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A Role for Mathematics in the Physical Sciences

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Abstract

Conflicting accounts of the role of mathematics in our physical theories can be traced to two principles. Mathematics appears to be both (1) theoretically indispensable, as we have no acceptable non-mathematical versions of our theories, and (2) metaphysically dispensable, as mathematical entities, if they existed, would lack a relevant causal role in the physical world. I offer a new account of a role for mathematics in the physical sciences that emphasizes the epistemic benefits of having mathematics around when we do science. This account successfully reconciles theoretical indispensability and metaphysical dispensability and has important consequences for both advocates and critics of indispensability arguments for platonism about mathematics.

I.

Two plausible principles seem to be responsible for much of the mystery surrounding the role of mathematics in our physical theories. On the one hand, mathematics plays a central role in these theories, in the sense that no non-mathematical alternatives are available. This near universal use of mathematics in the physical sciences reflects what I will call the *theoretical indispensability* of mathematics. That is, when we come to formulate physical theories that we take to be the best candidates for the truth, i.e. those that are best confirmed by the evidence we have, we find mathematics playing a central and undeniable part in these theories. On the other hand, there is a broad consensus that mathematical entities, if they exist, do not play a causal role in the happenings of the physical world. Whatever it is that mathematics is about, the mathematical domain is causally isolated from the physical domain. It is true that some deny this causal isolation because they interpret mathematics to be about physical things, e.g. Lewis' proposed megethology, which interprets mathematics in terms of concrete wholes and their parts (Lewis 1991, 1993). But even on these physicalistic interpretations of mathematics, where mathematical entities do bear causal relations to physical systems, it is not in virtue of these causal relations that the mathematics helps in scientific theorizing. Whatever causal relations might result from such an interpretation seem irrelevant to the value of mathematics for science. I will call this view that mathematical entities have no relevant causal role in the physical world *metaphysical dispensability*.[1]

One's reactions to indispensability arguments for platonism about mathematics no doubt turn in large part on which of these two principles one is inclined to emphasize. Advocates of such arguments, such as Colyvan, draw attention to the theoretical indispensability of mathematics (Colyvan 2001). Given that we must determine our ontological commitments using the most attractive regimentation of our best scientific theories, and given that our best scientific theories clearly do need mathematics, it is supposed to be inevitable that we include mathematical entities of some sort in our ontology. Still, this sort of argument does little to address the concerns of those philosophers of a more metaphysical bent who are impressed by the metaphysical dispensability of mathematics. Surely, they reply to indispensability arguments, we see clearly that mathematics plays a different role in physical theories than, say, theoretical entities, and we can capture the difference by articulating a metaphysics consistent with the metaphysical dispensability of mathematics. Thus, even if we cannot articulate non-mathematical physical theories, it remains prudent to exclude mathematical entities from our ontology, and admit only those entities whose causal role in the physical world we are confident of.

Both attempts to privilege one principle while ignoring the other are, to say the least, philosophically unsatisfying, and a common reaction to this situation is to go on to present arguments denying the principle one is unsympathetic with. Most famously, Field not only argued that mathematics is metaphysically dispensable, but tried to show that mathematics is also theoretically dispensable by actually formulating what he thought were non-mathematical and attractive versions of some parts of classical mechanics (Field 1980, 1991, 2001). Defenders of theoretical indispensability responded by insisting that Field's non-mathematical theories are either mathematical in the end or else so different from our ordinary scientific theories as to be relatively unattractive (MacBride 1999). While these criticisms of Field seem to have undermined his specific approach, a more recent flurry of attempts to block the connection between theoretical indispensability and ontological commitment show that advocates of indispensability arguments have yet to carry the day. Here I am thinking of Balaguer, Azzouni and Yablo, who despite their differences we might group under the label "fictionalists" (Balaguer 1998, 2001, Azzouni 2004, Yablo 1998, 2000, 2001, 2002). These fictionalists insist on metaphysical dispensability and, in various ways, use it to adjust their ontological commitments even when they cannot formulate non-mathematical versions of our best physical theories.

The temptation for one impressed by theoretical indispensability is to either somehow try to deny metaphysical dispensability or fall back on what I will call impatient naturalism. In the next section I will explore some attempts to deny metaphysical dispensability, and argue that these attempts fail. Still, the main idea pursued in that section will be extremely important in suggesting a way to reconcile theoretical indispensability with metaphysical dispensability. To anticipate my main conclusion, I will argue that mathematics allows us to make claims about higher-order or large-scale features of physical systems while remaining neutral about the basic or micro-scale features of such systems. This role for mathematics is essential to our ability to formulate physical theories that, at any given time, are confirmed by the evidence we have. Thus, any attempt to theorize without mathematics will lead to theories that are less well confirmed by the evidence typically available. While the resulting position successfully reconciles theoretical indispensability with metaphysical dispensability, it is not a complete vindication of indispensability arguments for platonism. For the only way mathematics can play this crucial role is if pure mathematics receives a large measure of confirmation *prior* to its application. So, I am not in any way attempting to argue for platonism on the basis of the role of mathematics in physical theories, but am only trying to clarify this role in a way consistent with theoretical indispensability and metaphysical dispensability.

Before I turn to some attempts to deny metaphysical dispensability, I want to briefly describe the position I call impatient naturalism. An impatient naturalist accepts both theoretical indispensability and metaphysical dispensability but rejects outright the proposals by Field, Balaguer, Azzouni, and Yablo by an appeal to the standards of scientific practice. These standards supposedly conflict with the non-literal or non-standard readings that fictionalists give to our best physical theories or undermine the scientific credentials of Field's alternative non-mathematical theories. A typical impatient naturalist demand, made by Burgess & Rosen, is for Field to submit his non-mathematical theories to a physics journal for evaluation (Burgess & Rosen 1998, p. 206, Burgess 1983). While entertaining as rhetorical flourishes, such demands leave a serious explanatory gap, one which I aim to fill in this paper. Every brand of naturalist must accept that scientists use mathematics in their physical theories and must assign some weight to this practice. But to stop there and refrain from any further investigation into why scientists might do this or how such a practice might contribute to the goals of science is an unattractive form of naturalism. An analogy will hopefully make the contrast between impatient and patient naturalism clearer. Suppose we are struck by the scientific practice of different labs repeatedly attempting to reproduce experimental results announced in journals. All naturalists must take this practice seriously and refrain from criticizing it for non-scientific reasons.

Some may still wonder why scientists do this or how this practice contributes to the goals of science. An impatient naturalist attempts to close off this investigation by insisting that this is just what scientists do. A more nuanced naturalist would review various possible explanations until a satisfactory one had been found. This is the method I adopt regarding the practice of using mathematics in our scientific theories, and I believe that the explanation I settle on helps us to understand why scientists have adopted this practice.

II.

In this section I explore the options for defending the theoretical indispensability of mathematics by rejecting metaphysical dispensability. If we could find some causal role for mathematical entities in the physical world, then there would be a very short account of why scientists employ mathematics in their theories. Scientists want their theories to give as complete an account as possible of the genuine causal structure of the physical world, and if mathematical entities figure in this causal structure, mathematics must clearly be part of these scientific theories.[2] The difficulty with this approach is well stated by Field in the course of his argument for the theoretical dispensability of mathematics:

> But even on the platonist assumption that there are numbers, no one thinks that those numbers are causally relevant to the physical phenomena: numbers are supposed to be entities existing somewhere outside of space-time, causally isolated from everything we can observe. If, as at first blush appears to be the case, we need to invoke some real numbers like 6.67×10^{-11} (the gravitational constant in $m^3/kg^{-1}/s^{-2}$) in our explanations of why the moon follows the path that it does, it isn't because we think that that real number plays a role as a *cause* of the moon's moving that way; it plays a very different role in the explanation than electrons play in the explanations of the workings of electric devices. The role it plays is as an entity *extrinsic to the process to be explained*, an entity related to the process to be explained only by a function (a rather arbitrarily chosen function at that). Surely then it would be illuminating if we could show that a purely intrinsic explanation of the process was possible, an explanation that did not invoke functions to extrinsic and causally irrelevant entities....... the elimination of numbers, unlike the elimination of electrons, helps us to further a plausible methodological principle: the principle that *underlying every good extrinsic explanation there is an intrinsic explanation*. If this principle is correct, then real numbers (unlike electrons) have *got* to be eliminable from physical explanations, and the only question is how precisely this is to be done (Field 1980, 43–44).[3]

This sort of argument shows how one can argue for theoretical dispensability based on metaphysical dispensability and what appear to be obvious additional assumptions. For Field these additional assumptions seem to be that explanations determine what is needed in a physical theory and that all explanations are causal. Given that mathematical entities are not part of the physical systems of concern, non-mathematical causal explanations must be available. And any features of our current scientific theories that are not needed for these causal explanations are not needed in our physical theories.

I have already suggested why it is not helpful to deploy a physicalist interpretation of mathematics in a defense of theoretical indispensability. Even if we adopt an interpretation of mathematics according to which 6.67×10^{-11} does turn out to be a part of the Earth-moon system, and so the real number is an intrinsic part of the system we are concerned with, this aspect of 6.67×10^{-11} is not relevant to the role of the gravitational constant in these units in our account of why the moon moves the way it does. As Field clearly recognizes, this number is connected to the system only in virtue of a function or mapping whose features are determined in part by the arbitrary units we have adopted. To shift to an even simpler example, "System S is at 40 degrees Celsius" can be plausibly regimented as "C(S) = 40" where C is a mapping taking all physical systems to appropriate real numbers, reflecting the choice of degrees Celsius as units.[4] Both 6.67×10^{-11} and 40 play their role in these scientific representations and resulting explanations only in conjunction with a mapping between physical systems and mathematical structures.

Given this mapping account of applications it might seem inevitable that mathematics is theoretically dispensable (as with Field), or that even if it is not, we can still refrain from accepting mathematics into our ontology (Balaguer 1998, ch. 7).[5] One attempt to block this conclusion focuses on what I will call abstract explanations. Abstract explanations have quite different features from the explanations involving coordinate systems mentioned above, and these differences suggest a role for mathematics in physical theories that Field and others seem to have missed. While, in the end, I will argue that abstract explanations are not by themselves sufficient to defend theoretical indispensability, they will provide an important clue as to how to do so.

By an abstract explanation I mean an explanation that appeals primarily to the formal relational features of a physical system.[6] Some abstract explanations that employ mathematics seem to qualify as intrinsic explanations. This is because even though they can be thought of as involving mappings between a physical system and a mathematical domain, these mappings do not turn on any arbitrary choice of units, but concern only the intrinsic features of the systems represented. As my example I take an explanation of why it was impossible to walk a certain kind of path across the bridges of Königsberg. Suppose that it is 1736 and we are with Euler in Königsberg. He claims:

A connected graph G is Eulerian iff every vertex of G has even valence and after a bit of mathematical study we accept what he says as true.[7] This mathematical study involves learning some new concepts: a *graph* (or *multigraph*) is an ordered pair, where in the first position is a set of objects called

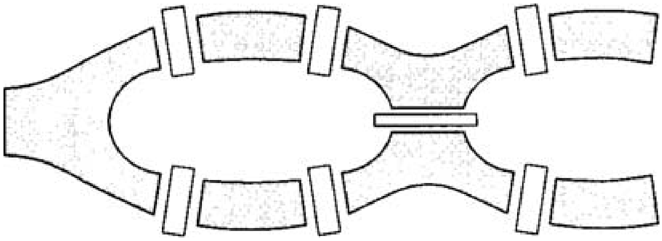

Figure 1. The bridges of Königsberg

Note: (Figures 1 and 2 are from Carlson 2001. Figure 3 is adapted from figure 2.)

vertices and in the second position is a set of (unordered) pairs of vertices called *edges*. A *path* of a graph is a series of edges where one of the vertices in the nth edge overlaps with one of the vertices in the $n + 1$th edge. *Connected* graphs have a path between any two distinct vertices. The *valence* of a vertex is how many edges it is part of. Finally, a graph is *Eulerian* just in case it is connected and has a path from an initial vertex v that includes each edge exactly once and that ends with v.

After this lesson in mathematics we walk over with Euler to the famous seven bridges of Königsberg. They look like Figure 1. If we treat the islands and banks as objects, and use the bridges to form edges, the physical system forms a simple graph. See Figure 2. At least one of these vertices has an odd valence (in fact they all do), so this graph is non-Eulerian. This is just to say that it is impossible for anyone to cross all the bridges exactly once and return to their starting point. If I was asked to explain why it is impossible to make such a crossing, then I would appeal to the fact that one of the vertices has

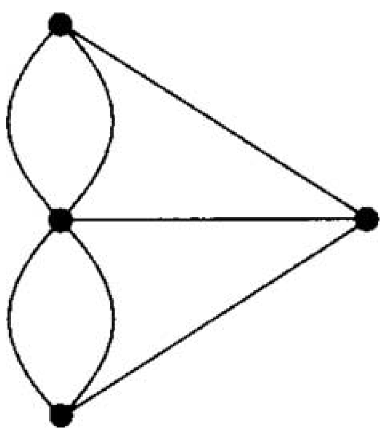

Figure 2. A non-Eulerian graph

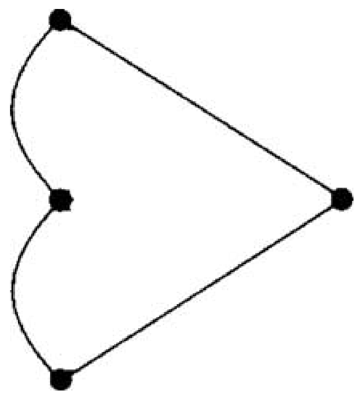

Figure 3. An Eulerian graph

an odd valence. To further fill out this example, imagine that we traveled to a nearly identical town down the river (called K', perhaps) where the residents had refrained from building three bridges so that their bridge system formed the graph given in Figure 3. Knowing Euler's theorem, I could now explain why we could cross all the bridges exactly once and end up at the starting point by appealing to the fact that all the vertices have an even valence.

I claim that it is a fact about the bridges of Königsberg that they are non-Eulerian and that an explanation for this is that at least one vertex has an odd valence. Whenever such a physical system has at least one bank or island with an odd number of bridges from it, there will be no path that crosses every bridge exactly once and that returns to the starting point. If the situation were slightly different, as it is in K', and the valence of the vertices were to be all even, then there would be a path of the desired kind.

Here we see the limitations of Field's conception of applications of mathematics. For him, each application involves relating a physical system to something mathematical via a coordinate system or unit of measurement. There is nothing about the solar system alone, he could say, that relates it to the gravitational constant in some arbitrarily chosen units. But not all applications are like that. I have not brought in any coordinate system or unit of measurement, or any arbitrary association between the bridges and graph theory. All that I have done is described the physical system at a higher level of abstraction by ignoring the microphysical properties of the bridges, the banks and the islands. It is tempting to say that the bridge system just is a graph, although this is somewhat misleading. The bridge system is of course not a graph because graphs are mathematical entities and the bridge system is physical. Still, the bridge system and this particular graph seem much more intimately connected than the system with a temperature and the number 40. We might capture this by saying that the bridge system has the structure of a graph, in the sense that the relations among its parts allow us to map those parts directly onto a particular graph.

Abstract explanations are useful to scientists because they are successful even when the microphysical configuration of a system changes. A microphysical explanation of why we could not walk a certain path on the bridge might fail if the microphysics of the bridges was sufficiently altered, e.g. the bridges were turned into gold. The abstract explanation seems superior because it gets at the root cause of why walking a certain path is impossible by focusing on the abstract structure of system. Even if the bridges were turned into gold, it would still have the structure of the same graph, and so the same abstract explanation would apply. By abstracting away from the microphysics, scientists can often give better explanations of the features of physical systems.

Batterman has recently emphasized a special kind of abstract explanation which he calls asymptotic explanation (Batterman 2002a).[8] Asymptotic explanations involve mathematical equations that result from taking one or more quantities in a fundamental mathematical law to a limit such as 0 or infinity. This sort of transformation can greatly simplify the mathematics involved and produce equations that are both tractable and universal, i.e. remain correct for a wide variety of physical systems. I agree with Batterman's claims that asymptotic explanations are important for science and that they deserve further scrutiny by philosophers of science. I add that some abstract explanations are not asymptotic explanations, as the Euler example shows, and that abstract explanations generally require philosophical examination.

In order to see how someone critical of theoretical indispensability could respond to abstract explanations it is useful to review Field's representation theorems and what role they play in his account of applications of mathematics. Field's theorems depend on first giving a non-mathematical version of the scientific theory under consideration. In the case discussed in *Science Without Numbers* this involves providing an axiomatization for the background space-time manifold needed and axioms for each physical magnitude usually associated with mathematical entities, e.g. temperature (Field 1980, ch. 7). Once this axiomatization is in place, Field offers a representation theorem which, if successful, would show that every (semantic) consequence of the mathematical theory T expressible in the non-mathematical terms of theory T′ is also a consequence of the non-mathematical theory T′.[9] T′, then, is the theory that Field can appeal to when asked to determine his ontological commitments, even though in practice it may be more convenient to work with the original mathematical theory T.

The specific T′ that Field developed has no place for abstract explanations as his background manifold invokes only space-time points and regions and his magnitudes are defined as properties of such points and regions. A crude Fieldian attitude to abstract explanations would thus be that only microphysical explanations are essential to science, and so even if abstract explanations occur in scientific practice, they are not really required. In the bridges case this amounts to the denial that there is any general scientific fact that an Euler path is impossible. Any particular attempt to walk an Euler path will fail, but this failure will have a perfectly transparent microphysical explanation that follows from T′, e.g. pointing out where the path crosses a given bridge twice.

I call this response crude because it presupposes a number of controversial philosophical assumptions. To carry it through quite generally would involve a defense of Humean supervenience as well as a rejection of even the minimal sort of naturalism that I am assuming in this paper. Recall that Humean supervenience is the doctrine that all facts (in our world and similar worlds) supervene on facts about microphysical particulars, their intrinsic properties and their spatio-temporal relations. That is, any difference in facts of any kind depends on differences at this microphysical level.[10] To extend this metaphysics into a rejection of abstract explanations would involve a denial of common scientific practices. That is, even though scientists employ abstract explanations, such a practice is reinterpreted as insignificant for helping to determine the scientific theories used when assessing ontological commitments. While some explanations are accorded importance in this regard, abstract explanations are ignored. Both positions are of course quite controversial and go beyond the more plausible commitment to metaphysical dispensability. While Field and others may accept these claims, it does not seem acceptable to assume them when arguing against theoretical indispensability or platonism about mathematics generally. For at least as stated above, Humean supervenience by itself is incompatible with the existence of mathematical entities, and thus would beg the question as a premise in an argument against platonism.

A superior response by an advocate of Field's program would be to accept abstract explanations as genuine and to go on to offer representation theorems for these parts of mathematics as well. There seems to be nothing to stop Field from merely adding new axioms to his non-mathematical theory T′ and proving a representation theorem in relation to this use of graph theory just as he proved a representation theorem relating to the use of real numbers in measuring temperature magnitudes. Indeed, in my account of how the bridge system forms a graph it looks like I have described the series of steps needed to do this. Field could define physical vertex or p-vertex, physical edge or p-edge, and then physical graph or p-graph in parallel to the mathematical definitions of these concepts with little change. Generalizing this idea, whenever an abstract mathematical explanation is found in science, Field could adopt axioms for higher-order or abstract physical properties and prove the resulting representation theorem. The resulting non-mathematical theories would be considerably more complicated than the theories Field and others have considered to date, but they seem a natural extension of the principles Field articulates.

III.

The lesson of the previous section is that a disciplined adherent of metaphysical dispensability bent on undermining theoretical indispensability should take any purportedly indispensable application of mathematics and absorb it into her non-mathematical theory by simply invoking new physical properties and relations and the requisite axioms needed to prove the representation theorem. This is most likely the picture motivating an important, though flawed, argument by Balaguer. Balaguer dubs our account of applications in terms of mappings the representational account, and argues that if the representational account is successful for applications of mathematics generally then mathematics is theoretically dispensable:

> whenever the representational account can be used to explain a given application of mathematics, we will be able to define a function Φ, of the sort discussed above, from the physical objects that the assertions in question (that is, the assertions of the given empirical theory that refer to mathematical objects) can be taken to be *about*, to the mathematical objects of the mathematical theory being applied. *But to define such a Φ is just to prove a representation theorem for the given use of mathematics*. That is, the Φ here is going to be exactly the sort of function that's constructed by a representation theorem: it's going to be an appropriate sort of homomorphism mapping an appropriate sort of empirical structure (which the assertions in question can be taken to be about) into the mathematical structure that we're applying. But as Field has shown, [footnote to Field 1980] once we've proved a representation theorem, it is not hard to nominalize the assertions in question. All we have to do is restate these assertions solely in terms of the empirical structure that we defined along with the homomorphism Φ (Balaguer 1998, p. 112, my emphasis).[11]

The error in this argument occurs in the emphasized sentence. An advocate of the mapping account clearly must accept mappings, as it is in terms of mappings that the statements of applied mathematics are assigned appropriate truth-conditions. Still, defining these mappings between physical situations and mathematical structures is not sufficient to prove the needed representation theorems. In addition to defining the mapping, one must adopt axioms about the physical situation and its properties that are quite strong. For example, in proving his representation theorem for temperature magnitudes Field assumes that "if point x has temperature $\psi(x)$ and point z has temperature $\psi(z)$ and r is a real number between $\psi(x)$ and $\psi(z)$, then there will be a point y spatio-temporally between x and z such that $\psi(y) = r$" (Field 1980, p. 57). Such an assumption is required to ensure that the temperatures of space-time points are sufficiently arrayed so that any statement involving real numbers and temperatures will have a clear non-mathematical analogue. By setting up this parallel between mathematical claims about temperature and non-mathematical claims about temperature properties, we get the conclusion that every consequence about these temperature properties implied by the mathematical theory is also implied by the non-mathematical theory.

The lesson to be drawn from Balaguer's argument is that a defender of theoretical indispensability who accepts the mapping account must find a way to reject the axioms needed to prove the appropriate representation theorem. One kind of objection to the axioms is that they are really just mathematical claims in disguise. These objections are inconclusive, though, because there is no consensus on how we decide whether or not space-time points, for example, are mathematical entities. More recently, Colyvan has pointed forcefully to the many respects in which the theory T′ that results from assembling all the needed axioms is worse off than the original mathematical T.[12] For example, T′ will generally be less unified than T due to the way in which the mathematics is eliminated. Again, unfortunately, there is no prior agreement on how we compare the goodness of two scientific theories when assessing our ontological commitments. Field at least grants that his T′ must be "attractive" (Field 1980, p. 41) and mentions at one point explanatory power (Field 1980, p. 8), but these criteria are too vague to deploy in either attacking or defending T′.[13]

A stronger objection is based on considering the evidence we have for the two theories. We have just seen that to prove a representation theorem for temperatures, one must assume quite strong axioms about instantiated temperature magnitudes. Similarly, to get a representation theorem for a physical theory employing graph theory, one must invoke strong axioms about physical graphs or p-graphs. The worry with these axioms is that they receive little or no support by our empirical evidence for the mathematical scientific theories T. In line with the naturalism assumed in this paper, we are entitled to the claim that the scientific theories adopted by the scientific community receive an acceptable measure of confirmation by our experimental data. But this data does not help us when we ask if instantiated temperature properties are continuous as required, or even whether temperatures are properties of space-time points in the first place. Our ignorance regarding such issues points squarely at one crucial role for mathematics in our scientific theorizing: we can offer abstract mathematical descriptions of physical systems, and have these descriptions be confirmed to a reasonable degree, even when we are ignorant of many of the features of the physical system. In the temperature case, we can use real numbers to describe temperatures while remaining neutral on the question of whether instantiated temperatures are continuous and a host of other interpretative questions about temperature. Similarly, we can use graph theory to describe the bridges of Königsberg without knowing either the details of the bridges' physical construction or the extent to which graphs are to be found in physical systems of this type or in the physical world more generally.

On the present view, then, when a scientist accepts a mathematical statement like "$C(S) = 40$" or "The bridge system forms a non-Eulerian graph" there is an implicit adjudication between the mathematical properties that the scientist believes are appropriate to ascribe to the physical system and those that are deemed inappropriate. Field denies that this distinction is in place, and so assigns analogues of all the necessary mathematical properties of the mathematical entities to the constituents of his physical systems using the axioms of the non-mathematical theory. We can block this extension by drawing attention to the evidence that scientists use to determine whether or not to accept a mathematical scientific claim. This evidence simply does not resolve all the issues that are relevant to every mathematical property. Despite this ignorance, scientists work with the mathematical statements. As a non-mathematical theory must take a stand on some of these murky issues, it is a violation of scientific standards to adopt it. The prudent course is to continue to investigate the systems to see which additional mathematical properties are correctly ascribed to the systems. Until that investigation is complete, the scientist accepts the mathematical scientific theory as the best confirmed available theory.

Mathematics, then, has an indispensable *epistemic* role in our physical theories that is clearly consistent with its metaphysical dispensability. While, perhaps in principle, if we knew all the facts about a given physical system, we could give a correct, non-mathematical description of these facts, it remains that, in practice, our ignorance of some of these facts leads us to employ abstract mathematical descriptions. If asked to compare an accepted mathematical scientific theory with a newly proposed non-mathematical one, the prudent scientist should reject the non-mathematical theory as currently less well confirmed than the mathematical theory. This is the explanation that I would suggest for why a physics journal would reject such non-mathematical theories.

I end this section with two further issues related to confirmation. First, for this account to work pure mathematics must receive a high degree of confirmation prior to its application in science. If mathematics received all or most of its confirmation due to its application, then mathematical scientific theories would be relatively unattractive compared to non-mathematical scientific theories. Adding mathematics would have significant costs. However, if mathematics was highly confirmed prior to its applications, then combining mathematics with a non-mathematical scientific theory would hardly detract at all from the overall degree of confirmation of the theory.

Ideally my account of the role of mathematics in physical theories would be supplemented with a story of exactly how mathematics is confirmed by mathematicians. I do not have such a story ready to hand, and so must fall back on the naturalistic premise that if mathematicians accept a given body of mathematical theory, they must have taken appropriate steps to confirm it.

A second point concerns the status of indispensability arguments for platonism about mathematics in light of my account of the role of mathematics in our physical theories. Some have insisted that indispensability arguments are the only or best argument for platonism (Field 1980, Colyvan 2001), but my proposed role for mathematics completely undermines this claim. For while I have defended the theoretical indispensability of mathematics, the way I have carried out this defense rules out any special kind of confirmation for the mathematics employed in our scientific theories. For mathematics to play the important role that it does, it must be confirmed prior to its application. If this prior confirmation was absent, then the axioms Field needs for his representation theorems would be just as good or bad as the axioms of the pure mathematical theory. Indeed it seems that, other things being equal, Field's axioms are better because they at least stand a chance of eventual empirical confirmation, even if they are currently completely unsupported by the evidence we have. Thus it was a serious tactical error for platonists to rest their position on the role of mathematics in science. A nuanced platonist must find a source of confirmation for mathematics prior to its application if mathematics is to be theoretically indispensable and metaphysically dispensable.

IV.

So far I have proposed a way of reconciling theoretical indispensability with metaphysical dispensability by focusing on the epistemic benefits of having mathematized scientific theories, i.e. having well confirmed theories despite our ignorance of various features of the physical situation. What I would like to do in this section is argue for the connection between this kind of theoretical indispensability and questions of ontological commitment. Many people that I have dubbed "fictionalists", such as Balaguer, Azzouni and Yablo, have claimed that we can accept mathematized scientific theories without adjusting our ontological commitments in any way. I believe that the flaws in these arguments come into sharper focus given the account of the role of mathematics just offered. I will not argue here for the strong claim that the only way to accept these theories is to be a platonist about mathematics, but only for the comparatively weaker claims (1) that the presence of mathematics in our best confirmed scientific theories forces us to offer some account of the subject matter of mathematics and (2) this account, whether it turns out to be platonist or nominalist, must be a realist account that assigns the statements of pure mathematics truth-values that accord with mathematical practice.

To reach these conclusions I must emphasize an aspect of Quine's criterion for ontological commitment that has so far merely lurked in the background. This is the epistemological motivation of Quine's test. Quine stipulates that we should believe in all the entities that are quantified over by our best scientific theories because these are the entities that we have some reason to be committed to. The test is not meant as a criterion for existence and would clearly be unacceptable if this is how it was meant. Not even the most strident scientific realist, who maintains that our theories are true, would insist that being mentioned in our theories is a criterion for existence. Still, I maintain that we should adjust our ontological commitments using these theories and only these theories. If the theory does not quantify over a kind of object, then we should not believe in that kind of object. Even if those objects nevertheless do exist, we have no reason to believe in them.[14]

I turn now to Balaguer's attempt to reconcile fictionalism with the acceptance of mathematical scientific theories. For clarity, I will label the fictionalist account of applications of mathematics that Balaguer defends the framework account. Balaguer calls the attitude towards mathematical scientific theories needed for his framework account "nominalistic scientific realism" (N-realism). The N-realist accepts our ordinary theories T, but restricts her ontological commitments to what is needed to handle what Balaguer calls the "purely nominalistic content" of T. Three principles are needed to support this position:

> (NC) Empirical science has a purely nominalistic content that captures its "complete picture" of the physical world.
>
> (COH) It is coherent and sensible to maintain that the nominalistic content of empirical science is true and the platonistic content of empirical science is fictional (Balaguer 1998, p. 131).
>
> (TA) Empirical theories use mathematical-object talk only in order to construct *theoretical apparatuses* (or *descriptive frameworks*) in which to make assertions about the physical world (Balaguer 1998, p. 137).

(TA) is supposed to be consistent with some applications being explained in terms of mappings, but is also a more flexible and general account of applications because it allows scientists to use mathematics in any way desired to make assertions about the physical world.

(TA) is supported by (NC) and (COH), which in turn depend on Balaguer's principle of causal isolation (PCI): "there are no causal interactions between mathematical and physical objects" (Balaguer 1998, p. 110, italics removed). Of course, this is just a version of what I have been labeling metaphysical dispensability. Given that there are no causal connections between physical and mathematical objects, Balaguer claims that there can be no ultimate or bottom-level facts whose constituents include both mathematical and physical objects:

> if we grant that the number 40 isn't causally related to S—and this is beyond doubt—then we are forced to say that while (A) ["The physical system S is forty degrees Celsius"] does express a mixed fact, it does *not* express a *bottom-level* mixed fact; that is, the mixed fact that (A) expresses supervenes on more basic facts that are *not* mixed. In particular, it supervenes on a purely physical fact about S and a purely platonistic fact about the number 40 (Balaguer 1998, p. 133).

So, at the level of facts, even if this is not captured by our scientific theories, there is a domain of physical facts and, if there are any truths of pure mathematics, mathematical facts. These bottom-level facts are the metaphysical basis for the truth of claims like (A), when they are true. And, crucially, the existence of basic physical facts makes it "coherent and sensible", as Balaguer puts it, for the N-realist to commit herself only to the constituents of these facts, i.e. physical objects and properties. The ontological commitment to mathematical objects and properties is rejected and so we can be fictionalists after all. On this approach theoretical indispensability and metaphysical dispensability are reconciled without granting mathematics any genuine subject matter.

Now, in light of all that we have seen, let us consider how it could be that the mathematics found in a given scientific theory is theoretically indispensable, and yet metaphysically dispensable. This will happen just in case we do not have a good understanding of the physical facts responsible for the physical phenomena we are investigating, although some of the facts may be clear. But if we do not grasp some of the physical facts, the theory we accept should remain neutral on the interpretative questions relating to these facts. Given this ignorance, both (NC) and (COH) are fatally undermined. Our theories, understood in light of the evidence we have, do not determine a collection of physical claims that we could view as the nominalistic content of those theories. Given this indeterminacy, it is not reasonable to assent to the nominalistic content of the theories we accept. Here I am presupposing the epistemological nature of Quine's criterion for ontological commitment. If Balaguer accepts this, then his N-realist is fixing her ontological commitments using an indeterminate aspect of her theory, i.e. its nominalistic content.

I think this objection is decisive as, while there can be doubts about exactly what the correct canons of scientific reasoning are, it seems quite clear that it is not rational to fix one's commitments using an indeterminate collection of claims. To see the problem, consider a temperature theory T proposed when the physical basis for temperature is still unresolved, and so perhaps T leaves open the crucial interpretative question of the existence of a lowest temperature. What does the N-realist countenance in committing herself to the nominalistic content of this theory T? Any set of claims sufficient to fix our ontological commitments would include either the claim that there is no lowest temperature or the claim that there is a lowest temperature. Given that our theory T remains silent on this issue, there is no definite nominalistic content there to assent to. Similarly, suppose our theory involves an abstract description of the bridges of Königsberg, but lacks any resources to describe the microphysical features of the bridges. The nominalistic content of this theory is completely unspecified. Does the N-realist admit bridges without microphysical features, or does she supply these features arbitrarily? Believing in bridges without believing in their microphysical features is strange to say the least, and adding such features goes beyond what the N-realist has reasonably confirmed.

There is still one way to make the commitments of the N-realist determinate, but to take this route is again to fly in the face of Quine's conception of ontological commitment. We can resolve the indeterminacy by replacing (COH) with:

> (COH*) It is coherent and sensible to maintain that *the actual bottom-level physical facts* render the nominalistic content of empirical science true and the platonistic content of empirical science is fictional.

So, in our example, the N-realist would commit herself to the constituents of the actual facts about temperature, including the existence of a lowest temperature, as it is these facts that actually underwrite the truth of her incomplete theory of temperature. The problem with this is not that these facts do not obtain or that their constituents do not exist. It is rather that the evidence available to the N-realist does not support her commitment to the constituents of these facts and, perhaps more disturbingly, given her lack of access to the ultimate facts the N-realist will not have access to her own ontological commitments either. The success of science may give her some evidence for some of the facts that obtain, say that objects have temperatures and the temperatures range at least from −100 degrees Celsius to 50 000 degrees Celsius. But the evidence available does not, at that moment, indicate what to say about the existence of a lowest temperature. So, the scientist who winds up believing in a lowest temperature by these means will believe in something that she has no evidence for. Similar issues arise with the bridges example. Supposing that they were actually constructed out of a rare metal, the N-realist who adopted (COH*) would admit into her ontology this very metal, independently of whether she had any genuine evidence for its existence. Unless we abandon Quine's test and sever the tie between ontological commitment and evidence, (COH*) is not a viable principle.

V.

I believe that any attempt to combine theoretical indispensability, metaphysical dispensability and a rejection of mathematical realism must either face the sorts of problems that Balaguer does or else revise our entire picture of ontological commitment. As an example of the former problem I take the work of Stephan Yablo. Yablo is quite sensitive to the pitfalls of appealing to non-literal language, but in the end he seems to face the same sort of objections that I raised against Balaguer and Field. To see the differences between Yablo's "figuralism" (as he calls it) and Balaguer's fictionalism, consider statements like "The physical system S is forty degrees Celsius" or

"The bridge system forms a non-Eulerian graph". As we have seen, Balaguer grants these claims both a normal and a nominalistic content and he adjusts his ontological commitments only in light of the nominalistic contents of these statements. What I have just argued is that these statements do not have determinate nominalistic contents. Unlike Balaguer, Yablo appeals directly to the similarities between mathematical language and non-literal or figural uses of language. Thus Yablo would maintain that he determines what to believe based on the entire content of applied mathematical statements, but only when these statements are assigned their proper contents, and that these proper contents are generated by the non-literal use of language. Still, he grants that the "real contents" of these statements may be something less than what they appear to be, which he sometimes calls their "literal content": "The real content of my utterance is the real-world condition that makes it sayable that S. The real content of my utterance is that reality has feature BLAH: the feature by which it fulfills its part of the S bargain" (Yablo 2002, p. 229) and elsewhere.

> What is the literal content of "the number of sheep is three times the number of goats"? That the sheep have associated with them a number that stands to the number associated with the goats in a certain numerical relation, a relation that the number of F's bears to the number of G's only if there are three times as many F's as G's. The real content is that there are three times as many sheep as goats. It does not seem very mysterious how we get from the one to the other. The real content is that portion of the literal content that concerns the sheep and the goats (Yablo 2001, p. 97).

Despite the innovations over Balaguer and the resources that result from an appeal to non-literal uses of language, Yablo faces precisely the same indeterminateness objection that I raised against Balaguer. As it stands, he has not offered any general account of how we are to discern the real contents hidden in the literal contents which we appear to be asserting or reasoning with. Perhaps in the number case, the simple transformations that he has discussed are workable. But this still leaves open our claims, which are still quite simple, like "The physical system S is forty degrees Celsius" or "The bridge system forms a non-Eulerian graph". There is no determinate real content here, either just about temperatures or just about bridges, to be revealed. The full, literal content involves a complicated mapping relationship between the systems and some part of the mathematical domain. If we try to remove this appeal to the mathematical domain, we get only indeterminate statements.

This does not show that Yablo could not carry out a general investigation of the use of applied mathematics, and extend his account of how the real content is isolated from the literal. In fact, this appears to be what his more recent work is focused on. Still, even if such a general account were developed to handle the wide range of applications currently in existence, it is important to note that Yablo would then face another kind of question. If he makes his real contents determinate, has he saddled the scientist with commitments that she is not warranted in accepting, as we saw with (COH*) and with Field's axioms? It is, of course, difficult to argue that any account Yablo developed would do this, but it seems that the burden of proof must be on Yablo to satisfy the indeterminateness objection without ascribing unreasonable commitments to practicing scientists.

Another strategy, sometimes also suggested by Yablo, is to revise our criterion of ontological commitment. Azzouni frankly admits that his approach requires a new account of ontological commitment, and I will end this section by briefly summarizing my reasons for not following him. First, there is a question of priorities. I am most impressed by the apparent conflict between theoretical indispensability and metaphysical dispensability, and am sympathetic with a broadly naturalistic approach to epistemology and ontological commitment. Thus, in line with Quine, I am trying to clarify our commitments with the aim of understanding how scientific theories work and why scientists adopt the methodological principles that they do. An integral part of this project is having an unequivocal and transparent criteria with which we can decide, after the process of clarification and regimentation, what things a scientist should believe in. Azzouni, by contrast, seems much more focused on resolving the platonism-nominalism dispute, especially with reference to the indispensability argument for platonism. From this perspective, Quine's criteria of ontological commitment can come to look like a substantial and unexamined premise that can reasonably be questioned by the nominalist. This questioning leads Azzouni to a distinction between quantifier commitment and genuine ontological commitment. As with Field, Balaguer and Yablo, Azzouni appeals to something like metaphysical dispensability to motivate his proposal. For Azzouni, though, the appeal is part of an argument that even if our theories are quantifier committed to mathematical objects, they need not be ontologically committed to mathematical entities because mathematical entities fail certain tests.

I am prepared to grant Azzouni's maneuver as an interesting and innovative response to a certain kind of indispensability argument for platonism, but as I have tried to make very clear I am not offering such an argument. Instead, I am trying to account for a certain kind of scientific practice, using the best tools of clarification that philosophers have developed to date. These tools include Quine's test for ontological commitment. From within the confines of my project, I do not see any advantages in abandoning Quine's criteria in favor of Azzouni's. In fact, the disadvantages seem clear. If I were to adopt Azzouni's criteria, there would be an additional explanatory task to resolve: why are scientists quantifier committed to mathematical entities, and not genuinely ontologically committed? The distinction introduced by Azzouni does not help me with my explanatory project and so I set it aside.[15]

VI.

I conclude by considering two serious objections to my account. Both objections raise further philosophical and technical considerations which I cannot resolve here, but discussing them will at least give me an opportunity to suggest how I hope to respond in greater detail in the future.

The first objection grants much of what I have said against Field and others, but traces the problems I have found to the role of mathematics in idealized scientific theories. Idealized theories are those that we use despite the fact that we are convinced that they are strictly speaking false. Thus, the reason we are unhappy with Field's non-mathematical version of classical mechanics is the same reason we are dissatisfied with ordinary, mathematical classical mechanics. Both theories are idealizations, and we can see in what respects they are idealizations when we compare their predictions with the theories that we actually accept, i.e. general relativity theory and quantum mechanics. Perhaps, the objection concludes, the role for mathematics that I have isolated is limited to such idealized cases. And, given that the theories that we actually use to determine our ontological commitments are not idealized in this way, the role of mathematics in these theories remains mysterious.

Now, I have certainly not addressed the role of mathematics in our best, current scientific theories like general relativity theory or quantum mechanics and I grant that I must do this in order to fully vindicate my account. Still, at least initially, the role of mathematics in these theories seems quite in line with what I have suggested is the case with classical mechanics. In general relativity theory, for example, we adopt differential geometry and relate various physical magnitudes to aspects of a preferred class of differentiable manifolds. Someone like Field would no doubt insist that this involves extrinsic considerations and that we would do better to develop a non-mathematical theory that directly describes the physical properties of space-time. Our reluctance to do this, I suggest, is due in large part to our wish to remain agnostic about the wide variety of features of space-time that we would have to take a stand on to get such a non-mathematical theory off the ground. It is no better to follow Balaguer and Yablo and claim that we countenance only the entities involved in the nominalistic or real content of general relativity theory. These contents remain indeterminate precisely because can we accept general relativity theory without deciding certain basic questions about space-time. So, I certainly do believe that this account is correct for our most up-to-date scientific theories, and hope to work out the details in future work.

The objection raises even deeper issues, though, once we realize that idealizations are central even in our best, current scientific theories.[16] That is, often we can recognize that our claims about a system are strictly speaking false, even though we have no superior, more realistic alternative to compare it with. For example, many of the mathematical models used to make predictions needed to test general relativity theory employ idealizations when they leave out planets of the solar system, or include them only in an unrealistic fashion. This is a central aspect of scientific practice, and one that seems to go hand in hand with the use of mathematics in scientific theories.

One worry that idealization raises is whether or not it is compatible with the mapping account of applying mathematics that I have relied on throughout this paper. Suppose that "The physical system S is forty degrees Celsius" is best regimented as a claim about a mapping C, namely that it is a homomorphism that preserves temperature and that $C(S) = 40$, but that we recognize it to be part of an idealization, and so as strictly speaking false. Given its falsity, we should not commit ourselves to any of the things it talks about. And, given the widespread use of idealization in science, perhaps we should not commit ourselves to any of the entities that Quine's criteria would suggest (Maddy 1992, 1997). While I grant that idealization renders a simple mapping account problematic, in future work I will present an explanation of how an extended mapping account is consistent with idealization. And, just as with the non-idealized cases I have assumed here, mathematics will play a role in allowing well confirmed, definite claims about physical situations, consistent with our ignorance of a wide variety of other features of these situations.

The other deep issue that I have not touched on concerns alternative, less ambitious versions of Field's program. Recall that Field formulated strong non-mathematical theories so that he could prove that the mathematical theories were conservative over the non-mathematical theories: every (semantic) consequence of the mathematical theory T formulated in non-mathematical terms is also a consequence of the non-mathematical theory T′. In reaction to my worries about the lack of evidence for these non-mathematical axioms, one might ask what would follow from weaker non-mathematical axioms that we might rate as more highly confirmed than the strong non-mathematical axioms that Field needs for his full representation theorems. To take a trivial but hopefully instructive example, consider the graph theory used in the Königsberg bridges case. Graph theory includes the study of graphs with infinitely many vertices. So, to establish a full representation theorem for this case, Field would need to adopt non-mathematical axioms for his p-graphs that would imply the existence of p-graphs with infinitely many vertices. Suppose, though, that these sorts of graphs were never used in applications. Even if they appear in the mathematical theory, it seems somewhat irrelevant whether or not we can get a representation theorem for this part of graph theory. Similarly, there are no doubt facts about the real numbers that are not exploited in any current applications of the real numbers. Why should our non-mathematical theory be worried about these sorts of "non-physical" parts of the mathematical theory? It seems sufficient to show how to formulate axioms for the non-mathematical theory that can do the work of the mathematics that is actually applied, and forget the rest.

I will call the pursuit of such axioms and associated representation theorems "weak representation theory". While it has received some discussion in light of Shapiro's objections to Field's program, I do not believe there has been any systematic research to date on what technical results are achievable within weak representation theory.[17] Such work could conceivably respond to the objections I have raised in this paper if it led one to formulate non-mathematical axioms that were both well confirmed and sufficient to explain the applications of mathematics actually made in the mathematical version of that scientific theory. This non-mathematical theory could then be used to determine our ontological commitments, and could also be used to explain what the nominalistic or real content of our mathematical, scientific statements actually was.

I have granted that idealization and weak representation theory greatly complicate my attempt to reconcile theoretical indispensability and metaphysical dispensability, but have also expressed the hope that future work will vindicate my epistemic account of why scientists use mathematics in their theories. To repeat, by including mathematics, scientists can formulate definite claims that zero in on those aspects of the physical situation that they wish to take a stand on, while remaining neutral about the aspects that they have yet to understand and that are not relevant to the phenomena they do understand. This account may be consistent with the eventual removal of mathematics from our scientific theories, if we were to ever have a complete understanding of the systems at issue. Today, though, we lack such an understanding, and it is today that we must determine our present ontological commitments in light of all the evidence we have. While prior metaphysical commitments might influence this process of regimentation, when it comes to our mathematical scientific theories, it is simply not possible to make our commitments determinate without taking the mathematics we use seriously.[18]

Notes

[1] See Baker 2003 for a critical examination of some attempts to clarify metaphysical dispensability. Baker 2005 appeared after this paper was completed.

[2] This is perhaps one of the motivations for Maddy's earlier work (Maddy 1990). See Cheyne 2001 for an extended defense of the metaphysical dispensability of mathematics.

[3] Other related passages in Field are Field 1991, pp. 18–19, 46, 193 and Field 2001, pp. 329–330.

[4] Cf. Balaguer 1998, p. 111 and Kyburg 1997.

[5] Balaguer's position will be discussed in § III.

[6] Abstract explanations are a species of what are sometimes called "structural explanations" (McMullin 1978).

[7] I closely follow the exposition given in Carlson 2001.

[8] See also Batterman 2000, 2002b.

[9] See footnote 17 for references relating to the need to mention semantic consequence here.

[10] Lewis 1994, 1999.

[11] Balaguer 1996, p. 295 offers a similar argument.

[12] Colyvan 2001, esp. ch. 4.

[13] Balaguer mentions a preservation of "look and feel" condition (Balaguer 1998, p. 129, fn. 2).

[14] Colyvan 2001 summarizes this point by saying that Quine's test is a normative claim about what we should believe exists and not a descriptive claim about what actually exists.

[15] I am here largely following Burgess 2004.

[16] Nancy Cartwright is one of the most careful advocates of this claim. See Cartwright 1999.

[17] For Shapiro's objection, Field's replies and further references, see Shapiro 1983, Field 1991, ch. 4, Burgess & Rosen 1998, § III.B.1.b and MacBride 1999, § 5. My label "weak representation theory" is adapted from Field's phrase "weak representation theorem" (Field 1991, p. 135). MacBride's remark that "weak representation theorems . . . will map all those mathematized claims that have empirical support into the nominalistic theory" (MacBride 1999, p. 451) seems quite close to my proposal here, but MacBride appears not to have been pursued this idea any further.

[18] Previous versions of this paper were presented at the Fifth Annual Midwest Phil Math Workshop (Notre Dame, 2004) and at a session of the Society for Realist/Antirealist Discussion held at the 2004 Eastern APA (Boston). I would like to thank both audiences for their helpful comments, especially Mark Colyvan, Susan Vineberg and Otávio Bueno. A misguided ancestor of section II was read at the 2004 Central APA (Chicago) and I am indebted to Julian Cole and Alan Baker for helping me to see this.

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