mathematical theory-tied belief that *p*, according to which its content is that the fact obtains that it is a theorem or a liminal assumption of an accepted mathematical theory that *p*, is related to an analysis of a non-mathematical belief that *q*, somehow connected with the mathematical theory-tied belief that *p* (for example the belief that '*p*' results from the axioms of the relevant theory by appropriate transformations licensed by the deductive rules of this same theory)[19], according to which the content of this belief is just that the fact that *q* obtains (this last fact being not further reducible to some other fact).

It seems to me, however, that there is more to be said on this matter. Since, even in presence of such an account, or better, before hoping to provide it, a combinatorialist should explain how the content of a mathematical theory-tied belief that *p* should be precisely determined under his or her view. Should this content be taken to be *i*) that the fact obtains that it is a theorem or a liminal assumption of a specified mathematical theory M that *p*, or *ii*) that the fact obtains that there is a (non better specified) accepted mathematical theory X of which it is a theorem or a liminal assumption that *p*, or again, *iii*) that the fact obtains that for any accepted mathematical theory X pertaining to an appropriate specified branch of mathematics, it is a theorem or a liminal assumption of X that *p*?

Moreover, once one of these options has been chosen, a combinatorialist should also explain what is to be taken as an appropriate justification of the relevant belief, and, possibly, do it so as to warrant that our customary and natural conceptions on what does it mean that an epistemic subject has a justified belief that *p* are conserved. For example, a proof that 5+7=12 within PA2 would certainly be a justification of the belief that 5+7=12, if this belief were analysed in agreement for the option (*i*) and M were identified with PA2, or in agreement for the option (*ii*) and PA2 were included in the relevant domain of accepted mathematical theories. But it would certainly not be enough to justify this belief, if it were analysed in agreement with the option (*iii*). It

[19] Note that such a belief is essentially different from the belief that '*p*' follows from the axioms of the relevant theory, or is a theorem of this theory: while the latter belief is a mathematical one (and is even, according to the granted reduction, a prototypical mathematical theory-tied belief), the former is not. The latter depends, indeed, on the intra-theoretical notion of following from, or of being a theorem, while the former is perfectly independent of any intra-theoretic notion and is merely justified by an empirical scrutiny of a system of appropriate inscription-tokens (together with the admission that such a scrutiny is enough for justifying a belief about the corresponding inscriptions-types). Moreover, the latter participates to the justification of the former (but certainly not vice versa), and this is just the reason for the two beliefs are connected, and a combinatorialist cannot, then, avoid accounting for the way the analyses that reveal their respective contents are related.

seems, then, that a combinatorial view about the content of mathematical theory-tied beliefs and their justification could be plausible only if it were capable of specifying what is the content of these beliefs, and what would count as a justification for them, in a way that would not be either utterly complex, or quite unfaithful to our customary and natural conceptions about this content and this justification.

I do not go further on this matter, since even a better explanation of the difficulties that a combinatorialist should overcome for meeting this request would require so many details as to need much more space than that my present note can take. I hope to have said enough for making clear how I understand the challenge that the Benacerraf's dilemma addresses to a combinatorialist, even if this is freed of any appeal to the notions of truth and knowledge.

4. Meeting Both Challenges at Once

One could think, at first glance, that the essential difficulty of the challenge to a platonist crucially depends on the difficulty, if not principled impossibility, of filling the gap between the mind and language independent abstract objects that a platonist takes to be there, and the human justifications that mathematical practice depends on. If it were so, one could also imagine to dissolve the challenge by advancing a characterisation of platonism alternative to Field's, or, at least, to overcome it by weakening the platonist thesis.

According to Field's picture, for a mathematical platonist: *i*) mathematics concerns appropriately specified objects and non-reducible actual facts about them[20]; *ii*) these objects are mind and language independent; *iii*) they are abstract. The more natural option would be that of retaining thesis (*i*), and abandoning theses (*ii*) and (*iii*). Call, then, 'minimalist platonist (about mathematics)' anyone who endorses (*i*), but is agnostic with respect to (*ii*) and (*iii*), an is, then, open both to endorsing and rejecting them[21].

Endorsing thesis (*i*) entails maintaining that there are mathematical objects[22], but doing that while disregarding thesis (*ii*)—that is, maintaining that there are mathematical objects, but admitting that they could be mind and/or language

[20] I say 'actual facts' to make clear that thesis (*i*) is not compatible with the view that the facts that mathematics is concerned with never obtain, since the objects that these facts are about do not exist (a view suggested by Field's arguments in Field 1980 and Field1982).

[21] Note that the negation of (*ii*), namely the thesis that mathematical objects (if any) are mind and/or language dependent, entails (*iii*), as well as, of course (the subjacent logic, here, being naturally classic), the negation of (*iii*), namely the thesis that mathematical objects (if any) are concrete, entails (*ii*), since the idea of concrete mind and/or language dependent objects appears inconceivable. Vice versa (*ii*) and the negation of (*iii*) are, of course, perfectly compatible to each other.

[22] Cf. footnote (20), above.

dependent (and, then, abstract)[23]—entails admitting that the existence of these objects is open to different conceptualisations, other than the ordinary one, according to which existence is a primitive intrinsic condition not submitted to any sort of specification. A minimalist platonist should, then, also concede this possibility.

According to him or her, the content of a mathematical theory-tied belief would continue to be, however, that a non-reducible fact about mathematical objects obtains, and a justification of such a belief should, then, continue to be a justification that this fact obtains.

One could retort either that such a minimalist platonist is no more a platonist, in fact, or that he or she has no more at his or her disposal enough explanatory power for suitably accounting for mathematical ontology and semantics. But it seems clear to me that taking the belief that 5+7=12 to be a belief about three distinct objects, namely 5, 7, and 12, and the belief that any natural number has a single successor to be a belief about a domain of distinct objects, namely natural numbers, and also admitting that the facts obtain that 5+7=12, and that any natural number has a single successor, and that these facts are non-reducible—which implies that the statements '5+7=12' and 'any natural number has a single successor' have a semantic structure that parallels their superficial syntactical form, and are true, at least disquotationally—is already adopting a strong philosophical view, endowed with a respectable explanatory power, both for mathematical ontology and mathematical semantics, one which is quite different from other views often defended in an anti-platonist perspective.

Hence, the problem with minimal platonism (the view defended by a minimalist platonist) seems to me to depend neither on its being too weak or not enough explanatory, nor on its openly being not platonist in spirit. It could rather depend on its being arduously specifiable without eventually admitting that mathematical objects are, after all, mind and language independent, which is the same as either coming back to platonism as described by Field, or embracing the quite unlikely view that these objects are concrete.

Still, if it were clear that adopting minimal platonism would make easy to plausibly meet to the challenge that Benacerraf's dilemma addresses to a platonist, not only in Field's, but also in my version, one could consider convenient to run the risk, even if this would make a new challenge appear, consisting in the request of an appropriate specification of this minimal view, capable of avoiding coming back to endorsement of thesis (*ii*). Unfortunately, this is far from clear, however: merely leaving open the possibility of taking mathematical objects to be mind and/or language dependent, in some way or another, does not provide an easy way for plausibly explaining how a justification of the mathematical theory-tied belief that *p* can be a justification that the fact that *p* obtains, if this fact is conceived as a non-reducible fact about mathematical objects.

[23] Cf. footnote (21), above.

The adverb 'plausibly' is crucial in this claim. It is intended to mean that the required explanation should not fall, *mutatis mutandis*, into the same difficulties as those the combinatorialist views fall into face to the challenge that Benacerraf's dilemma addresses to them. This would happen if one envisaged to easily provide the required explanation by letting mathematical objects to be nothing but the items fixed within our current mathematical theories.

Broadly speaking, this is the position defended, though in different perspectives, both by Shapiro's and Resnik's *ante rem* structuralism (Shapiro 1997, Resnik 1997), and by Linsky and Zalta's version of platonism within Zalta's object theory (Linsky & Zalta1995, Linsky & Zalta 2006, Zalta 1999, Zalta 2000). The problem with this position is that it is hardly compatible with meeting the challenge that Benacerraf's dilemma addresses to platonism without specifying what is a fact about mathematical objects, what is the content of a mathematical theory-tied belief, and what counts as a justification of such a belief, in a way which is not utterly complex, or quite unfaithful to our customary and natural conceptions about such a fact, content and justification. Let us see why.

Suppose that it were argued that, insofar as the fact that 5+7=12 is a fact about the items fixed within a certain version of arithmetic and there called '5', '7' and '12', this fact is nothing but the fact that a proof within this version of arithmetic ends with '5+7=12', and, analogously, that, insofar as the fact that any natural number has a single successor is a fact about the items fixed within a certain version of arithmetic and there called 'natural numbers', this fact is nothing but the fact that this version of arithmetic includes an axiom, or possibly a theorem, just asserting, in the relevant language, that any natural number has a single successor. Considering the way the relevant mathematical objects are identified, one could argue that these are non-reducible facts about them-—and, then, that the semantic structure of '5+7=12' and 'any natural number has a single successor' parallels the superficial syntactical form of these statements. But it would be harder to admit that this way of specifying these facts is faithful to our customary and natural conceptions about what are non-reducible facts about the numbers 5, 7, and 12, and natural numbers in general, since, according to these conceptions, these facts are not concerned with a particular theory, but merely with the numbers 5, 7, and 12, and with the natural numbers as such.

The same could be said if it were argued that the fact that 5+7=12 is nothing but the fact that the items fixed within a certain version of arithmetic and there called '5', '7' and '12' stand to each others in such a way that the value of the addition-function fixed within this same version of arithmetic, for the two first of them as arguments, is just the third, and, analogously, that the fact that any natural number has a single successor is nothing but the fact that the items fixed within a certain version of arithmetic and there called 'natural numbers' stand to each others in such a way that the successor relation fixed within this same version of arithmetic is functional and total.

Things would not go better if it were undertaken to overcome the difficulty by taking the foregoing facts to be nothing but the facts that, for any accepted version A of arithmetic, $5_{A} +_{A} 7_{A} = 12_{A}$ (or even $5_{A} +_{A} 7_{A} =_{A} 12_{A}$) and $\forall x[NN_{A}(x) \Rightarrow \exists! y[NN_{A}(y) \wedge SUC_{A}(x,y)]]$ (or $\forall_{A} x[NN_{A}(x) \Rightarrow \exists_{A}! y[NN_{A}(y) \wedge SUC_{A}(x,y)]]$), providing that the subscript 'A' indicate that the relevant statements have to be understood in one of the two previous ways, by taking the relevant version of arithmetic to be A. Since, though it is true that, so understood, these facts would not be concerned with any particular theory, conceiving them this way would still be not faithful to our customary and natural conceptions about what are non-reducible facts about the numbers 5, 7, and 12, and natural numbers in general, since according to these conceptions, these facts are not universal facts about versions of arithmetic, but just facts about the numbers 5, 7, and 12, and the natural numbers as such.

Moreover, if the facts that 5+7=12 and that any natural number has a single successor were so conceived, and it were also admitted (in agreement with minimal platonism) that the content of a mathematical theory-tied belief that *p* is that the fact that *p* obtains, it would follow that no proof within whatsoever version of arithmetic, and no argument supporting an axiom of whatsoever such theory could respectively justify the beliefs that 5+7=12 and that any natural number has a single successor. Only proofs or arguments within an appropriate theory of arithmetical theories could do it. Hence, as we do not have available any such theory (unless we considered that this is merely provided by historiography of mathematics), it would follow that we have no justification of these beliefs available (or that such a justification merely depends on historiographic remarks), which is, again, openly quite unfaithful to our customary and natural conceptions about what counts as a justification of these beliefs[24].

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[24] *Ante rem* structuralism could be taken as the view that mathematical facts are facts concerning structures, which are, as such, independent of specific theories, or are, at least, conveyed by different theories. For example, the facts that 5+7=12, or that any natural number has a single successor could be taken to be, according to *ante rem* structuralism, facts concerning the structure of progression as such, this structure being commonly conveyed by any appropriate version of arithmetic. Under this reading, these facts would not be universal facts about versions of arithmetic, but singular facts about a particular structure. A problem with this view is that either it requires that only categorical theories, all having the same model (under isomorphism), are appropriate rendering of a certain branch of mathematics (for example that only PA2, or other categorical theories having the same model as PA2 are appropriate version of arithmetic), which is quite implausible, or it depends on a notion of a structure (and on an identity condition for structures) allowing one to admit that different theories, having different models (under isomorphism)—for example PA2, ACA_0, RCA_0, and FA, to remain to the case of arithmetic—convey the same structure, which is not what *ante rem* structuralism in Shapiro's and Resnik's version admits. If, despite this and the difficulty it presents, this second route were taken, it would, moreover, be also necessary to explain how proofs within a certain particular theory, among those that convey a same structure, or arguments related with it can justify that facts about this very structure obtain, or to provide a general theory of these theories (that could certainly not be a general theory of structures), in which justifications for the obtainment of these facts can be

It seems, then, that the problems that Benacerraf's dilemma respectively rises for a platonist and for a combinatorialist lie more deeply than in the stage of the specification of these views where it is question whether mathematical objects are there independently of, or dependently on our intellectual activity, or whether mathematical facts just are, or, at least, are reducible to proof-theoretic facts. The problems are rather concerned with the question whether there is room for conceiving mathematical objects as being, as such, independent of mathematical theories, though maintaining that these theories are about them. In other terms: is there room for conceiving these objects as the objects about which these theories are, rather than, merely, as the objects that these theories are about?

There is no doubt that mathematical theories are, as such, human constructions. No sort of platonist or realist seems to have room for denying this. At most, one can argue that these constructions are (supposedly) about a transcendent reality. So, wondering whether a mathematical theory M is about objects that are independent of it is the same as wondering whether the cognitive subjects that have set M up, or those which work in M, or merely learn M can be credited, while doing this, with *de re* epistemic access to the objects that M is about, rather than merely with a *de dicto* epistemic access to them, that is, whether it can be said that it is with these objects that these cognitive subjects are dealing, while doing it, or it can only be said that these subjects are dealing with these objects, while doing it. In other terms, the question is whether mathematical objects can be fixed as individuals we have an epistemic access to—that is, individuals we can distinguish from others, and, when focusing on some of them one by one, also from each other—independently of M, to the effect that one can take them as the objects that M is about, and not merely take M to be about them.

It follows that a way for meeting both challenges at once (or, even, the only possible way for this) is by providing a plausible account of mathematics, according to which mathematicians and users of mathematics are credited with *de re* epistemic access to mathematical objects while they set a mathematical theory about them up, work in it, or learn it.

One could think that it is just for granting this that a platonist matching with Field's description maintains that there are mathematical objects, that they are mind and language independent, and that mathematics is about them. Still, maintaining this could, at most, grant that the same mathematical vocabulary can be used to speak of the same objects in whatever context of use, but not yet that one can have *de re* epistemic access to these objects. Since, it is just the possibility of this epistemic access to mind and language independent abstract objects that is put in question when Benacerraf's challenge is addressed to such a platonist (at least, according to my

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offered. The difficulties of solving these problems (and other well-known ones that *ante rem* structuralism present) apart, it also remains that any possible plausible solution of them (if any) would presumably be, once more, quite unfaithful to our customary and natural conceptions about what counts as a justification of mathematical theory-tied beliefs.

understandings of this challenge). Hence, conceding that a platonist matching with Field's description is not able to meet this challenge cannot but come together with acknowledging that such a platonist is not able to account for the possibility of having *de re* epistemic access to mathematical objects.

It seems, then, that a platonist can hope to account for this only if he or she is not only a minimalist platonist, but is also ready to definitively deny either that mathematical objects are mind and language independent (the foregoing thesis (*ii*)), or that they are abstract (the foregoing thesis (*iii*)). If the latter option is discarded as highly implausible, such a platonist should, then, maintain that mathematics is about abstract objects that we fashion through our intellectual activity (which reduces to deny thesis (*ii*)), and look for a way of accounting for our fashioning these objects that leaves open the possibility for us to have *de re* epistemic access to them.

In my view, what is distinctive of an object (either concrete and abstract) is just this: an object is an individual item—that is, an item apt to provide the putative reference of a singular term, or to count as an element of the putative range of a first-order quantifier (possibly in a multi-sorted first-order language, or in a multi-sorted first-order fragment of a higher-order language)—that some cognitive subjects can have a *de re* epistemic access to, that is, that there is, for these subjects, a way for dealing with it such that one can say that it is with it that these subjects are dealing, and not merely that these subjects are dealing with it. Existence, intended as a primitive intrinsic condition not submitted to any sort of specification, does not matter here: taking *a* to be an object does not require, to my mind, to admit that *a* exists in such a primitive sense. If one is willing to argue that an object exists—as I'm, indeed, for mathematical objects—one has to specify a peculiar sense in which it does, a sense that depends on its particular nature, better on the particular nature that is ascribed to it. Fashioning an abstract object, or a domain of abstract objects, consists, then, in my jargon, in fixing an individual item or a domain of individual items, in such a way as to make possible for some cognitive subjects to have a *de re* epistemic access to it (more on this matter in the next section).

Conversely, nothing that has not been so fixed, or that cannot be so specified as to make possible for some cognitive subjects to have *de re* epistemic access to it, can be taken to be an object. It is merely a logically appropriate reification of a concept or a sheaf of properties, a posit merely resulting from nominalising some predicates or associating them to some names, and stipulating that this is enough for ensuring reference. Hale and Wright seem to imply that something like this happens for places in structure, as defined in *ante rem* structuralism: according to them, by merely giving an "axiomatic description […] characterising a structure" we cannot "do more than *convey a concept*", namely we cannot "induce awareness of an articulate, archetypical *object*, at once representing the concept in question and embodying an illustration of it" (Hale & Wright2002, p. 113). I essentially agree. This does not mean, however, that providing an axiomatic description cannot result in, or be part of, fashioning some abstract objects in my sense. It is so, indeed, when this description is such to make possible, or to contribute to make possible, for some cognitive subjects to have *de re* epistemic access to the items it fixes, while dealing with them as the objects that a theory, which does not involve, as such, this axiomatic description, is about.

I have advanced above that a way for meeting at once both the challenges that Benacerraf's dilemma addresses to a platonist and to a combinatorialist is by providing a plausible account of mathematics, according to which mathematicians and users of mathematics are credited with *de re* epistemic access to mathematical objects while they set a mathematical theory about them up, work in it, or learn it. According to such an account, a theorem, an axiom, or any other sort of liminal assumption of such a theory are, indeed, to be intended as *de re* descriptions of these objects, or *de re* attributions of properties or relations to them. Hence, the content of a belief expressed by such a theorem, axiom, or liminal assumption is just that these objects satisfy these descriptions or conform to these attributions, and proving or supporting such a theorem, axiom, or liminal assumption is the same as securing these descriptions or giving reasons for these attributions. Moreover, insofar as these descriptions and attributions are *de re*, the same objects can be passible of other similar descriptions or attributions depending on other theories which are also about them, and any such theory is a way for speaking of them, that is, for describing them or attributing properties or relations to them.

What I have just said on what I take to be an object, and on what fashioning an abstract object or a domain of abstract objects consists of should make clear that this way of meeting these challenges is perfectly in line both with the view that mathematical objects are objects in a proper sense, that is, they are more than mere logically appropriate reifications of concepts or sheaves of properties, and with the idea that mathematics not only is a human production, namely the result of our intellectual activity, but it is also self-determining, does requires no exercise of any mysterious faculty allowing us to access to a transcendent reality, and no more depends on such a reality to be there. In short, this way of meeting these challenges is in line with the basic proposals of both a platonist and a combinatorialist.

What still remains to better explain is how I think possible to fashion an abstract object, or a domain of abstract objects, in such a way as to make possible for some cognitive subjects to have *de re* epistemic access to them, or, more precisely, how I think possible to fashion mathematical objects, in such a way as to make possible for some cognitive subjects to have *de re* epistemic access to them, while they set a mathematical theory about them up, work in it, or learn it. This is a quite complex question, indeed. Let me try, however, before concluding my paper, to outline the direction along which I think possible to respond to it, and then to provide the account of mathematics that I have invoked.

5. Fashioning Abstract Objects and Having *De Re* Epistemic Access to Them

The first thing to be said is that *de re* epistemic access to abstract objects we fashion cannot come together with the very act of fixing them. It cannot but come after it. Hence, the question is not whether and how we can have such an access to abstract objects while fixing them, but rather, whether and how we can fix them in such a way that we can, later, have such an access to them, namely describe them *de re*, or attribute properties or relations to them *de re*. This is perfectly consonant with the picture of mathematics suggested by platonism as described by Field. Since, according to this view, mathematical objects are just there before we can speak of them, describe them, or attribute properties or relations to them. Still, the sense in which we are said to attribute properties or relations to mathematical objects, and the way such an attribution interacts with a description are quite different in the two cases.

According to platonism as described by Field, we have no other intellectual capability relative to mathematical objects than that of recognising how they are. Any assertion on them is, then, in a proper sense, a description, or, at least, a tentative description. We can attribute properties or relations to them only insofar as we introduce appropriate conceptual tools for performing such a description, to the effect that the attribution, here, is properly part of the description. It follows that there is no intrinsic difference in nature between the intellectual activity we perform when we fix some mathematical objects and the intellectual activity we perform when we assert something about them: in both cases, what we perform is a (tentative) description. Typically, we do the former through appropriate definitions, and the latter through appropriate theorems. The definitions can be explicit, or implicit. In the former case, they either come after some axioms or liminal assumptions, and are licensed by them (which is generally the case in formal theories), or come before them and complete them (which is generally the case only in informal theories). In the latter case, they directly consist in some axioms or liminal assumptions. There is, then, no other intellectual activity we can perform with respect to mathematical objects than defining them, stating some axioms or liminal assumptions about them—which is often the same as defining them—, and asserting (after having derived) some theorems about them, and in any case, all what we do is (tentatively) describing them. The only difference is that the description provided by a theorem is secured by the description provided by the relevant definitions, axioms or liminal assumptions: the relevant objects cannot but be as the theorem asserts, if they are as these definitions, axioms or liminal assumptions do. But whether a statement about them is taken as a definition, an axiom or a liminal assumption, or as a theorem merely pertains to a choice that only depends on our ability in discerning properties and relations of the relevant objects, and/or on reasons of expository economy.

According to the picture I'm trying to offer, things are much more complex. For one thing is fixing mathematical objects, so as to make possible for us to have, later, *de* *re* epistemic access to them, another is dealing with them while having this access to them. The latter can be done, at least, in three essentially different way: we can merely select some of them among others; we can further specifying them or some of them by properly attributing to them new properties or relations, without modifying their intimate nature; we can recognising that they are so and so, that is, have some properties or stand to each other in some relations. Typically, we fix mathematical objects through appropriate definitions, either explicit or implicit, in the same connexion as before with axioms or liminal assumptions. This is in no way a description; it is rather a constitution: what we do is making these objects available, which could also be intended, under appropriate specifications, as the act of bringing them into existence. We can also have recourse to definitions, either explicit or implicit, again, and in the same connexion as before with axioms or liminal assumptions, while having *de re* epistemic access to mathematical objects and dealing with them. We do it, when we select some of these objects among others, or we further specifying them or some of them by properly attributing to them new properties or relations. Still, whereas in the former case, we merely perform a description of the relevant objects, in the latter we rather carry on their constitution, by making it more fine-grained, so to say: the attribution is, then, now proper, insofar as it is not part of a description, but rather of a constitution. Finally, we typically recognise that some mathematical objects, which we have *de re* epistemic access to, are so and so by asserting (after having derived) some theorems about them. This consists, again, in performing a description of these objects, though its function is, now, not merely that of distinguishing these objects from others, by emphasizing some of their features, but that of having a more fine-grained look at them, by making explicit what is only implicit in their definition, or in the axioms or liminal assumptions about them. In other terms, what we perform, now, is a description secured by a previous constitution, possibly involving previous selections and proper attributions.

An example should make my point clearer.

Suppose to have defined cardinal numbers as neologicists suggest, that is, as the values of the function from concepts to objects defined by Hume's principle. It is far from mandatory to consider that this definition identifies independently existing objects that are recognised, just because of it, as the cardinal numbers. One can rather take it as a skilful stipulation that merely fixes some abstract items as something we shall be later able to distinguish as such and to speak of. It does it without saying anything about any sort of property or relations of these objects, apart from their being values of this function. These objects are then fixed without telling anything else about them, in particular, without assigning to them any relation. One can then look at them, as such, that is, having *de re* epistemic access to them, and define some relations on them, or on some of them that we have previously selected among the others.

For example, if Hume's principle is stated as a proper axiom added to an appropriate system of second-order logic whose monadic predicate variables are intended to range on concepts, as neologicists suggest, one can define the successor relation on cardinal numbers, by appalling to one or another among many well-known equivalent formulas of this system of logic, then look at the cardinal number of the concept $[x : x \neq x]$, and show that those cardinal numbers that bear the weak ancestral of the successor relation to it form a progression under this weak ancestral, taken as a strict-order relation.

It seems quite clear to me that, by doing that, one is operating on the cardinal numbers as objects that are fixed in advance (through the Hume's principle), and that this is made possible because one has *de re* epistemic access to them: it is on these objects that the relevant strict-order relation is defined, which consists in a proper attribution of a relation to them and carries on their constitution. We, then, ground on this attribution for selecting the natural numbers among the causal ones. This is done by firstly selecting zero as the cardinal number of the concept $[x : x \neq x]$, and then emphasising the feature that same cardinal numbers have of bearing to it the weak ancestral of the successor relation. Finally it is of natural numbers, so selected among cardinal ones, that we prove that they form a progression under this relation, which entails that anyone of them has a single successor (merely saying that these cardinal numbers are to each others in a strict-order relation and form a succession does not precisely account for what happens, then). Asserting this is performing a description of these numbers, which is secured by a previous constitution, involving a previous proper attribution and selection.

Once this is done, one can continue and appeal to the successor relation, so as to define an additive operation on the natural numbers, then prove a number of additive theorems on them, for example that 5+7=12. Once more, it is on these objects that this operation is defined, and it is about them that these theorems are then proved (merely saying that these objects stay to each other in some additive relations does not precisely account for what happens).

One can say that, by operating this way, one is, in fact, working within a particular theory, namely Frege Arithmetic, and then falling into the same problem considered above for the justifications of mathematical beliefs coming from proofs within particular theories. But arguing this way would mean disregarding a crucial fact: that Frege Arithmetic is built up for dealing with objects that are fixed in advance, and perfectly independently of many of its ingredients.

One could even go up to argue that: *i*) Hume's principle, intended as a proper axiom added to an appropriate system of second-order logic, is nothing but a particular version of a more fundamental principle, namely a clear-cut enough, but still essentially informal stipulation assigning the same cardinal number to any pair of equinumerous concepts; *ii*) this more fundamental principle is enough, as such, for fixing cardinal numbers as abstract objects that we can, later, have *de re* epistemic access to. If this is admitted, one should also concede that Frege Arithmetic is built up for dealing with objects fixed in advance from its very beginning, that is, that also stating Hume's principle as a proper axiom added to an appropriate system of second-order logic is, in fact, nothing but a way for describing objects fixed in advance, so as to allow working on them in a convenient way. It would then follow that Frege Arithmetic is a theory of objects that we have *de re* epistemic access independently of it, and that we precisely have *de re* epistemic access to these objects while we set this theory up, work in it, or learn it.

Moreover, one could also argue that there is a way for informally defining addition on these objects as a binary operation satisfying a number of conditions specified relatively to them. If this is so, also the definition of addition within Frege Arithmetic can be taken as a way for dealing with a sort of relations that these objects bears independently of it, and there would, then, be room for claiming that a proof of an additive theorem within Frege Arithmetic is a justification of a belief whose content is independent of this theory.

Another possible objection to this line of argumentation could be that nothing ensures, *pace* Frege, that arithmetic is a theory of cardinal numbers intended as numbers of concepts, and, then, that an arithmetic belief is a belief about them. There is, indeed, no reason for crediting the identification of natural numbers with numbers of concepts with any sort of pre-eminence over other possible identifications of them, for example over their identification with ordinals, or, merely, with elements of a progression.

I agree on this: there is no reason for that. Still, what I want to argue for, by offering the foregoing example, is not that Hume's principle (either formally or informally understood) fixes natural numbers as they actually are. It is rather that there is a way for fixing abstract objects counting as natural numbers, so as to allow us to have *de re* epistemic access to them, while dealing with them within a version of arithmetic, and, then, independently of this version of arithmetic, or, even, independently of any version of arithmetic, or, at least, of most ingredients of any such version. There are certainly other ways for doing it. And there is also room for arguing that the exact contents of our beliefs that 5+7=12, and that any natural number has a successor are not fixed once for all, but can vary from context to context, community to community, or, even, subject to subject[25].

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[25] One could even imagine that, in some special contexts, the content of these beliefs is completely theory-laden, that is, that what one is believing by believing that 5+7=12 and that any natural number has a single successor is just that some proof-theoretic facts obtain. To my mind, the point is, then, not that of discovering or revealing what is the real content of these beliefs, but merely that of making clear that there is room for accounting for the possibility that this content be independent of a particular theory, though maintaining that these beliefs are justified through the usual arguments advanced in mathematical practice, which depend, instead, on particular theories. One should also avoid to confound, for example, the belief that 5+7=12 entertained by a professional mathematician or by a mindful user of mathematics—which is actually concerning natural numbers, or, at least, the natural numbers 5, 7, and 12—with the widespread belief that 5+7=12 entertained by mathematically uneducated subjects, possibly concerned with other objects,

I have neither space in this paper, nor enough clear ideas in my mind for arguing in favour of one of these possibilities, or for suggesting a possible alternative. My only present purpose is to make clear what is, in my view, the crucial basic challenge that Benacerraf's dilemma addresses to a plausible philosophy of mathematics, and to suggest that there is a possible way-out which is compatible with platonism, or, at least, with a platonist spirit.

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like the mere numerals '5', '7' and '12' involved in our usual decimal numeral system, or the countable collections to which these numerals are assigned. It seems clear to me that accounting for this latter belief and its justification is not a task of philosophy of mathematics, properly speaking, but rather of some branch of sociology or socio-linguistic. Whatever a philosophy of mathematics might argue for, concerning the belief that 5+7=12, should, indeed, be also applicable, *mutatis mutandis*, to other mathematical (or arithmetical, at least) beliefs that have no correlate among the widespread beliefs entertained by mathematically uneducated subjects. After all, what philosophy of mathematics is concerned with is mathematics, not the way mathematical vocabulary is unconsciously, or, at least, differently used in every-day life.

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