Problems with the recent ontological debate in the philosophy of mathematics

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Abstract: What is the role of mathematics in scientific explanations? Does it/can it play an explanatory part? This question is at the core of the recent ontological debate in the philosophy of mathematics. My aim in this paper is to argue that the two main approaches to this problem found in recent literature (i.e. the top-down and the bottom-up approaches) are both deeply problematic. This has an important implication for the dispute over the existence of mathematical entities: to make progress possible in this debate, we either have to find a different approach to the problem of the role of mathematics in scientific explanations (one that is not affected by the problems that, as I argue in this paper, plague the existing approaches), or we need to recast it in different terms.

1. Introduction

As it is well known, the most discussed and, arguably, the most powerful argument for mathematical realism is the enhanced indispensability argument. The gist of this argument is that we should extend our ontological commitment to include mathematical entities because, as is the case with other theoretical posits that we have no qualms accepting into our ontologies, they are indispensably used (or they play an indispensably explanatory role)[1] in our best scientific explanations. There are two main lines of attack against mathematical realism couched in these terms: (1) one targets the (alleged) explanatory role of mathematics in science, (2) the other questions the implications that the fact that mathematics can play such a role can have on our ontological commitment. In this paper, I am concerned with the quarrel generated by the first line of attack, more precisely with whether there is a way to push forward the dispute over the explanatory role of mathematics. As I will argue, we can distinguish in recent literature

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[1] It is important to note that these two, namely 'indispensably used in scientific explanations' and 'playing an indispensably explanatory role,' are not equivalent as some may think. For an in-depth discussion of this point, see Târziu (2018a).

between two main approaches to the problem of the role of mathematics in scientific explanation and both are deeply problematic.

2. The state of the debate

The most heated debate in the recent philosophy of mathematics gravitates around the role of mathematics in scientific explanation. On one side of this debate, we find the mathematical realists who adopt an *explanationist* stance on this problem (Baker 2005, 2009; Colyvan 2001; Baker and Colyvan 2011; Lyon and Colyvan 2008; Lyon 2012; Baron 2014)[2] and take mathematics as playing a genuinely explanatory part in scientific explanations, on the other we find the nominalists who prefer a *representationalist* stance (Melia 2000, 2002; Rizza 2011; Tallant 2013; Saatsi 2011; Daly and Langford 2009; Barrantes 2019) and argue that the role of mathematics in scientific explanations is only that of representing some explanatorily relevant physical facts. Choosing between these two positions can prove a difficult task, though, because there are prima facie reasons for not liking any of them (namely explanationism seems too strong while representationalism seems too weak). For a little glimpse at what some of these reasons are in my opinion, consider this. The most widespread ways to think about scientific explanation are in terms of dependence relations, nomic subsumption, and unification. But it can be hard to accept that something in the physical world depends in any way on mathematical facts,[3] or that mathematical theorems can be taken as laws of nature, or that they can figure between those hypotheses which make up the explanatory store which unifies the set of statements accepted by the scientific community. This may deter some from embracing explanationism.[4] Unfortunately, representationalism doesn't seem to provide us with a better alternative. Applied mathematics is almost never just representational, and, in the

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[2] There are, of course, explanationists that are not mathematical realists like Leng (2005b), Lange (2013) and Pincock (2015).

[3] Not everyone has qualms with this, though. Philosophers like Lyon (2012) and Pincock (2015), for example, don't find anything wrong with the idea that there are dependence relations holding between mathematical and physical facts.

[4] There are some philosophers (e.g. Molinini 2014) for whom the fact that these ways to think about scientific explanation don't accommodate mathematical explanations of physical phenomena counts as evidence that they are flawed and they need to be either abandoned or extended (cf. Saatsi 2016).

cases of alleged mathematical explanations[5] discussed in the literature, it is particularly hard to accept that the role of mathematics is just that of being a convenient way of expressing things about the physical world (see e.g. Bueno and Colyvan (2011, p. 351), Pincock (2012, p. 204). Some representationalists such as Balaguer (1998, p. 137) acknowledge that this position is actually an oversimplification).

Of course, prima facie implausibility doesn't usually act as a powerful discouragement for the philosophical adventurer (especially when she is motivated by some previous agenda to go in a certain direction). This explains, in my opinion, why the literature is full of examples of alleged mathematical explanations and their nominalized versions. In broad strokes, the discussion takes place along the following lines: the mathematical realist comes up with a case that for some reason (e.g. intuition, the indispensability of the mathematical part, what the expert practitioners are saying)[6] is considered to be a mathematical explanation, i.e. a scientific explanation in which mathematics plays an explanatory role. Then, the onus of showing that that case is in fact not a mathematical explanation falls on the unconvinced (see French (2015) and Saatsi (2016) for a different opinion about the burden of proof). The most common strategy employed by the latter is to search for a nominalistic alternative to the mathematical realist's example, i.e. an alternative in which the mathematical part is replaced by some relevant physical fact that is taken to bear the explanatory burden in both explanations. According to the explanationists, this strategy is deeply problematic because, first of all, it doesn't seem to sit well with the scientific practice. The examples discussed by explanationists are taken from science, so they have the approval of the scientific community, something that cannot be said about the nominalists' alternatives. Since scientists don't take the mathematics involved in these cases as having a merely representational role, we shouldn't either (Baker and Colyvan 2011, pp. 329-30; Saatsi 2016, pp. 1050-51). Second of all, the representationalists' strategy seems to work well only for those cases in which we deal with elementary applications of mathematics (Baker and Colyvan 2011, p. 325). In more complex situations we encounter big problems if we try to identify those features of the physical system that are allegedly indexed by mathematics (French 2015, p. 65). But, as Pincock argues, if "we fail to know much about

[5] By 'mathematical explanations' I mean throughout this paper 'mathematical explanations of physical phenomena.'

[6] For an extensive discussion of some of these reasons, see Târziu (2018a, 2018b).

these features, then merely picking them out does not constitute an adequate explanation" (Pincock 2012, p. 209).

Such arguments create the impression that there is actual progress being made in the discussion of the problem of the role of mathematics in scientific explanations and, implicitly, in the ontological debate over the existence of mathematical entities. My aim in this paper is to argue that this impression is false because the two main approaches (found in recent literature) to this problem are deeply problematic.[7] I will distinguish between a top-down approach and a bottom-up approach and I will argue that neither one can be used to push these debates forward.

3. The top-down approach

What role does mathematics play in scientific explanations? Can it play an explanatory part? In order to know if and understand why something can play a certain role in a particular context, we need to know two things: (a) what is in general needed for something to play that role, and (b) if the thing we are concerned with has what is needed. This may look like a trivial task, but it is actually a very hard problem to tackle. Let us consider the case of explanatory roles. According to one approach to this problem, let us call it *the top-down approach*, in order to know if a part of an explanation plays an explanatory part in it, we need to know what is in general needed for something to be explanatory and to determine if the part of the explanation we are concerned with satisfies the required criteria. So, what we need is a theory of explanation that lays down for us the criteria for explanatoriness, and an analysis that employs such a theory for determining what we are dealing with in a particular case.

Saatsi (2016) is an important recent advocate of this approach in the context of the ontological debate in the philosophy of mathematics.[8] In his opinion the "debates about the

[7] I want to emphasise here that I do not take these two approaches to be the only possible approaches to the problem of the role of mathematics, but the most popular in recent literature. Hopefully, this prevents the reader from (a) framing the discussion in this paper as a sort of dilemma according to which the debate proceeds either in a 'top down' manner or a 'bottom up' manner, and (b) from taking the conclusion of this paper as being that we have to abandon the debate because all the possible approaches are deeply problematic.

[8] This approach can be traced back to Baker (2005). According to Baker, what needs to be done to distinguish those scientific explanations that merely involve mathematics from the genuine mathematical explanations of physical phenomena is to show that "the mathematical component of the explanation is explanatory in its own right, rather than functioning as a descriptive or calculational framework for the overall explanation. This is difficult to do without having in hand some substantive general account of explanation" (p. 234).

‘explanatory role’ of mathematics (in the context of the indispensability argument) should be conducted much more closely in relation to specific accounts and conceptions of explanation” (p. 1066). Saatsi believes that the best way to “push the debate forward” is by bringing…

> "…our best understanding of scientific explanation to bear on the debate, and examine the notion of '(mathematics') explanatory role' in relation to different analyses and conceptions of explanation. In this way, we can hope to gain a much closer grip on 'explanatory role' so as to be better placed to judge whether mathematics plays the kind of explanatory role that actually matters for the ontological debate in question" (Saatsi 2016, p. 148).

Even though it may not be obvious at first sight, this approach is highly problematic. What is wrong with it? Well, in order to do what it is supposed to do, such an approach needs to be accompanied by a non-biased justification for choosing a particular theory of explanation, and there are good reasons for thinking that we cannot have such a thing.

When it comes to the top-down approach to the problem of the role of mathematics in scientific explanation, for example, the crucial question is this: how do we decide which theory of explanation to use in such a context? There are many theories of explanation that one can choose from, and it is not clear that we can eliminate bias in choosing one adequate for this task. For example, it makes a huge difference when dealing with an alleged case of mathematical explanation if we choose to look at it through the lenses of Strevens’s kairetic account of explanation or Woodward’s counterfactual account. While both accounts seem to countenance mathematical explanations (Woodward 2003, ch. 5.9; Strevens 2008, ch. 8.4), Woodward’s fits better with (or can be adapted to accommodate) the examples of non-causal explanations discussed in the literature (Bokulich 2008; Saatsi and Pexton 2013; Reutlinger 2016; Saatsi 2018). According to Strevens, what qualifies mathematics as an explanatory tool is its ability to represent relations of causal dependence (Strevens 2008, p. 331), so his account has a clear representationalist vibe. Which one of these theories should we adopt if we are concerned with assessing the role of mathematics in scientific explanations? Is there a way to answer this problem without the risk of being accused of being biased? If we try to answer the first question *only* by appealing to the kind of justifications in favour of one of these theories that are already given by their champions, it would not be of much help because it would not tell us what makes it relevant in the context of the discussion about mathematical explanations.

If, on the other hand, in answering it we start from the fact that it is relevant in some way to the issue of mathematical explanations, we cannot avoid being accused of being biased.

Adopting a pluralist perspective on explanation may seem like a good way out of this conundrum because such a perspective, by countenancing that no theory can accommodate all types of explanation, suppresses the need to choose between different theories of explanation. Unfortunately, this solution only pushes the problem to a different level without actually solving it. By adopting a pluralist stance on explanation we do indeed bypass the problem of deciding which theory of explanation to use when preoccupied with assessing the role of mathematics in scientific explanation. But this doesn't mean that (dubious) choice is no longer involved in the debate and so ceases to be problematic. When adopting pluralism we make a choice, and an important one at that: we choose between the view that all explanations can be adequately accommodated by one general account and the view that there is no monolithic theory that can encompass the complexity of the scientific explanatory practices. An option for the latter view based on the conviction that there actually are mathematical explanations is evidently biased.

Can it be shown that behind one's proclivity for pluralism there isn't some hidden ontological agenda or a previously acquired conviction that mathematics is indeed explanatory in science? No. Any attempt to do this would run into similar problems as those discussed above in connection to the issue of choosing the right theory of explanation for assessing the role of mathematics in scientific explanations: the justifications for adopting pluralism would be either irrelevant for the debate over the explanatory role of mathematics, or biased.

Aside from this, there are other reasons why adopting a pluralist perspective on explanation is not as good a solution to the theory choice problem as it may seem at first sight. In order for pluralism to help overcome this problem, two prerequisites have to be met: (a) to be already in the literature on scientific explanation (at least) a theory that can accommodate mathematical explanations, but without being specifically developed for this purpose; (b) such a theory has to peacefully coexist with other accounts. The first condition is necessary for a (pluralistic rescue of the) top-down approach to be possible – adapting a pre-existing theory, or developing a new one to make sense of mathematical explanations falls under the bottom-up approach. The second condition is important for avoiding running into the problem of theory choice. The thing is that, as far as I know, the explanationists have failed (*so far*) to show that condition (a) can be met. The only attempt to show that mathematical explanations fit a preexistent model of explanation is made in Lyon (2012).[9] According to Lyon, mathematical explanations are a "particular type of program explanation: ones in which the mathematics is indispensable to the programming" (pp. 566-7). But, as persuasively argued in Saatsi (2012), there are good reasons to believe that the Jackson-Pettit model cannot accommodate the cases of mathematical explanation discussed in the literature.

4. The bottom-up approach

An important strategy available to someone (of representationalist persuasions) who adopts a top-down approach to the role problem is that of launching a sort of general analysis, i.e. of trying to see if *any* theory of explanation can countenance the type of explanatory relevance relation needed for mathematical explanations to be possible (something like this can be found in Saatsi (2016), for example). The idea behind this strategy is that if the general analysis would produce a negative result, it would put the representationalist at a clear advantage, and so, it has the power to move the debate forward. Unfortunately, this strategy cannot deliver what is expected from it because one can argue that the top-down approach is inappropriate in the context of the discussion about mathematical explanations. To understand why this might be so, let us consider how cases and theories can be used to make sense of each other. In many (if not most) situations we use theories to understand specific cases, let us call this *the understanding context*. There are other situations, though, in which we reverse the direction and use cases to assess theories, we can call this *the evaluative context*. In which one of these contexts are we when discussing about mathematical explanations? Those who believe that a top-down approach is appropriate for this discussion are at least implicitly considering that we are dealing with an understanding context, but they might be mistaken because none of the theories found in the literature seems to provide a satisfactory understanding of such explanations. This may be taken to suggest that the context we are in is actually evaluative (remember the case of the DN-model of explanation!). In such a context, the only available approach to our problem is a *bottom-up approach*. According to this approach, to understand

[9] The discussion in Baron (2014), for example, cannot be included in this category because the account of explanation it employs, i.e. Rice's (2012) account of optimality explanations, was developed specifically for making sense of non-causal explanations. Someone may be tempted to include here, besides Lyon, Baker (2005). I don't think this is a good idea, though, because, in my opinion, what Baker is doing at the end of this paper is at least insufficient for justifying such an inclusion.

why something can play a certain role in a particular context, we need to extend or construct a theory that determines what is in general needed for something to play that role based on the characteristics of some cases (considered paradigmatic).

If we apply it to the problem of the role of mathematics in scientific explanation, the idea behind this approach is that we have a pre-theoretic way of knowing[10] that in some cases mathematics plays a genuinely explanatory role in science and, by analysing such cases, we can obtain a better understanding of their characteristics. This understanding, if sufficiently elaborated, can be encapsulated into a new theory of explanation, or it can be used to transform (modify or extend) an existing theory.

So, as the bottom-up approach to the role problem has it, there are situations in which we know that (b) is the case but more needs to be done to have an answer to (a), i.e. we know that the thing we are concerned with has what is needed to play a particular role, but we don't understand why that is the case, so we need to start from what we know and work towards constructing/transforming a theory that can adequately tell us what is in general needed for something to play that kind of role.[11]

This approach may seem very attractive, especially to those discontent with the traditional (armchair) way of doing philosophy, but, as I will try to show here, it is at least as problematic as the top-down approach.

The first thing we cannot help but notice if we take a closer look at this approach is the strangeness of its structure: it doesn't make much sense to claim that there are things of a certain kind ***x*** characterized by the fact that they instantiate property ***P***, while at the same time acknowledging that you don't really know much about this property. It makes even less sense to pretend to have identified several such things by looking for ***P***. I believe it is safe to say that statements of the form "***x*** is the thing that…" in which the ellipsis (meant to be replaced by some trait of the thing one is concerned with) is replaced by something nobody really knows

[10] The "pre-theoretic way of knowing" alluded to here amounts usually (in the recent literature) to a reliance on the opinions of the scientific community.

[11] In the realist camp, this approach is used, for example, in Baker (2005; 2009; 2012), but it is also adopted by antirealist explanationists like Pincock (2015) and Lange (2016). This approach has also been used recently in many interesting papers on mathematical and other non-causal explanations that, as the authors themselves claim, are orthogonal to the recent debate over the existence of mathematical entities and therefore are irrelevant to our topic and won't be discussed further in this paper, e.g. in Baron, Colyvan, Ripley (2017), Povich (2019), Baron (2019).

much about, are meaningless and therefore cannot be used in a significant discussion about ***x***. But isn't this exactly what some mathematical realists are doing when they claim to know that in a particular case we are dealing with an explanation in which mathematics plays an explanatory role and at the same time admit not knowing what exactly it means for mathematics to play that role?

We usually identify/classify things as being of a certain kind by appealing to (at least some of) their characteristic features. Not knowing what these are should at least discourage us from making claims regarding them, right? Why is this common-sense principle disregarded in the recent literature on mathematical explanation where people like Alan Baker provide examples of alleged mathematical explanations while admitting that they don't know what makes mathematics explanatory in such cases? I can think of two potential reasons for this. First of all, it can be linked to our general tendency to overestimate our ability to identify stuff. Most of the things in our world (e.g. other people, animals, plants, laptops, etc.) are fairly easily identifiable. This generates in many the prejudice that things are in general this way. Secondly, and more importantly, even in those rare situations in which we do realize that there are things that are not that easily identifiable, we still commit identification-related mistakes by misattributing special identification powers to other people (people we believe for some reason to be in a better position than we are to make adequate identifications). This second problem is especially important for our discussion because, in the recent ontological debate in the philosophy of mathematics, there seems to be an agreement between all those involved that…

> "… the philosophical analysis of explanation has to turn to science and interpret philosophically the intuitions of the practicing scientist. A good account of explanation should be able to reflect these intuitions" (Molinini 2014, p. 228).

So,

> "It is expert practitioners who should guide our judgements on cases and influence our philosophical theory of explanation" (Pincock 2015, p. 870).

Relying this way on what the scientists have to say and so "accepting the lead from scientific practice" (Saatsi 2016, p. 1051) may be seen as a good solution to the strangeness problem discussed above: it actually does make sense to claim that there are mathematical explanations while at the same time acknowledging that you don't know much about them because you can rely on expert practitioners for detecting such explanations. For instance, in the case of the explanation for why the life-cycle periods of the North American cicada are prime, one is justified in claiming that we are dealing with a mathematical explanation because:

> “The way biologists talk and write about the cicada case suggests that they do take the mathematics to be explanatory, and this provides good grounds, at least prima facie, for adopting this same point of view” (Baker 2009, p. 625; see also Baker and Colyvan 2011).

But is it a good idea to put our trust in what scientists are saying when it comes to (mathematical) explanations? What exactly makes them good explanations identifiers? As far as I know, there is no elaborate answer to this question in the literature, but I believe that the idea is that, by being expert practitioners, scientists have a special relationship with scientific explanations. What is supposed to make the relationship special is the fact that, being in the business of producing explanations, scientists *know how* to give explanations and this know-how comes with identification privileges. So, in Pincock’s words, we should let them guide our judgements and influence our theories. Strangely, this makes a lot of sense for many, if not most, recent philosophers. I say strangely because, outside philosophy, experts are not typically considered reliable when it comes to expressing their (second-order) knowledge about their practice. Actually, there is an entire literature in cognitive psychology and artificial intelligence (knowledge engineering) devoted to the problem of knowledge elicitation, i.e. the problem of acquiring (second-order) knowledge from experts (i.e. knowledge about what it is that they are doing when they are doing *x*, where *x* is to be replaced by the domain of expertise).[12] As only a brief look at this literature reveals, eliciting knowledge from experts is far from being an easy task (achievable, for example, by paying attention to what the experts are saying)[13] because

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[12] To prevent a potential misunderstanding, I have to emphasize at this point the fact that, even though in knowledge engineering the purpose of knowledge elicitation is that of building a computer program, "the problem of knowledge elicitation" doesn't concern putting the knowledge of experts in a form suitable for developing expert systems, but it has to do with obtaining such knowledge in the first place.

[13] In the recent literature, the interview is considered an unsatisfactory method for eliciting knowledge from experts: “One of the major problems that has become apparent is that interview techniques are of only limited value in extracting procedural knowledge from human beings. The possession of rules does not imply any ability to describe them verbally. Take natural language as an example. We all 'know' the grammatical and semantic rules

"experts often have great difficulty in consciously accessing and in expressing their knowledge" (Wallis 2008, p. 130). This is sometimes referred to in the literature as *the paradox of expertise*:

> "As individuals master more and more knowledge in order to do a task efficiently as well as accurately, they also lose awareness of what they know" (Johnson 1983, p. 79).

Of course, this doesn't deter the experts from talking about what they believe they know and what they think they are doing. This, of course, makes things worse for someone who wants to extract knowledge from experts, because, besides not being completely aware of what it is they are doing, experts can mislead us by offering bad accounts of their expertise. This is not the place to get into much detail about the problems faced by cognitive psychologists when trying to elicit knowledge from experts, but it is important to emphasise the fact that they found evidence that there is in general a poor correlation between what the experts are saying and what they are actually doing (Johnson 1983, p. 83; Chervinskaya and Wasserman 2000, p. 44). So, pace most recent philosophers, we should be very careful when we interpret philosophically what the expert practitioners are saying about particular cases because, very often, experts represent their knowledge in a simplified and distorted way (Johnson 1983, p. 82; Berry 1987, p. 147; Cooke 1994, p. 803; Chervinskaya and Wasserman 2000, p. 44).

Returning now to the question asked above, I believe it is safe to say that, given the evidence (amassed in cognitive psychology and artificial intelligence) that experts are poorly capable of reliably expressing what they know, it is not a good idea to trust what scientists are saying when it comes to mathematical explanations.

But even if this was not the case, i.e. even if we were not justified to believe that experts are in general unreliable when it comes to articulating their knowledge, there are other powerful reasons for thinking that it is inappropriate to rely on expert practitioners for identifying those explanations in which mathematics plays a genuinely explanatory role.[14] To make adequate identifications, scientists would need to be able to determine the explanatory worth of different parts of an explanation. But this involves a lot of knowledge about the structure of explanations and about explanatory relevance. What reasons do we have for believing that scientists possess

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of our native language in that we can generate and understand an unlimited number of sentences. Few of us have any ability to describe or write such rules down, however" (Evans 1988, p. 112).

[14] Similar thoughts can be found in Knowles and Liggins (2015, p. 3404).

such knowledge? The only uncontroversial thing we can say about them is that they can give explanations, but being able to give explanations doesn't depend on having this type of knowledge any more than knowing how to speak one's native language[15] depends on knowing something about the grammatical, semantic and morphological rules of that language. When we use our language we do obey rules, of course, but we are not aware of them at least until we find out about their existence (most probably at school). I believe the same applies to giving explanations, except in this case there is no agreed-upon view about what explanations are and what is involved in giving one. If this is right then we cannot rely on the fact that scientists are experts when it comes to giving scientific explanations for attributing them the type of knowledge needed by someone to adequately identify those explanations in which mathematics is explanatory; and, since there is no other source of knowledge about explanations to which they could have privileged access to, it is safe to say that they lack the identification privileges attributed to them in the recent literature on mathematical explanation.

Scientists simply cannot play the role attributed by philosophers in the discussion about (mathematical) scientific explanations. But, if this is the case, then the solution to the strangeness problem discussed above is not good: there are no expert practitioners we can rely on for pre-theoretically sorting the mathematical from the rest of the scientific explanations. Is there anything else that can be done to avoid this problem? Yes, we can appeal to intuition and claim that, even if we don't have yet an account for this type of explanation and we don't know much about them, intuitively mathematical explanations are different from the rest of scientific explanations. An obvious problem with this solution is that relying only on intuition for identifying those explanations in which mathematics plays an explanatory part is not at all desirable for someone interested in seeing this debate move beyond a simple parade of intuitions about a bunch of cases (this worry has been expressed by Baker (2012, p. 246) and Saatsi (2016, p. 147)).

A less obvious problem with appealing to intuitions is that there are good reasons to believe that what is claimed to be intuitions in the context of this debate is most likely something else, namely *biased choices to see things in a particular way*.[16] To see that this is indeed the case, it is important to acknowledge that in the context of an ontological debate

[15] This may not apply to additional languages for the reasons discussed further.

[16] The idea here is not to make an empirical claim regarding the intuitions of philosophers, but to try to generate some doubt about whether they really have the intuitions that they claim they have.

one's 'intuitions' cannot be divorced from one's ontological agenda. Someone with realist affinities will, no doubt, tend/prefer to see (at least some of) the examples discussed in the literature as cases of mathematical explanations and search for/try to develop accounts that can accommodate such explanations. Someone with antirealist proclivities will surely see things differently. But, if one's beliefs about something are subservient to the argumentative needs of the ontological position one upholds, it is unlikely that intuition is their source. So, instead of dealing with intuitive insights about the explanatory role of mathematics in science, it is more likely that, in the recent ontological debate in the philosophy of mathematics, we encounter biased choices to consider mathematics as explanatory or not driven by the needs of the positions involved in this debate. I believe it is obvious why this reflects badly on the whole debate.

Before ending this section, I believe it is important to say a few words about the method of reflective equilibrium because someone can be under the impression that this method is very close to what I call here the bottom-up approach. This impression can generate two different reactions to my arguments: (a) either take what I am saying in this section as aimed at a distorted (or poorly understood) version of this method and so as useless, (b) or take this method as the way out of the problems that, I argue, affect the bottom-up approach.

Typically, the method of reflective equilibrium is presented in the literature as involving three stages (see, for example, Scanlon 2003 and Cath 2016). In the first stage, we start with a set of beliefs/intuitions about particular features of some relevant domain – in our case, we start with one or several cases of what we consider to be mathematical explanations of physical phenomena and with what we believe to be the characteristic features of these cases. In the second stage, we try to provide some theoretical principles that can account for the initial beliefs – we try to provide an account of what makes mathematics explanatory in the cases presented in the first stage. The last and most important stage of this method consists in trying to solve the conflicts that most likely occur between the initial set of beliefs/intuitions and the set of theoretical principles, by moving back and forth between them and making revisions until we achieve reflective equilibrium, i.e. until we manage to fit the two sets into a coherent scheme.

If we concentrate our attention only on the first two stages, the method of reflective equilibrium may not look very different from the bottom-up approach to the problem of the role of mathematics in scientific explanations. As I said above, what characterizes this approach is the fact that we start from one or several cases of mathematical explanations and we search for an account of what makes mathematics explanatory in those cases. This (apparent) similarity may trick us into thinking that what some of the philosophers concerned with mathematical explanations are doing is developing theories via a reflective equilibrium method. If this is right, then my arguments in this section are wrong because when using the method of reflective equilibrium there is no need to start from justified beliefs. Indeed, the whole point of this method is to *form justified beliefs* about a relevant domain. The problem with this interpretation is that it overlooks the importance of the third stage for the method of reflective equilibrium. This method can generate justified (coherent) beliefs about a target domain only if this stage is properly executed, but none of the philosophers concerned with mathematical explanations that adopt a bottom-up approach (Baker, Pincock, Lange, Povich, Baron, etc.) engage in the sort of back and forth between examples and theory characteristic for the method of equilibrium. Also, these philosophers have a completely different understanding of the role of the starting cases than that presupposed by the method of equilibrium, because their appeal to what the scientists are saying is clearly meant as a justification for their beliefs that the cases under discussion are indeed cases of mathematical explanations.

So the first potential reaction to my arguments is wrong: I have not mistaken here the method of reflective equilibrium for something else and so I am not arguing against a distorted (or poorly understood) version of this method. How about the second reaction? Is reflective equilibrium the solution to the problems discussed here in connection to the bottom-up approach? I remind the reader that what we are concerned with in this paper is whether there is a way to make progress in the recent ontological debate in the philosophy of mathematics. So what we want to know is this: can adopting the method of reflective equilibrium help with advancing this debate? No, I don't think so. Actually, a notorious problem with this method is that it cannot adjudicate central philosophical controversies, as the one discussed in this paper (see, for example, the discussion in McPherson 2015). This is obviously not the place to get into details, but the idea is that "different people may have very different initial beliefs and, hence, might reach different equilibria when they apply this method" (Cath 2016, p. 221) and so an impeccable employment of this method is compatible with reaching conflicting views.

5. Some ideas about the way forward

There was a time when the ontological debate in the philosophy of mathematics didn’t revolve around the role of mathematics in scientific explanations, but around the indispensability of mathematics in science. This changed, though, because several philosophers from both sides of the debate (most notably Field (1989), Colyvan (2001; 2002), Melia (2000; 2002), and Baker (2005)) considered that the debate would benefit from being recast in terms of the explanatory role of mathematics in science. The key idea was that doing this was beneficial for the discussion because it created a common ground between the mathematical realist and the scientific realist – “the indispensability debate only gets off the ground if both sides take IBE seriously, which suggests that *explanation* is of key importance in this debate” (Baker 2005, p. 225). Taking "IBE seriously" meant putting the problem of the explanatory role of mathematics in science at the centre of the ontological debate in the philosophy of mathematics. But, as I tried to show above, the two main approaches to this problem found in the literature are deeply flawed. So, recasting the debate in these terms did nothing to steer it towards a better path.

What does all this mean for the ontological debate concerning the existence of mathematical entities? As I see it, there are two options available to someone interested in this debate: the most obvious one is to try to find a different approach to the problem of the role of mathematics in scientific explanations (one that doesn't run into the same kind of problems as those discussed above); another option is to try to recast the debate in different terms. I have nothing to say about the first option – I have no intuition about how such an approach should look like, and so I don’t have a clue if such a thing is possible or not.

Fortunately, the second option is not as opaque (for me) as the first one. An interesting way of pursuing this option is by following Charles Chihara's advice to enlarge the target of our investigations by focusing also on pure mathematics because the applicability of mathematics in science is neither the only nor the most important aspect of mathematical practice that requires investigating.

Some philosophers are discontent with the fact that the recent discussion is concentrating too much on the role of mathematics in science and disregards a very important thing, namely what is going on in pure mathematics. These philosophers would certainly prefer an ontological dispute that pays proper attention to what mathematicians are doing to advance pure mathematics. I believe Charles Chihara is representative of this way of seeing things. In Chihara (2010) he argues that "developing one's view of mathematics by focusing almost solely on applications of mathematics in science can lead to a distorted understanding of the nature of mathematics. This is because, although applications of mathematics can teach us much about the nature of mathematics, there is also much to be learned about mathematics from considerations of pure mathematics, where matters of applications carry no weight at all" (p. 174). Chihara illustrates this point by discussing the shortcomings of Hartry Field's nominalistic view of mathematics. Field's view is, in Chihara's opinion, representative of the doctrines developed by those concerned first and foremost with accounting for the applicability of mathematics in science. As Chihara argues, this view turns out to be inadequate if one is interested in understanding “the many uses of structures in pure mathematics, where reference to, and reasoning in terms of, structures have been found to be strikingly fruitful, even though the value of such uses cannot be adequately understood or explained in terms of the Conservation Principle” (p. 161).

This is one of the options available to those still optimistic about the prospects of finding a good path for making progress in this debate, but there is another option that I believe we ought to take into consideration, namely to abandon the debate in its current form. Several things can make one give up on this debate, the most important of them is the belief that, for some reason, we can *never* find a good argument for or against one of the sides in this debate[17] (i.e. there is no way to settle the ontological dispute over the existence of mathematical entities). The best-known argument in support of this belief is given in Balaguer (1998). Here it is argued that we have to accept this strong epistemic conclusion because (i) the alleged mathematical realm is epistemically inaccessible, and (ii) the only defensible version of Platonism and nominalism are very similar when it comes to their view on mathematical practice so there are no consequences of these doctrines that can be tested against it (Balaguer 1998, pp. 163-169).

With a bit of work, Balaguer’s second point can be used (independently of how convinced is one by his arguments for the strong epistemic conclusion) to provide a different justification for abandoning the debate. A way to do this is by arguing along the following lines: if different ontological doctrines are similar with regards to their accounts of mathematical practice, but what we are (or at least should be) interested in as philosophers of mathematics is understanding the practice of pure mathematics, then we should abandon any concern with ontology. This compelling argument is given by Mary Leng. In Leng (2005a) she argues that “philosophers of mathematics would do well to set aside worries about ontology and think instead about some other worries that remain regardless of one’s ontological biases”

[17] I’m paraphrasing here Mark Balaguer’s well-known strong epistemic conclusion (Balaguer 1998, p. 151).

(p. 71). In Leng's opinion, it is not the job of philosophers of mathematics but of philosophers of science to worry about mathematical ontology.

I am very sympathetic to these arguments for abandoning the debate. What I said above doesn't support, though, anything as general as Balaguer's conclusion. I believe it does nonetheless at least raise a concern about whether there is such a thing as a good path for making progress in this debate (and if there is, what should it look like?). My preferred answer to this problem, if we take it to concern only the recent debate, is similar to that of Mary Leng's. As I hope it is clear from the above discussion of it, the current ontological debate over the existence of mathematical entities led to the concentration of most of the philosophical energy towards finding cases of genuine mathematical explanations in science and of a theory of explanation that would accommodate such explanations, i.e. a theory that can account for the way mathematical facts explain physical phenomena. But doing this doesn't contribute in any way to our understanding of the phenomenon of mathematical practice[18] – the thing philosophers of mathematics should be primarily concerned with.[19] So, instead of wondering about the best way to make progress in the recent ontological debate, we should abandon it (and let the philosophers of science worry about the alleged explanatory role of mathematics in science).

6. Conclusions

Can we solve (or at least make progress towards solving) the problem of the role of mathematics in scientific explanations? Two recent philosophical debates would benefit greatly from finding a good path towards answering this question: [20] the ontological debate in the

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[18] I do not provide any arguments in this paper in support of this thesis. I rely entirely for this on what is said in Leng (2005a).

[19] Just to be clear, I am not in the slightest an adept of the extreme position that philosophers of mathematics should be concerned exclusively with pure mathematics or that they should not be interested in ontological questions. I believe that providing an account of the nature of mathematics is impossible without answering ontological questions, and I also believe that Shapiro is right to claim that any philosophy of mathematics that does not provide an account of the relationship between mathematics and science is incomplete at best (Shapiro 1997, p. 244).

[20] Somebody can complain that what I am doing here is casting a general worry that would apply to any philosophical dispute. This may indeed be the case (I am a bit sceptical, though, that my arguments are so widely

philosophy of mathematics, and the dispute in the general philosophy of science over the existence of non-causal scientific explanations. My aim in this paper was to argue that those approaches to this problem that dominate the recent literature are deeply problematic. We can distinguish between two such approaches: in the first one, we start from a theory, and in the second one we start from a case and work towards developing a theory. If the first path is chosen, one needs to come up with a non-biased justification for why a particular theory of explanation should be used for the task of trying to answer this problem. But, as I argued in section ***3***, it is impossible to do this. If the second path is chosen, one needs to show that there is some pre-theoretical way of knowing that in some cases of scientific explanations mathematics plays a genuinely explanatory part – usually this amounts to nothing more than a reliance on what the scientists are saying. Without such a thing, it would be impossible to make a convincing case that the account one arrives at is not *ad hoc*, i.e. specially tailored to serve one's ontological agenda. But, as I argued in section ***4***, the scientists (and the expert practitioners in general) are unreliable when it comes to articulating their knowledge and, in the case of mathematical explanations, it is not at all obvious that they possess the kind of knowledge needed by one to adequately identify those explanations in which mathematics is explanatory.

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applicable), but I don't see why it should bother us. After all, every methodological discussion about a particular debate is bound to reverberate in other parts of philosophy.

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