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BOOK SYMPOSIUM

Structures, fictions, and the explanatory epistemology of mathematics in science

Christopher Pincock: Mathematics and scientific representation. New York: Oxford University Press, 2012, 330pp, $65.00 HB

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Christopher Pincock's *Mathematics and Scientific Representation* is an interesting and important new book on the applications of mathematics to science. The book is loaded with fascinating case studies that bring out the varied and interesting ways that mathematics is used in science. Pincock provides a much more nuanced picture of the roles that mathematics plays in empirical science than we usually get from philosophers of mathematics writing on the topic of applications. He describes various dimensions along which different uses of mathematics can differ; for instance, to pick out just one of the dimensions he discusses, we sometimes use mathematics to help us represent the causal structure of a series of events, whereas in other cases, we use it to provide an acausal representation of a system in a static, fixed state. The overall picture we get from Pincock is a sort of taxonomy of different kinds of uses of mathematics. He also draws interesting connections

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between the various kinds of uses and argues convincingly that the different uses can all be seen in an epistemological light. In other words, what mathematics does in empirical science, according to Pincock, is help us gain knowledge of the world. This is an extremely good book; I recommend it very highly.

Pincock also draws various philosophical conclusions from his discussion of the applications of mathematics. One of these conclusions is that we should reject mathematical fictionalism. I think this is a mistake, and in this paper, I will try to respond to Pincock's argument against fictionalism.

Fictionalism is the view that (a) the platonistic interpretation of mathematical discourse is correct, that is, sentences like '$2 + 2 = 4$' are best interpreted as being about (or at least purporting to be about) abstract objects (i.e., non-spatiotemporal objects); but (b) there are no such things as abstract objects; and so (c) sentences like '$2 + 2 = 4$' are not true. Now, of course, fictionalists have to admit that there's *something* right about '$2 + 2 = 4$'; there's obviously an important difference between this sentence and, say, '$2 + 2 = 5$', and fictionalists need to be able to account for this. They can do this by appealing to the notion of *truth in the story of mathematics*. In particular, fictionalists can say that (1) '$2 + 2 = 4$' is true in the story of mathematics and '$2 + 2 = 5$' isn't (just as, say, 'Santa is jolly' is true in the story of Santa Claus, and 'Santa is morose' isn't); and (2) truth in the story of mathematics is a much more important property than mathematical truth is, because it corresponds to "rightness" or "correctness" in mathematics.

If fictionalists take this line, then they will need to say what the story of mathematics consists in. One view here is that it includes everything that follows from the axiom systems that are currently accepted in the various branches of mathematics (Field endorses a view roughly like this in his 1998); but I have argued elsewhere (2009) that fictionalists would do better to take the story of mathematics to be the claim that platonism is true, that is, the claim that there really exist abstract objects of the kinds that we have in mind when we do mathematics (i.e., numbers and sets and so on). Thus, on this view, to say that a sentence is *true in the story of mathematics* is to say that it *would have been true if platonism had been true*; for instance, '$2 + 2 = 4$' is true in the story of mathematics because it would have been true if the natural numbers had existed.[1]

Pincock's argument against fictionalism (on pp. 250–55) is based on his claim that fictionalists can't answer what he calls the *export challenge*. To bring this challenge out, let ES be our best empirical scientific theory of the world; if you like, you can think of ES as a big conjunction of all of our accepted empirical theories—for example, quantum mechanics and evolution theory and so on. Now, since ES contains references to mathematical objects, fictionalists have to say that it's not true. But this seems problematic; science seems to be telling us that we should believe ES, and fictionalists are telling us that we shouldn't. What, then, *should* we believe, according to fictionalists? This is Pincock's export challenge. Fictionalists need to tell us what exactly we should believe, instead of ES. Or to put the point

[1] Thus, fictionalists are committed to saying that counterfactuals can be true even if there are no abstract objects (and they will presumably also want to say that they can be true even if there are no non-actual possible worlds). I have defended this view elsewhere (2010), but I obviously can't get into this here.

differently, they need to tell us what exactly ES is telling us about reality, or about the physical world.

Actually, this is only *half* of Pincock's export challenge. I will call this the *negative export challenge*; again, the challenge here is to say *what* we should believe, instead of ES. The other half of the challenge—the *positive export challenge*—is to say *why* we should believe the fictionalist's replacement of ES (whatever it turns out to be), instead of ES itself. I will begin by discussing the negative export challenge; I will return to the positive export challenge below.

Before I say how I think fictionalists can answer the negative export challenge, I should point out that Pincock provides an argument for thinking that fictionalists won't be able to answer this challenge (254–55). His argument can be summarized as follows:

> *The multiple-interpretations argument*: ES is subject to varying interpretations, and on these different interpretations, it says different things about the nature of the world. Consider, for example, the claims in ES about *heat*. If we take heat to be a fluid (as Laplace did), then ES will say one thing about the world, whereas if we take heat to be a transfer of energy, then ES will say something different about the world. Thus, it's hard to see how fictionalists can get very precise about what exactly ES is telling us about the world. And so it's hard to see how they could get precise about what we should believe instead of ES.

I don't think this argument works, and I will say why below. But first, I want to explain how fictionalists can answer the negative export challenge. They can do this by taking what I said above about pure mathematical sentences like '$2 + 2 = 4$' and simply extending it to ES. In particular, fictionalists can respond to Pincock's negative export challenge by saying that instead of believing ES, we should believe the following:

> *Nominalistic-ES*: ES is true in the story of mathematics; in other words, if platonism were true (i.e., if there really existed abstract objects of the kinds that we have in mind when we do mathematics (and the kinds we have in mind when we talk about propositions and sentence types and so on)), then ES would be true; or perhaps better: The physical world has a nature that makes it the case that if platonism were true, then ES would be true (if you'd rather, instead of talking here about the *physical* world, we can talk about the *concrete* part of the world—more on this below).

It's important to note that nominalistic-ES is a genuine claim *about the nature of the physical world* (or the concrete part of the world). More specifically, in committing themselves to nominalistic-ES, fictionalists ontologically commit themselves to all of the concrete objects that ordinary scientific realists are committed to—for example, electrons and planets and so on—and, moreover, they commit themselves to these objects having the intrinsic nature that ES says that they have. For on the fictionalist view, nominalistic-ES is true just in case ES would be true in a scenario in which (a) the physical world was exactly as it presently is and (b) there really existed abstract objects of the kinds that we have in mind when we do mathematics (and linguistics and so on). The reason fictionalists can endorse this view is that abstract objects (if they exist) are causally inert. Because of this, it follows that in order for ES to be true, two entirely independent sets of facts need to obtain, namely, a set of purely platonistic facts about abstract objects and a set of purely nominalistic facts about things like electrons and lobsters and planets. But given that the two sets of facts hold or don't hold independently of one another, it seems reasonable to believe that the nominalistic facts obtain and the platonistic ones do not. And according to fictionalists of the sort I've got in mind, this is exactly what we should believe in connection with ES; we should believe that the physical world (or the concrete part of the world) is just the way it needs to be to make ES true; in other words, we should believe that the physical world "holds up its end of the ES bargain"; or to put the point more precisely, we should believe that the physical world is such that if platonism were true, then ES would be true. And, again, when fictionalists commit themselves to this—when they commit themselves to nominalistic-ES—they commit themselves to the idea that there really is a physical world and that that world has precisely the intrinsic nature that it needs to have to make ES true.

It's important to note that on the view I'm proposing here, nominalistic-ES is not supposed to capture what ES "really says", or what we "really believe" when we accept ES. Pincock sometimes puts the negative export challenge as a challenge to say something about "our beliefs outside the fiction" (252). On the view I've got in mind, ES "really says" what it *seems* to say—that is, it says things about the physical world and about abstract objects—and so it's simply *false*. Moreover, when we accept ES, what we "really believe" is ES itself. (This isn't entirely accurate; after all, there are idealizations in ES, and we might doubt that claims involving idealizations are strictly true; and we might be pretty sure that there are mistakes in ES that future inquiry will uncover; and there are *some* people (e.g., fictionalist philosophers of mathematics) who don't believe ES at all; but for present purposes, we can ignore these nit-picky points, and proceed as if people straightforwardly believe ES.) In any event, on the view I've got in mind, what we believe (or what most of us believe) when we accept ES—namely, ES itself—is just *false*. The above response to the negative export challenge isn't meant to take this back; it's meant to explain why it *doesn't matter* that ES is false. It doesn't matter because even if ES is false, it is true in the story of mathematics (or at least close to this, or approaching this, or some such thing, depending on how well scientists have done their job), and so it's *for-all-practical-purposes true*, or something along these lines. In short, on the fictionalist view I've got in mind, ES is false, but it's a *useful* falsehood, and it is entirely *harmless* that it's false.

Given this response to the negative export challenge, it should be clear what I would say to Pincock's multiple-interpretations argument. If ES is subject to multiple interpretations, then nominalistic-ES is too. But so what? If platonists and ordinary scientific realists can rest content with a theory (namely, ES) that's subject to multiple interpretations, why can't fictionalists? Nominalistic-ES doesn't seem to be any more indeterminate than ES itself is, so I don't think there's a problem here.

Pincock might respond to this by saying something like the following (indeed, given what he says on pp. 253–54 about the abstract-concrete distinction, I think he *would* respond in this way).

> *The abstract-concrete objection*: Fictionalists are actually worse off here than the rest of us are. For there are *more* unclear terms in nominalistic-ES than

> there are in ES. In particular, the term 'abstract object' (which appears in nominalistic-ES) is unclear, and this creates a problem for fictionalists. For given the imprecision in the abstract-concrete distinction, it's not clear what exactly nominalistic-ES is saying about the world. Suppose, for instance, that ES included sentences about *minds*—for example, sentences like
>
> (S) The number of sheep is three times the number of sheep minds.
>
> If ES included (S), then fictionalists would have to say that (S) is true in the story of mathematics. But it's not clear what this claim would amount to because it's not clear whether minds count as abstract or concrete. In other words, if fictionalists said that (S) is true in the story of mathematics, it wouldn't be clear whether they were saying that (a) the relevant concrete objects (in particular, minds and sheep) are such that if there were numbers, then (S) would be true; or that (b) the relevant concrete objects (in particular, sheep) are such that if there were minds and numbers, then (S) would be true.

The first thing I'd like to say in response to this is that on the way that *I* think of the abstract-concrete distinction, minds are pretty obviously concrete. Concrete objects are just non-abstract objects, and abstract objects are (on the standard definition) non-spatiotemporal objects. Thus, even if Cartesian dualism is true, minds are concrete, for they are spatiotemporal; my mind seems to be spatially located (it seems to be in my head and not, say, in a garage in Buffalo), and even if you doubt this, it seems almost undeniable that it's temporally located (it's located in the twenty-first century and not, say, the fourteenth century). Now, I suppose one might dig in her heels here and argue that Cartesian souls are *not* spatiotemporally located; if so, I would be disinclined to argue the point; I would be more inclined to simply change the definition of an abstract object to something like *non-physical, non-mental, non-spatiotemporal object*. Thus, it seems to me that I can avoid the worry about minds by simply stipulating what *I* mean by an abstract object, that is, by stipulating that even if Cartesian dualism is true, minds (or souls or whatever) count as concrete and, hence, not abstract.

(By the way, this is why I said above, in articulating my response to the negative export challenge, that it might be better to talk about *the concrete part of the world* than *the physical world*. If we're all assuming that mind-brain materialism is true, then fictionalists can talk about the physical world; but this is a bit sloppy; the more precise way to put the view is in terms of the concrete part of the world—which, of course, if fictionalism is true, is the *whole* world.)

In any event, it might seem that the above remarks about minds don't provide a fully satisfying response to the abstract-concrete objection. For you might think that minds were just an *example* of what Pincock was talking about—that is, just an example of objects that are neither clearly abstract nor clearly concrete. Thus, you might think that Pincock could respond here by simply changing the example. For instance, following Resnik (1985, 1997), you might think that quantum particles and spacetime points are neither clearly abstract nor clearly concrete.

I want to make the same two points about these other so-called borderline cases that I made above about minds. First, on the way that *I* understand the abstract-concrete distinction, spacetime points and electrons are clearly concrete. And second, if opponents of fictionalism dug in their heels on this point and insisted that it's unclear whether these objects are abstract or concrete, I would be disinclined to argue with them. I would take this as evidence that we just had different concepts of an abstract object, or different representations of the abstract-concrete distinction; and if this were the case, I would likely want to say that there was no fact of the matter who was right. My fix of the situation would be to simply stipulate what *I* mean by an abstract object. And since I'm not wedded to the idea that there is a nifty necessary-and-sufficient-condition-style definition of 'abstract object', I would be happy to define the term by simply listing off cases (and since there is a finite number of terms in ES, I could literally do this by listing off *all* the cases). So, for instance, I could just say that an object is abstract iff it's a mathematical object (e.g., a number, or a set, or a function), or a proposition, or a sentence type, or a Fregean sense, and so on. And I could say that an object is concrete iff it's not abstract, and I could carefully stipulate that minds, subatomic particles, and spacetime points are concrete. If I articulated my version of fictionalism in terms of this stipulated definition of an abstract object, then the above abstract-concrete objection would simply evaporate. And given this, I think it's fair to say that nominalistic-ES is every bit as clear and determinate as ES is.

Let me turn now, very briefly, to the *positive* export challenge. To answer this challenge, fictionalists have to say why we should prefer nominalistic-ES to ES. This, I think, is equivalent to demanding that fictionalists provide a positive argument for their view. But if this is right, then if we merely want to defend fictionalism against objections, we don't have to answer the positive export challenge. After all, one might think (and I *do* think) that there are no good reasons for favoring *either* of the two views here, that is, for preferring ES *or* nominalistic-ES. If I can motivate the claim that there's no reason to prefer ES to nominalistic-ES—that is, that from our current epistemic vantage point, nominalistic-ES is *just as good* as ES—then I will have blocked Pincock's argument against fictionalism. This is the spirit in which I have been trying to answer the negative export challenge. And so given the dialectical situation here, I just deny that the positive export challenge needs to be met. In short, we can *defend* fictionalism without providing a positive argument for it.

Returning again to the negative export challenge, it's worth noting that the view I've been proposing here can be used in connection with not just scientific sentences but other kinds of sentences that Pincock refers to as being "outside the fiction" of pure mathematics. For instance, on p. 252, Pincock asks us to consider what fictionalists should say about sentences like the following:

(A) The number 0 is numerically distinct from the null set.
(B) The number 1729 is interesting.

It seems to me that fictionalists should say whatever platonists say about these sentences. More precisely, they should say that these sentences are true in the story of mathematics if and only if platonists should say that they're true. Now, it might not be obvious why fictionalists can help themselves to this strategy, so let me explain why they can. I will do this in connection with (A). Part of the reason that fictionalists can mimic whatever platonists say about (A) is that according to the best versions of platonism, a mathematical sentence is true if and only if it's true of the *intended* objects. Thus, platonists should say that (A) is true if and only if it's true of the objects that *we have in mind* when we do arithmetic and set theory. Thus, since platonists should also say that the mathematical realm is plenitudinous (and, hence, that there *are* number-like objects that aren't sets),[2] platonists should say that the truth of (A) is ultimately determined by our intentions; if when we do arithmetic, our overall intentions make it the case that we mean to be talking about objects that aren't sets, then (A) is true; but if the best theory of our overall intentions entails that when we use numerals, we mean to be talking about objects that just *are* sets (and in particular, that when we use '0', we intend to be talking about the null set), then (A) is false. But given all of this, fictionalists can say whatever platonists say here: If we intend to be talking about non-sets when we do arithmetic, then (A) is true in the story of mathematics, that is, it's such that if there really were objects of the kinds that we have in mind when we do arithmetic and set theory, then it would be true; and so on. (It doesn't matter here what the actual truth value of (A) is, but for whatever it's worth, I think it's pretty obviously true (or true in the story of mathematics, if fictionalism is true). But at the end of the day, I would defer to mathematicians on this point.)

Similar remarks can be made about (B); fictionalists should say that it's true in the story of mathematics if and only if platonists should say that it's true. I guess my own view here is that platonists should say that there's no objective fact of the matter whether (B) is true (because interestingness is a matter of taste). (Ramanujan thought that 1729 was interesting because it's the smallest number expressible as the sum of two cubes in two ways; but I suspect there are some people who aren't very excited by 1729, and I fear that at least some of these people would remain uninterested even if they became aware of its attributes in the sum-of-two-cubes department.) In any event, if platonists should indeed say that there's no fact of the matter whether (B) is true, then fictionalists should say that there's no fact of the matter whether it's true in the story of mathematics. But the more important point here is that fictionalists can mimic whatever platonists say about (B).

Indeed, this is the overall theme of my answer to Pincock's negative export challenge. For *any* sentence that refers to mathematical objects—whether it's a pure mathematical sentence like '$2 + 2 = 4$', or a sentence from empirical science, or a silly sentence like (B)—fictionalists should mimic what platonists say; if platonists should say that the sentence is true, then fictionalists should say that it's true in the story of mathematics; and so on. I have elsewhere (2009) called this version of fictionalism *theft-over-honest-toil fictionalism*; for the core idea behind this view is that fictionalists can essentially steal everything that platonists say.

And this brings me to the last point I want to make. Fictionalists of the kind I've been describing can agree with virtually everything in Pincock's fascinating theory of the ways in which mathematics helps us in empirical science. They can say that it's true in the story of mathematics that mathematics is useful to humans in their scientific endeavors, or that mathematics makes the numerous epistemological

[2] To say that the mathematical realm is plenitudinous is (roughly) to say that there are as many abstract mathematical objects as there could be. There are numerous arguments for the claim that plenitudinous versions of platonism are superior to non-plenitudinous versions. I can't get into this here, but see my (1998).

contributions that Pincock says it does. (Fictionalists might want to add here that since what we come to believe through our uses of mathematics—namely, ES—isn't strictly true, what we achieve here isn't strictly *knowledge*; they might want to say that what we achieve is *virtual knowledge*, where this is just like knowledge except that it requires truth in the story of mathematics instead of truth.) In any event, on behalf of all fictionalists, I would like to thank Pincock for providing such a rich and interesting book full of insightful ideas on the applications of mathematics that can be stolen.[3]

Elaine Landry

Christopher Pincock's *Mathematics and Scientific Representation* offers a detailed and well-considered account of just how mathematics provides successful scientific representations by considering five types of epistemic contributions. What is most impressive about this book is the equal attention given to both the mathematical and philosophical details; the reader is provided with the mathematical nuts and bolts that are typically left out of philosophical expositions, but which, as Pincock clearly shows, are needed to understand their philosophical import. This attention to the mathematical minutiae is not, however, mere pedantry, for as Pincock shows, much of current philosophical debates go wrong owing to their lack of attention to the mathematics. See, for example, Chapter 3 where Pincock works out the distinction between concrete causal and abstract acausal epistemic contributions, and ends with a new and informative critical analysis of the philosophical debate between Batterman and Wilson. Pincock's arguments are of interest not only to philosophers of science and mathematics, but also to working scientists. By appreciating these types of contributions as part of the "toolkit" of mathematics, we come to appreciate the essential role that the applied mathematician has to play in our understanding of the success of science. This is a message that, I think, has not been, and ought to be, well heard by both mathematicians and philosophers alike.

The book itself contains fourteen chapters and three appendices, and is broken into two parts. Part I considers the types of *epistemic contributions* that mathematics makes when we consider the accuracy of science. These are as follows: *concrete causal*, *abstract acausal*, *abstract varying*, *scale* and *constitutive*. Each chapter then takes-up, in great detail, an example from science itself and along the way promises to provide a philosophical position that lies midway between that offered by Porter's *social conception* and Franklin's *metaphysical conception*. Pincock rejects both of these views; he argues that neither offers an answer to the question of how mathematics contributes to the success of science. With the aim of taking up this question in a manner that shows that the applicability of mathematics is, *contra* both Wigner and Steiner, both reasonable and non-mysterious, Part II works out the details of Pincock's philosophical position by contrasting his view with both the *platonist's* position, as arising from indispensability arguments, and the *fictionalist's* position, as arising from arguments by analogy with literary works.

[3] I would like to thank Chris Pincock for comments on an earlier draft of this paper.

In broad stroke, the aim of Part II is to show that philosophically speaking one only need be committed to *realism about truth-values*, and this commitment need only be understood *epistemically*. That is, to come to have the type of mathematical knowledge we have and the mathematical representations we do, at least some mathematical claims need to be taken as true and justified a priori. Thus, in answer to the question of why mathematics is so central, and often times indispensible, to our best scientific theories, we now have an answer: “Mathematics is there because it is the ideal tool for arriving at well-confirmed and widely applicable scientific representations” (280). Lastly, in Chapter 14, Pincock considers the question of “[w]hat implications they [the main claims made throughout the book] suggest for the proper interpretation of pure mathematics and its epistemology” (279). Finally too, it is here that my critical remarks will begin.

We are told, just at the beginning of Chapter 14 (281) that:

> (…) any interpretation that assigns the right truth-values to the claims of pure mathematics is adequate to account for our epistemic contributions. The argument for this can be put briefly. These epistemic contributions have been analyzed in terms of the contents of our scientific representations. However, when mathematics is central to this content, the way we should make sense of the content is in terms of the existence and non-existence of structural relations. So long as the subject matter of mathematics is assigned the right structure, these contributions can obtain. As we saw with Hellman and Lewis, delivering the right structures need not involve the existence of abstract structures.

So in sum, we have three components to the philosophy that underpins Pincock’s account of scientific representation: (1) a structuralist account of applicability; (2) a structuralist account of scientific theories; and, (3) a structuralist account of pure mathematics itself. Unfortunately, no account of any of these components is worked in the detail that it ought to be. Regarding (1), all we are told is that (282):

> [a]ll three of these interpretations [Hellman’s modal, Lewis’ mereological and Shapiro’s ante rem] deploy what could be called a two-stage account of applications. In the first stage, the subject matter of the mathematics is identified. This identification turns on features that do not yet involve the way the mathematics is applied. In the second stage, then, a further explanation is needed of how the subject matter of the mathematics is related to the domain of application. Though differing in certain respects, all three interpretations can specify these relations in terms of the structural relations I have emphasized. In a precise sense, applications turn on merely structural relations, on these approaches.

To be fair, Pincock does offer an account of what he means by models, representations and contents in Chapter 2. The view that he defends is that a model is a purely mathematical structure; it is not a representation until it is claimed to be about a specific target system. So it is only once a model is provided with a physical interpretation that we obtain a representation with content. Content is to be understood as the target system specified in a certain way, so that a representation is accurate if the target system is indeed that way, where "that way" is specified in terms of the shared structural relations between the representation and the target system. This, then, is where the structural aspect of his account of representation is to be found. Again as we are told in Chapter 2 (25):

> [t]he main claim that I try to defend about the content of mathematical scientific representations is that the content is exclusively structural. By this I mean that the conditions of correctness that such representations impose on a system can be explained in terms of a formal network of relations that obtains in the system along with a specification of which physical properties are correlated with which parts of the mathematics.

But there is a substantial philosophical literature on the issue of just what these structural relations amount to. For example, to mention a few options, some hold that they are given in terms of isomorphism, others believe they are given in terms of embeddability, and yet others believe they are given in terms of partial isomorphism. Still some argue that it's not the structural relations themselves but rather the inference patterns that allow for representation and, hence, for applicability. Finally, and perhaps most problematically, Psillos holds that there is a deep divide between structure and content so that a purely structuralist account of content is not possible.

And, moreover, lying behind these philosophical options is component (2) above, namely, the corresponding structuralist account of the nature of scientific theories. Again, in Chapter 2 (26), we are told:

> [a] theory for some domain is a collection of claims. It aims to describe the basic constituents of the domain and how they interact. A model is any entity that is used to represent a target system. Typically we use our knowledge of the model to draw conclusions about the target system. Models may be concrete entities or abstract mathematical structures. Finally, a representation is a model with a content.

But again there is no connection made between Pincock's structuralist account of scientific theories and those found in the philosophical literature. As a result, we are left with many questions. Are theories themselves to be thought of semantically, as a family of models, or syntactically as a set, of perhaps Ramsified, sentences? Are models themselves structures, or do models have a structure? Are models interpretations, or do models have interpretations? Does talk of structure have to be framed set-theoretically? Finally, there is the structural realist question of whether, and to what extent, the structure of scientific theories tells us something about the world?

I, for example, have argued against French, and other structural realists, that no such framework, either philosophical or mathematical, is needed and that the notion of shared structure that does "realist work" can only be specified locally, that is, in specific instances of applicability, so that "global" appeals to isomorphism, partial homomorphisms, etc., simply miss their mark. I think something like this view can be carved out of Pincock's by further linking these local instances of applicability to his five types of epistemic contributions; but it's just too hard to tell. It would have indeed been quite helpful then, if, while providing all the rich and robust mathematical details of his examples of the five types of epistemic contributions, Pincock would have at least told us what notion of shared structure was at play in each. I am not saying that Pincock has to take up *all* of these issues regarding his structuralist account of scientific theories, but I do think both that he ought have mentioned that there are weighty philosophical considerations that need to be investigated.

As noted, Pincock does take up component (3) in greater detail in Chapter 14, that is, he does consider the question of what philosophy of pure mathematics might be consistent with his structuralist view of applicability and also might provide an interpretation that assigns the right truth-values to the claims of pure mathematics. And, in so doing, if we are to make good sense of the epistemic contributions of mathematics, offers the philosophical underpinning to his account of a priori justification that is needed for his version of the indispensability argument. His aim here is to show that there are no arguments from considerations of applications to an interpretation of pure mathematics and, conversely, that a rejection of structuralism about pure mathematics would not undermine his account of applicability.

When considering the ontological question of whether one ought to prefer Shapiro's structuralist *ante rem* approach to the more common "object Platonism" we are told (284) that:

> [e]ven if this sort of argument [from consideration of applications] could be found, it would still not be enough to mandate an interpretation of mathematics in terms of abstract structures…. There are any number of alternative interpretations that have a broadly structural flavor but dispense with abstract structures.

After rejecting Benacerraf's "learning" account of structuralism, we are left with the similar conclusion that (285)

> [t]he structuralist approach to applications might naturally suggest a structuralist interpretation of pure mathematics, but there is no clear argument from one to the other. The structuralist approach to applications is too open-ended to rule out any interpretation of pure mathematics that renders the right structural claims true.

Having thus failed to ontologically motivate an account of mathematical structuralism that might remain consistent with his structuralist account of applicability, Pincock turns next to consider issues of epistemology. Here we are reminded that we need an account which underpins the claim (285) that

> (…) some sort of a priori justification for pure mathematics is needed if we are to make sense of how mathematics makes its epistemic contributions to the success of science.

That is, both his recasting of the indispensability argument and his account of the role of constitutive representations require that some "central" parts of mathematics be justified a priori. Cutting a midpoint between the conceptual and empiricist approaches of Peacocke and Jenkins, respectively, Pincock here uses his "extension-based" epistemology to show at least that an epistemology underpinning such a claim is possible. The idea here is that "concepts can be grounded by considerations internal to mathematics" (295), so that his extension-based account can start (295–6)

> (…) with the assumption that mathematicians have achieved some degree of mathematical knowledge at some stage in the history of mathematics … On this approach, then, the knowledge available in a mathematical context provides a crucial part of the evidence in favor of new mathematical claims. When everything goes well, the grounded concepts and the presentation of the right known theorems can extend our mathematical knowledge.

Pincock next provides two interesting historical examples that show that, at least in some cases, appeal to a "purely mathematical" inference to the best explanation [IBE] is made which itself leads to the extension of a concept. What is confusing is the leap from this to the conclusion that "[a]n extension-based epistemology for mathematics that puts this sort of [IBE] reasoning at the center is consistent with the conclusion that mathematical knowledge is a priori" (297). As Pincock first concedes, even making use of mathematical IBEs to extend our knowledge claims, we have not so easily dismissed the empiricist. Again aiming to carve out a mid-position, he places himself in agreement with Shapiro, allowing that "perception of small collections of things is an important part of the justification for some of our mathematical beliefs" (298) but adds that we may also call into play purely mathematical IBEs to extend our mathematical knowledge. While I still have my doubts about whether this licenses the conclusion that mathematical knowledge is a priori, I leave it to the reader to decide.

What remains as more problematically confusing, however, is that we are then told (298)

> [t]his approach is consistent with the *ante rem* structuralist interpretation of pure mathematics, but departs from the epistemology articulated by Shapiro.

Something is clearly amiss. Shapiro is a platonist about structures, and recall that the aim of Part II is to show that philosophically speaking one *only* need be committed to *realism about truth-values*, and this commitment need *only* be understood *epistemically*. In what sense, then, does Pincock's epistemic account of the existence of structures depart from Shapiro's? Essentially the idea is that while Shapiro runs a "coherence argument" for the existence of structures, Pincock's extension-based epistemology requires only that (298)

> (…) agents must obtain evidence for the existence of the extensions they propose. This evidence may come from reflection on properly grounded concepts, or it may come from proofs whose premises are not all based on the features of relevant concepts. In particular, we allow explanatory considerations to provide evidence for mathematical claims.

Is Pincock then advocating for some type of explanatory platonism about structures themselves? It's hard to tell. But clearly we have moved from claims about the truth-values of mathematical claims, including those claims about structures, to claims about the existence of mathematical structures.

Continuing on to providing an extension-based account for the concept of mathematical structure itself, we are told that (298)

> [s]et theory delivers so many structures that it is possible to locate nearly any domain in a domain made up of sets. Prior to the acceptance of set theory, each new extension was challenged by the mathematics community and its acceptance came about only after a hard-fought struggle. After set theory was accepted, these debates about mathematical existence became less and less a part of mainstream mathematics. If extension-based epistemology of mathematics is to be vindicated, then it must provide a compelling reconstruction of this turning point.

Again there is a problem: Set theory can only play this ontologically explanatory role if there is but one set theory that acts as a foundation, but, if one adopts a structuralist approach to set theory itself then there is no reason to prefer, say, ZFC to GB set theory. In any case, we might get to the existence of set-structures but we do not get to the existence of structures themselves. Even Shapiro concedes that his *ante rem* account only suffices for non-algebraic structures. But surely mathematical representations make use of algebraic structures. So unless Pincock adopts the view that *all* structures "really are" set-structures and, perhaps more challenging for his account of scientific representation, unless he adopts the view that shared structure "really is" shared set-structure, we are left wanting an account of what possible explanation, either scientific or mathematical, ontic or epistemic, could license the existence of structures.

Thus, it is not just Koellner's pluralism that threatens to overturn the cart that carries Pincock's account of mathematical structuralism and so throw wide his entire account of scientific representation, it is the new and ever changing conceptions of just what a foundation should be. So while Pincock is right that (299)

> [w]hatever the outcome of this debate, it remains imperative for philosophers of mathematics to return their focus to the history of mathematics and the debates about its development. Only in this fashion can we provide an epistemology of mathematics that renders it a priori. This is the key to understanding how mathematics contributes what it does to the success of science.

The problem is that there appears to be at least two distinct histories (for example, the Fregean and the Hilbertian) and Pincock leaves us with too little to decide between them. While some, like Shapiro and Hellman, have advocated set theory as a Fregean, constitutive, foundation, others, like McLarty and myself, have argued for category theory as a Hilbertian, organizational foundation. In any case, however, none but Shapiro would take this philosophically reconstructed history as a basis for any claim about the existence of structures.

Now, I am not saying that Pincock needs to be committed to taking category theory as an organizational foundation, but I am saying that often times the commitment to set theory as a constitutive foundations has led to claims among scientific structuralists (see Suppes and French, for example) that the notion "shared structured" needs to be globally and formally framed in set-theoretic terms. I have elsewhere argued against such a position; claiming instead that no such formal framework, even one given in category-theoretic terms, is needed because both the notion of shared structure and the IBEs that are used in structural realist accounts of applicability are local. I do think that Pincock and I might be in agreement about this point, but, again, it's too hard to tell.

In sum, while Pincock has done us a great service in working out the mathematical details of the various types of epistemic contributions that scientific representations make, he needs to tell us more about the explanatory epistemology (as opposed to the explanatory ontology) of mathematical structures so we can philosophically work down to talk about the structure of scientific representations. And too he needs to tell us more about the specific, and I believe local, structure of scientific theories so we can philosophically work up toward an epistemically tractable structuralist account of applicability that works out the details of the role of shared structure.

Sorin Bangu

Pincock's *Mathematics and Scientific Representation* (MSR) is a rich book, addressing virtually all important themes in philosophy of science and mathematics. In fact, it is so rich that it is almost two books. The first, part I of MSR, is a detailed discussion of the "epistemic contributions" of mathematics to science; the second, part II, is less compact and talks about "other contributions" (but many of the topics are approached in light of what is said in part I). The work as a whole is not only a sustained effort to engage with perennial topics in philosophy of mathematics and science, but also an attempt to intervene in some recent debates in epistemology, especially on the status of the a priori (in the last chapter). Pincock displays admirable patience in disentangling the various aspects of the issues he tackles, and does this by appealing to numerous scientific and mathematical examples.

The book is a new and welcome addition to a series of distinguished works which take the applicability, and the actual applications, of mathematics in science as the primary material for philosophical reflection. Roughly speaking, this is an area of investigation that to a large extent leaves behind the traditional concerns of the philosophy of mathematics (e.g., is mathematics reducible to logic? Is platonistic realism an epistemically viable view?) and looks at the philosophical puzzles raised by mathematics from a broader, scientifically-informed perspective. The central question asked in this literature is, in essence, what is the proper way to understand the relation between mathematics and science, and how such an understanding illuminates the nature of both. However, the focus on applicability oftentimes yields new answers to the old questions, and a good example of this is how the so-called indispensability argument revives mathematical realism.

Although I am sympathetic to the general orientation of the book, and to what I take to be the project pursued in it—namely, to show that "mathematics does more [for science] than just allow us to derive consequences of non-mathematical claims"

(199)—I will devote most of my contribution to this symposium to articulating some (hopefully constructive) criticisms. For reasons of space, I'll limit my commentary to two issues. I shall first examine Pincock's very motivation to embrace the "epistemic" conception of the contributions of mathematics to the success of science. I see it as grounded in his discontent with what he calls the "metaphysical conception", and thus, I will try to clarify what this conception is, and what its shortcomings are. While the contrast between the metaphysical and epistemic conceptions might have just been a peripheral issue for Pincock, I belive that this discussion serves as a good preparation for a more general challenge to the main thesis of the book. That is, I will question the gist of the epistemic conception, the idea to emphasize the role of mathematics in facilitating the (dis)confirmation of scientific theories. Second, since I am not entirely happy with some aspects of Pincock's take on the indispensability arguments (discussed especially in the second part of the book, chapters 9 and 10), I will make a couple of quick critical remarks on his position.

The goal of the book is "to account for the presence of mathematics in our scientific representations. The central issue is to see to what extent the central place of mathematics in science contributes to the success of science." (3) Pincock is wisely aware that the desired account can't be "exhaustive" (198) and must show sensitivity to the diversity of situations in which mathematics is applied, since "what a given piece of mathematics contributes to the success of science in one representation can differ from the contribution from some other piece in some other context" (4). The account is an "epistemic" one. Most generally put, we find mathematics in science because it helps us in knowing the world, or (less vaguely) helps us with expressing this knowledge in a form which is particularly desirable: by employing mathematical objects and structures, scientists can formulate mathematical representations of many physical situations, and, as we'll see shortly, what is primarily desirable about these representations is that they can be more easily (dis)confirmed than other types of representations.[4]

As I said above, in order to see the motivation to embrace an epistemic conception it is important to understand what the alternatives to this conception are, and what is unappealing about them. The other elements of the contrast class, as Pincock constructs it, are the "sociological" and the "metaphysical" conceptions. Below I will spell out these two contenders, but in the case of the metaphysical conception this will turn out to be a difficult task. Along the way, several questions will crop up: is it possible to actually isolate an epistemic viewpoint when it comes to the contribution of mathematics to science? And, even if one eventually accepts that an epistemic conception can be articulated (the way Pincock does, by connecting the use of mathematics to the confirmability of theories), is this conception actually an attractive option?

There is not much to say about the first contender, the social conception, so we can begin with it. According to this conception, what is important about mathematics is "the respects in which [it] contributes to the convergence of

[4] Pincock talks in terms of "confirming a representation" and this must be, I take it, a shortcut for the idea that we confirm certain *claims* about the world made on the basis of representing it in a certain way.

scientists on a given representation" (4) When speaking of "the convergence of scientists", Pincock has in mind the fact that one way to appreciate the role of mathematics in science is to point out its fundamental role as a common language of scientists, which in turn facilitates reaching consensus on what the problems (and the solutions) are. This is of course true but it strikes Pincock (and I agree with him) as a rather superficial aspect of mathematization. Hence I shall leave the matter here, and not count this idea as a promising approach when it comes to trying to understand the role of mathematical formalism in contributing to the success of science. But there is a much more serious contender, and this is the metaphysical conception which I will discuss next.

One of the first things Pincock says when introducing it is that "According to the metaphysical conception, mathematics contributes to accurate scientific representations because the physical world is essentially mathematical." (4) This means that "an inventory of the genuine constituents of the physical world reveals that these constituents include mathematical entities." (4) And also: "…metaphysical conception. That is, the physical world is essentially mathematical…" (13) On the basis of this characterization I was reading Pincock's rejection of the metaphysical conception as a rejection of a form of Pythagoreanism, the ancient idea that the world is literally made of numbers. But I'm not sure this is the right reading here. Pretty much like the social conception, this conception doesn't have any serious defender nowadays, so in the end, I don't believe that this is what Pincock takes the metaphysical conception to be.

Another way to clarify the metaphysical conception is by reflecting on passages such as this one:

> [I]t is hard to deny that there are certain cases that fit the metaphysical conception of the contribution of mathematics to the success of science. At the same time, there is good reason to think that this cannot be the whole story. In the vast majority of cases in which we find mathematics being deployed, there is little inclination for the scientist to take the mathematics *to reflect the underlying reality of the phenomenon* in question. (4; italics added)

Therefore, to advocate the metaphysical conception is to take mathematics "to reflect the underlying reality of [a] phenomenon"— and this is something we should avoid. My inclination is to construe the rejected view here as a form of (theistic) Keplerianism. This is the idea that while mathematical and physical objects are different in nature, the relations between physical objects instantiate, or reflect, (mathematical) relations between mathematical objects. (As is well known, a particularly striking illustration of this insight is in Kepler's astronomy, where the distances of the planets from the sun were supposed to somehow correspond to the geometrical ratios calculated for the nested five Platonic solids.) So, when speaking of "the underlying reality", and rejecting the idea, does Pincock reject theistic Keplerianism?

Again, I find it hard to tell, in part because there is no direct textual hint of this. But note, however, that this type of contribution which mathematics was taken to make to science (by Kepler, for one) surely deserves the label "metaphysical"; moreover, there are immediate and strong *epistemic* connotations of this conception.

This seems to be a typical case when a metaphysical/theological view has direct epistemological import: by studying pure abstract mathematics one also gets to know about the concrete physical world—since God made the latter using the former. But the fact that metaphysics and epistemology mix so naturally here should be concerning for Pincock (if this is what he has in mind when speaking of the metaphysical conception), because it makes one suspect that the two kinds of conceptions (epistemic and metaphysical) can't in fact be treated separately the way he would like to.

My final attempt to understand what is the metaphysical conception and its shortcomings draws on Pincock's next point made in the previously quoted passage, namely that "In the vast majority of cases in which we find mathematics being deployed, there is little inclination for the scientist to take the mathematics to reflect the underlying reality of the phenomenon in question. *We see this, for example, whenever a representation employs a mathematical idealization*." (4; italics added)

The new aspect introduced here, and a central one for the book, is the presence of idealizations in science. How does this help us in understanding what the metaphysical conception is and why it has to be abandoned? An idealization represents certain physical objects or phenomena and contains literally "false" claims about them (4) Because we do this through mathematical means, the end-result, the mathematized theory, "does not reflect any underlying physical constituents of the system" (5) Simply put, by looking only at the mathematical representation one would get the wrong idea of what the world is like; importantly, the causal nexus is typically not revealed (though sometimes it is, since not all representations are "abstract acausal", as Pincock calls them; these are cases when mathematics does "track" causal relations). Hence the role of mathematics is not to represent the world accurately—or rather, to represent *all* aspects and details of a physical context—but to help representing only those aspects that happen to interest us. Therefore, it looks as if embracing the metaphysical conception would amount to falling prey to this error, that is, to believe that mathematics does reflect "the underlying reality" of a certain phenomenon.

Now it is true that back in Kepler's days many probably did believe this; but I doubt that this type of error—which, I take it, is the very reason for discarding the metaphysical conception—is common nowadays, among either philosophers or scientists: we surely are more modest and cautious in interpreting modern scientific theories (as Pincock himself points out in one of the previous quotes). So, to "go epistemic" on *this* ground seems to me rather insufficiently motivated. Moreover, if the prevalence of idealizations in science is what drives Pincock away from the metaphysical conception, then one of the problems I highlighted above reoccurs—to repeat, can metaphysics be kept out of the picture? Traditionally, the philosophical reflection on scientific idealization is a mixture of epistemological *and* metaphysical themes, raising the same questions asked for the first time back in Galileo's days: What is the relation between what we get to know from studying idealized, simplified models and what we can claim about the messy, real world? Can we extrapolate our reasoning on simplified cases to the realistic cases? And so on.

To summarize the discussion so far. I'm in agreement with Pincock that the social conception is a non-starter, but I'm unsure how to spell out the metaphysical conception, and thus I find it somewhat unclear what kind of worry makes it unappealing. Also, a more general concern persists, that when it comes to the philosophical issues pertaining to the applicability of mathematics one can't actually separate the metaphysical and the epistemological aspects.

Let me now proceed to an examination of the gist of the epistemic conception, according to which the "essential [epistemic] contribution [of mathematics] to the success of science…is the fact that we take our evidence *to confirm* the accuracy of our best scientific representations." (8; italics added)

In light of some of the previous points, I will try to summarize (what I take to be) Pincock's epistemic position in terms of a crude photographic metaphor: mathematics plays a role in science because scientists oftentimes need to "zoom-out". When looking at the mathematical equations found in science we typically get a sense of the higher-level structural relations, which are often isolated by employing scaling techniques—hence no wonder that Pincock devotes a whole chapter (the fifth) to their analysis and endorses structuralism. On this basis, Pincock draws an interesting connection between the casting of scientific theories in an abstract mathematical framework and their confirmability. He says: "the main conclusion that I argue for in this book is that mathematics makes an epistemic contribution to the success of our scientific representations. Epistemic contributions include *aiding in the confirmation* of the accuracy of a given representation through prediction and experimentation. But they extend further into considerations of calibrating the content of a given representation to the evidence available, making an otherwise irresolvable problem tractable and offering crucial insights into the nature of physical systems." (8; italics added)

To get to the challenge I mentioned at the outset, I'll try to spell out the important ideas gestured at here. First, we have to focus on what Pincock calls "abstract acausal" representations. They are described as "lacking both of the key characteristics of causal representations […]: change in time and supporting a certain kind of counterfactual variation." (51); also, they don't contain information on the "the microphysical constitution" of the physical object (or system) under consideration (64). A helpful example is to consider the seven (actual) bridges in the town of Koenigsberg, and represent them as a mathematical object—as a graph (drawn on p. 52).[5] Such a representation (originally due to Euler) qualifies as "abstract acausal", since the graph doesn't represent the change of the physical bridges over time and is consistent with various possible micro-constitutions of them.

We can now reflect on what Pincock proposes as the connection between this kind of representation and confirmability:

> [A]n acausal representation […] affords a representation of features of interest of a system that may not be represented by any causal representations. Second, *it is typically easier to confirm or disconfirm because it has less content (in my sense) that its causal associates. Because it says less about its targets, a successful test will provide significant support.* By contrast, the associated

[5] Pincock credits Wilholt (2004, 287) with discovering this example, who in turn found about it in a conversation with Haim Gaifman (private communication).

causal representation will remain harder to confirm because of its rich causal content and detailed representation of a wider range of target systems. (58; italics added)

The issue I want to raise is an immediate consequence of the emphasized part of the passage. If we read it carefully, it looks like the gain afforded by mathematization—easiness of dis/confirmation—is offset by a loss of "content". Therefore, on one hand, mathematizing helps in simplifying testing the accuracy of this type of representations, and thus, is an epistemic aid in deciding whether they are confirmed or not. Yet, on the other hand, mathematization seems also an epistemic hindrance, as it diminishes these representations' content. Thus, roughly put, as a result of mathematization, we end up knowing both more *and* less about the world. Hence the challenge: even if we accept that mathematics plays an epistemic role, is this role a *positive* one? Isn't the gain pretty much canceled out, or significantly diminished, by the loss incurred? How are we to judge the net result? Or, to repeat the general worry I began with: is this conception actually an attractive option? Note, however, that by saying this I 'm not saying that Pincock is out of answers here; I'm only pointing out that I wasn't able to see them fully explored in the book.

I will now switch gears and discuss an issue from the second part of the book, namely the treatment of the indispensability arguments (IA henceforth).

Pincock is somewhat sympathetic to the IA, but likes a version of it which, in my opinion, is unduly cautious.[6] For Pincock, the IA establishes *only* that mathematical claims incorporated in a confirmed scientific theory are true—and this position has been dubbed "realism in truth-value" (by Shapiro 1997). But Pincock also thinks the alternative to this position, "realism in ontology" (also Shapiro's term), the more definite view that mathematical objects should be included in ontology, can't be supported on the basis of indispensability considerations. Realism in truth-value is of course still realism, and thus, does have some metaphysical bite. For one thing, it is incompatible with the increasingly influential fictionalism (in essence, the view that mathematical statements are false because the mathematical objects they talk about don't exist, or, more precisely, that these statements are *literally* false, but somehow true "in-the-story-of-mathematics"). I share Pincock's skepticism about this view (see ch. 12), and I especially like one argument he advances to back it up. (For reasons of space I can't discuss it, but the important bits are on p. 199 and 255.) So, what I am discontent with here is Pincock's prudence,[7] and below I will try to identify its twofold source.

First, Pincock is unsure whether first-order logic "is really the best way…of regimenting the language of science" (193). If so, one can't apply (Quine's) criterion of ontological commitment to the first-order versions of the scientific and

[6] My positions in this section reflect the views I articulated in a recent book on the IA, argument which I take to support a non-platonistic form of realism in ontology (See Bangu 2012).

[7] There are other problematic aspects of Pincock's discussion, but there is no space to present them here. A central one is the absence of a thorough examination of the role of confirmational holism within the IA, and thus a reaction to Maddy's (1997) objections. It seems to me they have to be addressed even if one defends only the "realism-in-truth-value" version of the argument.

mathematical statements, hence one can't extract any mathematical "objects" to include in ontology. The second reason to accept only realism in truth-value (and doubt realism in ontology) is Pincock's sympathy for the strategies of reinterpreting mathematical claims such that their truth doesn't require the existence of "objects", this reinterpretation being typically effected in modal terms (as done by Hellman, Chihara, and earlier on by Putnam).

Is first-order logic the best way to regiment science? To begin with, for a Quinean the choice of a logic is made by following the same principles of theory-choice applied in science more generally. First-order logic is not chosen a priori or arbitrarily, but by comparing its features with possible alternatives.[8] While it is surely conceivable that first-order logic loses out in the end, such an outcome has to be argued for seriously. Pincock (and those entertaining this thought) might have already done such a thorough comparative analysis, but I fail to see it in the book. Similar considerations can be advanced about the modal strategy. This is no doubt an interesting project, but not one on which there is consensus that it succeeded—quite the contrary. There are rather convincing (to my mind) objections to this strategy (some in Shapiro 1997) and I might have missed Pincock's take on them in the book. Moreover, the strategy comes under attack from a different angle, the now-classical Quinean battery of arguments against the very meaningfulness of the modal talk. So, in the end, the question is whether these criticisms do incline the balance toward the realism in ontology conclusion of the IA. Since one can't cover everything in a book, I'm of course aware that Pincock might in fact have good reasons to believe they don't; but until the Quinean learns about them she is entitled to hold on to that conclusion.

Despite these reservations, I believe the book provides plenty of valuable material for reflection to those interested in recent scientifically-informed philosophy. It offers both a panoramic view of the current debates and detailed analyses of scientific case studies. It will hopefully generate constructive discussions and thus, will contribute to tightening the connections between the philosophy of mathematics and philosophy of science.

Author's reply: Christopher Pincock

I will begin by thanking each of the contributors for their helpful and thoughtful discussions of my book. It is certainly an honor to have this opportunity to read their reactions and further develop my arguments. As I say in the preface, "I look forward to other proposals that my efforts might generate" (xiii). The objections and responses offered by Balaguer, Landry and Bangu are just what I had hoped for.

I owe perhaps the greatest debt to Balaguer as it was partly through an extended engagement with his 1998 book that I developed my own epistemic approach to the value of mathematics for science (see Pincock 2007, 262). By knowing pure mathematics, it becomes much easier to know the science that we know. At the heart of this picture is the claim that, as I put it at the beginning of my chapter on

[8] Hylton (2007) offers more details on Quine's reasons for preferring first-order logic.

fictionalism, “to understand a given mathematical scientific representation, an agent must believe some mathematical claims” (243). The pure mathematical claims pertain to a purely mathematical structure. I claim that many scientific representations involve purported relations between that structure and some physical target system. Experimental evidence can then be used to determine the extent to which these relations obtain and the representation is accurate. Over time, this procedure generates scientific knowledge. This is why I take Daniel Bernouilli’s slogan as my epigraph: “There is no philosophy which is not founded upon knowledge of the phenomena, but to get any profit from this knowledge it is absolutely necessary to be a mathematician.”

In the chapters leading up to the discussion of fictionalism I argue that this epistemic approach is consistent with some anti-platonist philosophies of mathematics such as Hellman’s modal-structuralism. For this reason, indispensability arguments for platonism fail. To this extent, Balaguer and I are in agreement. But we disagree on the viability of a fictionalist approach to mathematics: Balaguer thinks it is defensible, while I argue that it is ultimately incompatible with the epistemic approach. In his essay Balaguer argues that a certain sort of fictionalist can adopt the epistemic approach. I do not think this argument is successful, but it has been very helpful to engage with it as it shows how certain things I say in the book are misleading and obscure my primary argument. I want to summarize that argument now in a way that is hopefully clearer than what I say in the book. Then I will try to determine how Balaguer’s discussion bears on this argument.

The challenge I raise to the fictionalist is what I call the export problem: “The fictionalist must isolate the representational contents from the fictional contents and explain why the weaker representational contents are the proper targets of our genuine beliefs” (251). Balaguer labels the first part the “negative” export problem and the second part the “positive” export problem. For any given scientific representation or theory, the fictionalist grants that the claims should be taken at face-value. This generates the fictional contents of the theory. Balaguer captures this with talk of “what would have been true if platonism had been true”. But the fictionalist himself does not believe the fictional contents. Instead, he retreats to what I call the representational contents of the theory: “the physical world has a nature that makes it the case that if platonism was true, then ES [our best empirical science] would be true”. Balaguer claims that this yields a solution to the negative problem, and argues that the positive problem need not be solved by the fictionalist.

I did not intend to raise two separate problems. To isolate appropriate representational contents one must show that these contents are the ones we should believe. This is because “the representational content is used to assess ontological commitment” (254). Or as I try to summarize the argument before diving into the details, “My argument is that this challenge cannot be met because any set of rules that are detailed enough to do the job will presuppose knowledge of the actual world that we do not have. This forces the fictionalist to undermine the epistemic contributions from mathematics to the success of science that I discussed at length in part I” (252). Here is perhaps a better way to summarize the intended argument. The value of mathematics to science is that it helps to generate scientific knowledge. In this sense, scientific knowledge presupposes mathematical knowledge. The fictionalist denies mathematical knowledge, so he removes a crucial presupposition of the scientific knowledge that we now have. For this reason, it is not reasonable to use that very scientific knowledge to determine one's ontological commitments. And I assume that there is no other kind of knowledge available to accomplish this task.

One way that these issues play out in the chapter itself is when I imagine a fictionalist using how the world actually is to isolate the representational contents of a given theory. This is not legitimate because representational contents determine an ontological commitment and, on this view, "this commitment obtains or fails to obtain independently of the evidence that the speaker has" (254). Balaguer does not adopt this solution, but I believe that his solution faces the same basic problem. Balaguer relies on stipulation to determine the representational contents. This proposal is advanced in his discussion of cases like minds or spacetime points where it might not be clear if the objects are abstract or concrete: "I can avoid the worry about minds by simply stipulating what *I* mean by an abstract object", and later, "My fix of the situation would be to simply stipulate what *I* mean by an abstract object". However, if this is the means that the fictionalist uses to arrive at their representational contents, and it is these contents that determine ontological commitment, then the fictionalist is fixing the objects they believe in based on a stipulation. These commitments are not appropriately constrained by the scientific evidence available.

Perhaps a charitable defense of Balaguer's fictionalism would invoke a distinct kind of non-scientific evidence that is available to justify or motivate the stipulations in question. The causal separation between abstract and concrete objects is something that Balaguer often emphasizes. Here he says "given that the two sets of facts hold or don't hold independently of one another, it seems reasonable to believe that the nominalistic facts obtain and the platonistic ones do not". It is not clear to me what notion of "reasonable" is in play here. I would concede that the fictionalist can arrive at a consistent set of commitments by beginning with the findings of our best science and removing some beliefs. But if the beliefs are removed independently of the evidence available, then the resulting commitments are not reasonable. I do not think that the fact of causal isolation acts as a kind of evidence that is relevant to reasonable belief. As I have already noted, Balaguer takes this sort of worry to be the "positive" challenge, and rejects it: "if we merely want to defend fictionalism against objections, then we don't have to answer the positive export challenge". So, perhaps there is a kind of stalemate familiar from other philosophical debates. My objection to fictionalism is ultimately a demand to motivate the fictionalist's policies for determining reasonable ontological commitments. The fictionalist refuses that demand. The fictionalist might be able to articulate and defend some notion of rationality that fits with their commitments, but until they do so, it is hard to see how the debate can move forward.

Landry has developed a sophisticated structuralist approach to mathematics, science and representation, and so it is only natural for her to focus on these aspects of my book.[9] She claims that there are actually three different structuralist views

[9] See Landry (2012) for a recent overview as well as many references to earlier work.

developed in the book: a structuralist account of the applicability of mathematics, a structuralist account of scientific theories and a structuralist account of pure mathematics. However, she objects that "no account of any of these components is worked out in the detail that it ought to be". I am not sure that I understand the objections she raises to the three structuralisms that she finds. I believe that everything that I say is consistent with Landry's own local, organizational approach to these issues. I will first summarize the views that I defend in the book and then respond to some of Landry's more specific complaints.

My primary goal in the book is to determine how mathematics contributes to the success of science. The means that I adopt to achieve this goal is a theory of mathematical scientific representations. These representations are not full-blown theories in the sense of classical mechanics or population genetics. They are instead local, context-dependent representations of particular target systems or classes of target systems. For example, I develop my account of scientific representation with reference to the heat equation and what has to be settled for it to say something about a given iron bar. As Landry notes, I use the terms "theory", "model" and "representation" in a special way that fits this general approach. I do not claim that this is the only way to use these terms and I do not offer my theory of representation as an analysis of how scientists or philosophers use these terms. Instead, I admit that "I am forced to make some controversial assumptions" and claim that these assumptions "are part of one package of views that will prove sufficient to make sense of the central role of mathematics in science" (25).

My aim in chapter 2 is to spell out as clearly as possible one way of thinking about mathematical scientific representation. Some of the assumptions that I initially deploy in chapter 2 are weakened in later chapters. For example, I begin by assuming a traditional objects platonism interpretation of pure mathematics, but argue later that many alternative views fit with my approach to representation. More importantly, I initially make certain simplifying assumptions about mathematical and scientific concepts that are then questioned in chapter 13 where I engage with Wilson. The structuralist account of applicability that I wind up with, though, is not much different than the view summarized in chapter 2: "for any given mathematical scientific representation, we have found its content if we can answer three questions: (1) What mathematical entities and relations are in question? (2) What concrete entities and relations are in question? (3) What structural relation must obtain between the two systems for the representation to be correct?" (27). I distinguish four increasingly sophisticated kinds of content that result from different sorts of answers to these questions: basic, enriched, schematic and genuine. When we move beyond basic contents, I clearly state that a wide variety of structural relations are available beyond simple isomorphisms and homomorphisms: "we allow the specification of the structural relation to include mathematical terminology" (31).

Now Landry is right that "there is a substantial philosophical literature on the issue of just what these structural relations amount to", but I do not see why I need to engage with it here given the contours of my project. Other structural approaches to representation may also be sufficient to make sense of the value of mathematics to science, and I do not intend to rule them out. I mention briefly van Fraassen's views and the "inferential" approach suggested by Suárez, Bueno and Colyvan

(28–29). It would be a problem for my view if the assumptions I adopted were inherently problematic or vulnerable to some devastating objections. But Landry does not raise any problems or even discuss the details of the approach to content that I develop. So it is not clear to me what worries she is raising.

I am also confused by Landry's discussion of structuralist accounts of scientific theories. As she notes, I say that a theory is a collection of claims and focus primarily on more localized representations as the vehicles of scientific knowledge. The only place where the distinction between theory and representation plays a role in my discussion is in chapter 11 where I engage with Batterman's work on asymptotic explanation (222, 241). So I do not defend any structuralist account of scientific theories such as the semantic view of theories. Landry does raise some legitimate questions about models and structures: "Are models themselves structures, or do models have a structure? Are models interpretations, or do models have interpretations? Does talk of structure have to be framed set-theoretically?". On my view, a model may be a concrete object or an abstract mathematical structure. It is a thing composed of objects with properties and standing in some relations. In the abstract case, a model may both be a structure and also have a structure. Models are provided with physical interpretations along the lines described in Sects. 2.2–2.4 and summarized above. There is no need to describe structures in set-theoretic terms, although set theory is often a valuable tool for this purpose.

Many of Landry's critical comments are directed at my discussion of structuralism about pure mathematics in chapter 14. She notes that I do not defend a structuralist view of pure mathematics, but that I say instead that "considerations based on applications cannot be used to support objects platonism against ante rem structuralism" and also that "There are any number of alternative interpretations that have a broadly structural flavor but dispense with abstract structures" (284). Still Landry is critical of my claim that my approach to applications is consistent with Shapiro's ante rem structuralism if it can be supplied with an a priori epistemology. I do not see what is "clearly amiss" here. Landry may think that Shapiro's interpretation of pure mathematics mandates an empiricist epistemology. But this is not the case. I do grant a potential role for perception of small collections in an epistemology for structuralism: "Experience manipulating 5 stones and 7 stones may serve to ground the appropriate concepts here" (298). However, it is important to note that here I use Jenkins' notion of empirical grounding. Jenkins argues that an empirical grounding of concepts is consistent with the a priori justification of beliefs (291). Landry may disagree, but she does not discuss Jenkins' argument. Finally, I would emphasize that I do not present this proposal as a polished epistemology for pure mathematics, but only "as a plan of attack on this daunting problem" (286). Landry may think that I have failed to sketch a viable plan, but I am unable to see what her objections are.

Bangu's recent book *The Applicability of Mathematics in Science: Indispensability and Ontology* (Bangu 2012) discusses many of the same issues that I pursue in my book. Bangu has a somewhat different perspective on applications and I would encourage anyone interested in mathematics and science to consider his arguments carefully. I certainly hope to do so in future work. In his essay here

Bangu asks for further clarification of the contrast between the metaphysical and epistemic approaches that I discuss in chapter 1. A related worry is whether the epistemic conception of the contributions that mathematics makes to science is substantial enough to account for the widespread presence of mathematics in science: "is this conception actually an attractive option?". Bangu also presses me to further motivate my rejection of the indispensability argument for platonism (as opposed to realism in truth-value).

The conceptions that I distinguish are general accounts of how mathematics contributes to the success of science. We find that mathematics is deployed in many scientific representations and this naturally raises the question of what the mathematics is doing there and how its presence might be helping scientists to arrive at accurate representations of the physical world. As Bangu notes, I introduce the metaphysical conception by saying that it is the view that "mathematics contributes to accurate scientific representations because the physical world is itself essentially mathematical" (4). The explanation that I had in mind here says that our best science has a lot of mathematics in it because a complete description of the physical world requires mathematical terms. And this requirement is further motivated by the claim that mathematical entities are themselves part of the physical world. This view is perhaps too outlandish to be taken seriously by most philosophers of science today, but it has a venerable history. In the book I mention Franklin, who traces his own "Aristotelian realism" to Aristotle (Franklin 2009). For example, on one interpretation Aristotle thought that our best representations of the moon mention spheres because the moon is actually a sphere (Distelzweig forthcoming).

I go on to contrast this metaphysical approach with an epistemic conception of the contribution of mathematics to the success of science. I accept Bangu's criticism that this conception is not that clear. Here is what I say when I introduce it: "even when mathematics is not playing the metaphysical role of isolating fundamentally mathematical structures inherent in the physical world, it can still be making an essential contribution to the success of science. For part of this success is the fact that we take our evidence to confirm the accuracy of our best scientific representations. And it is here that the mathematical character of these representations makes its decisive mark" (8). Later in the same chapter I summarize things this way: "The contribution of the mathematics is in helping us to formulate and investigate representations that we can actually confirm" (12).

Bangu argues that the distinction between these two conceptions is unclear and that as a result "when it comes to the philosophical issues pertaining to the applicability of mathematics one can't actually separate the metaphysical and epistemological aspects". My reply is that the epistemic conception does not assume that the physical world is essentially mathematical and so it takes issue with a fundamental component of the metaphysical conception. A vivid way to describe the difference is mentioned in passing in chapter 10 where I discuss indispensability arguments. On the epistemic conception a complete description of the physical world at "the end of science" is possible without the use of mathematical language (199). As I conceive of the metaphysical conception, it must deny this possibility. A platonist who defends the metaphysical conception is thus something like the heavy-duty platonist discussed by Field: he takes “the relation between physical things and numbers to be a brute fact, not explainable in other terms” (Field 1989, 186).

I hope this helps to clarify the epistemic conception that I defend. I would also emphasize that one can adopt the epistemic conception and still grant Bangu’s basic worry that metaphysical aspects of the situation are important. Knowledge requires truth and truth involves representing things as they actually are. To the extent that we know something in science, we are accurately representing the facts. The character of these facts helps to establish answers to metaphysical questions. So I do not intend to rule out metaphysical questions. I only aim to reject a certain kind of metaphysical answer to the question of what mathematics is doing in science.

Still, is the epistemic conception attractive? Bangu notes that “we end up knowing both more *and* less about the world … is this role a *positive* one?”. I agree that the positive features of mathematized science should be balanced against the negative features. This is why I devote chapter 7 to an extended discussion of six cases where mathematics contributed to scientific failures (141). I argue that these failures resulted from misunderstandings of the ways in which mathematics helps in science. Furthermore, I think that over time scientists have done better at figuring out what a given application of mathematics tells us about the physical world. Ultimately, it is difficult to determine if the epistemic conception of the value of mathematics for the success of science is the whole story for why mathematics is so central to science. It is the best approach I have been able to come up with, but I hope that others like Bangu will continue to develop their own alternatives.

Bangu concludes by briefly defending Quine’s indispensability argument for objects platonism. As he notes, I argue that Quine’s notion of indispensability leaves Quine vulnerable to interpretations of pure mathematics that deliver the right mathematical truths, but that fail to quantify over any abstract, mathematical objects. In my reconstruction the crucial Quinean premise is that “Mathematical entities play an indispensable$_Q$ role in science” (195). Indispensability$_Q$ is Quine’s special notion of indispensability. I argue that it must be tied to “what is required by the best regimentation of the language of science” where “alternative regimentation[s] can be criticized only on the basis of scientific criteria like simplicity and empirical adequacy” (195).

When the argument is put in these terms, I claim that it is not clear that Quine’s own preferred regimentation in terms of first-order logic and abstract mathematical sets is really the best. Bangu suggests that the burden is on Quine’s critics to survey the possibilities and show that some non-Quinean alternative is best. However, I think the burden is on the advocate of the indispensability argument to argue that Quine’s regimentation is the best. The availability of well-worked out alternative interpretations raises the worry that Quine’s approach is not the best, even in Quine’s own terms. I make an additional point in connection with Putnam’s claim that the mathematical realm is subject to different equivalent descriptions. The objects platonist “may insist that only the platonist interpretation of our ordinary mathematical claims is acceptable” (196). However, this position requires a different argument than an indispensability argument. Putnam’s claim is about what makes pure mathematical claims true. I argue that this “is a question about pure mathematics independently of how it is deployed in science. So it must be resolved by an argument that is not an indispensability argument" (196). I take this to be one of the main lessons of my book: the metaphysics and epistemology for mathematics is best settled by attending to the history and contemporary practice of pure mathematics.

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