Critical Review of *Mathematics and Scientific Representation*

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Christopher Pincock, ***Mathematics and Scientific Representation***. Oxford: Oxford University Press (2012), xiv+330 pp., $65.00 (cloth).

Introduction. *Mathematics and Scientific Representation* is a rich and intricate book and will be of great interest to a wide range of philosophical readers. It focuses on the application of mathematics within science in all its messy detail, rather than solely on austere foundations. Christopher Pincock has a deep and science-informed understanding of a range of mathematical techniques, and much of the book engages with the application of these, deftly highlighting subtle distinctions between different uses of mathematics.

At the same time, the book tackles a more traditional topic: what we should say about the metaphysics and epistemology of physical and mathematical theory. Part 1 of the book argues that the possibility of confirmation requires that some parts of physical theory be granted a nonsemantic relative a priori status. In part 2, it is argued that in order to understand the application of mathematical claims to observable phenomena, one already needs to believe at least some of these mathematical claims.

The aim of this critical review is to set out and evaluate these two arguments. In focusing on these arguments, this review does little justice to the detailed case studies that pervade the book. While our conclusions about these specific arguments are largely negative, it is worth emphasizing at the outset that Pincock's book provides an extremely clear survey of a vast and growing literature and is in this respect to be highly recommended.

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Discussion of Part 1. The book divides appropriately into two parts. The first of these is largely dedicated to the careful discussion of applications at which Pincock excels. In chapters 3–6, Pincock introduces five kinds of epistemic contribution made by mathematics to science. Concrete causal representations track changes in a system over time and support counterfactuals. Abstract acausal representations model nondynamical features such as the properties of steady state solutions. Abstract varying representations enable us to model common features of diverse systems like harmonic oscillators, while scaling techniques allow us to focus on those features of a system that dominate at given scales. These classifications are helpful, and the account sheds philosophical light on the kinds of tricky topics recently discussed by Batterman and Wilson without ever seeking to oversimplify. But a fifth category of epistemic contribution, or, more accurately, a new distinction, is at the center of Pincock's argumentative aims. This is the distinction between *constitutive* and *derivative* representations, discussed in chapter 6.

There is nothing essentially mathematical in this distinction; although, as Pincock himself observes, it is no surprise that we find both kinds of representation in mathematical form, given the opportunity for precision that mathematics offers. Rather, the distinction relates to the notion of the relative a priori: indeed, in this chapter, Pincock hopes to propound his own version of that notion. We doubt, however, that the logical space needed for Pincock's position exists between traditional accounts of the relative a priori and confirmational holism, which does away with the notion altogether.

For Pincock, a representation is *derivative* when its success depends on the success of another, constitutive, representation (sec. 6.1, 121). This, Pincock emphasizes, is a relative notion, although it is not obvious why it must be so, and that it should be seems to be a threat to its cogency (see below). For the definition of a *constitutive* representation, Pincock turns to the notion of *relatively a priori principles*. In short, a constitutive representation seems to be a family of interconnected relative a priori principles.

The notion of the relative a priori originates with Reichenbach and Carnap. Later versions of this notion can also be found in Kuhn (1962) and Friedman (2001), whom Pincock also discusses. The common idea is of a principle or set of principles that earn their justification not through experience (hence they are a priori) but instead through a conventional choice (hence "relative," in the sense of being relative to one's practical concerns) and that "serve a crucial epistemic function in allowing the confirmation of derivative representations" (sec. 6.1, 122). In other words, their "acceptance is a necessary condition on a given [derivative] belief being rational" (sec. 6.2, 123).

For example (this is Pincock's own example, running through chap. 6), Newton's universal law of gravitation is not even qualified to be confirmed or disconfirmed—that is, we cannot rationally come to accept or reject it until we have an understanding of what *force* is, an understanding that is provided by Newton's three laws of motion. Thus, Newton's three laws are the constitutive representation to the universal law of gravitation's derivative representation.

But we should like to add a word of warning here. The claim that Newton's three laws are relatively a priori ought to seem puzzling in light of the fact that they at least appear to have substantive consequences, such as the existence of a family of inertial frames. (After all, the laws came to be rejected, and it seems their rejection did not constitute a simple change in language.) In fact, this is a result that Pincock accepts, but it was not tolerated by Carnap (or, after him, by many of the authors in the roster mentioned above). Rather, Carnap's mature account (for which, see his reply to Hempel in Schilpp [1963]) separates out the factual (i.e., synthetic) and conventional (i.e., analytic) components of a theory's laws and attributes a constitutive role only to the conventional component.

Pincock's version of the relative a priori differs importantly from all of the aforementioned authors' versions. According to the latter, constitutive principles serve the epistemic function they do by being essentially *semantic* in nature. That is, they create the possibility of confirming or disconfirming any derivative representations by endowing derivative representations with meaning: in our example, Newton's three laws tell us what "force" (or *F*, sitting on the left-hand side of the equation that is the law of gravitation) means. (Or better: the *analytic component* of Newton's three laws tells us what "force" means.) This, according to Pincock, is precisely where previous versions of the relative a priori go wrong. For the account of meaning required to sustain the meaning-giving nature of constitutive representations simply is not right. According to Pincock (sec. 6.2, 125), it will have the following two undesirable consequences: (1) meanings will be understood as giving rules of proper use of a term or set of terms, and (2) these rules will relate to how sentences using these terms may be supported (e.g., in certain inferences or by appeal to certain experiences). It is supposed that the problem with 1 is that it conflicts with the now-familiar lesson from semantic externalism: briefly, that competent language users need not be cognizant, even possibly cognizant, of the meanings of the words they use. It is supposed that the problem with 2 is that it makes meaning "too easy to achieve," especially in the case of purely mathematical terms; that is, it overlooks a "plausible requirement . . . that these rules must correspond to the genuine features of the things I wind up referring to using the new word" (sec. 6.2, 126).

We find neither of these objections to the traditional account compelling. Carnap himself answered the objection to 1 by appealing to rational reconstruction: it is only after a detailed philosophical inquiry that the rules that govern—or ought to govern—a term or set of terms is made explicit; there is no compulsion to demand detailed knowledge of these rules from the competent users of those terms. As for the objection to 2, first of all, it is not at all clear that the conventionalism issuing from a semantic understanding of constitutive representations makes meaning "too easy to achieve." Mature versions of this conventionalism concede that there are real constraints on meaning making, such as conservativeness over the antecedently accepted language and its rules (see, e.g., Przelecki 1969). Second, it is unclear what position Pincock can level this objection from that does not make constitutive representations answerable to experience in a way that collapses the whole derivative/constitutive distinction into familiar Quinean holism.

A further objection to the semantic account, specifically leveled against Friedman's version (in sec. 6.4), is that it makes the relation between constitutive and derivative representations implausibly strong. To take our example, Friedman's account entails that Newton's three laws must be true—or at least believed true—for the law of gravitation to even have a truth value. But this is surely wrong, Pincock says (sec. 6.4, 133), since we now seem perfectly happy to say that both Newton's three laws and the law of gravitation are false. At most, we need to understand Newton's three laws before we have a reason to endorse the law of gravitation.

Pincock is, we think, correct in this objection against Friedman, but its implications for the semantic account generally are not dramatic. If we combine the proviso above, that it is not Newton's three laws but merely their analytic component that constitutes meaning for the law of gravitation, with a strict demand (like Lewis's 1970) that the extension of a theoretical term (in this case, "force") be fully determined in all models, then we can wholeheartedly agree with Pincock that the laws need not be true—or even just believed true—in order for the law of gravitation to have a truth value. Indeed, on Lewis's account, the falsity of Newton's three laws entails the falsity of the law of gravitation, just as Pincock claims.

So much for Pincock's reasons for rejecting the semantic account. Let us turn to Pincock's positive, nonsemantic account. According to Pincock, the role that constitutive representations play in providing the possibility for us to have a reason to accept a derivative representation is not as meaning-determining constraints but as background beliefs. These background beliefs mediate the encounter between theoretical claims (the derivative representations) and the evidence. To take our example again, "on [Pincock's] purely epistemic proposal, the reason that an agent must believe [Newton's three laws] to confirm or disconfirm [the law of gravitation] is that it is only the conjunction of [Newton's three laws] with observations . . . that bear any evidential connection to [the law of gravitation]" (sec. 6.4, 135–36).

One can imagine these sentiments being expressed by Duhem or Quine. So how does Pincock's account differ from that of the confirmational holist? Pincock's account differs in retaining an a priori status for the constitutive representations. This has the effect of winnowing the vulnerabilities in our web of belief in the event of receiving recalcitrant evidence. Thus, recalcitrant evidence should not leave us in a three-way quandary (reject the evidence as unreliable, reject the derivative representation, or reject the constitutive representation), as the holist claims, but merely a two-way quandary (reject the evidence as unreliable or reject the derivative representation). In Bayesian terms, constitutive frameworks (like Newton's three laws) are artificially (i.e., independent of experience) afforded a high degree of confirmation; this then makes possible an estimation, at the very least, of the degree of confirmation that a given item of evidence (planetary trajectories, say) will confer onto a given derivative representation (like the law of gravitation).

However, the nature of the a priori status that Pincock attributes to constitutive representations must be rather subtle. For one thing, the derivative/constitutive distinction is intended to be a relative one, and it is far from obvious how apriority could be a matter of degree, as it would then have to be. Furthermore, Pincock accepts that constitutive frameworks are eventually rejected under the weight of continual disconfirmation of all of their derivative representations. Pincock must accommodate this without making the apparently evidence-independent initial acceptance of a constitutive framework anything more than a methodological necessity—something that the confirmational holist could surely also sign up to.

Pincock's attempt to square this circle finally involves an appeal to the distinction between pure and applied mathematics. Only mathematical frameworks under a particular physical understanding may be rejected under the growing weight of recalcitrant evidence; the pure mathematics divorced from any physical understanding whatever may then be safely afforded an a priori status. The problem for Pincock is that this move makes apriority an essential feature not of constitutive representations but rather of the pure mathematics that may or may not form their part. Consequently, he is left with no means—beyond those available to the holist—with which to articulate a nonsemantic version of the derivative/constitutive distinction.

That there is any space at all between Pincock's account and the holist's depends on his attributing apriority to pure mathematics. But an argument for the a priori status of pure mathematics is not to be found in chapter 6. The reader must wait until chapter 10 for an explicit argument for Pincock's position and against the Quinean holist.

Discussion of Part 2. In the second part of the book Pincock turns away from explicit discussion of the application and uses of mathematics and toward the philosophical consequences of these applications, most specifically toward the indispensability argument. Pincock does not think that mathematics is unreasonably effective; indeed, much of part 1, and particularly chapter 7 on failed applications, can be seen as defending the reasonableness of the effectiveness of mathematics. He does think that mathematics is indispensable but denies that we can draw metaphysical conclusions from this indispensability.

The book is a very welcome part of a "new" philosophy of mathematics, one that focuses on the details of practice and application, rather than on numbers and axioms. But readers whose primary interest is in the epistemology and metaphysics of mathematics may leave slightly disappointed. One source of disappointment may be the relative weakness of Pincock's conclusions. As we will see below, in terms of the metaphysics of mathematics, the book seeks only to rule out fictionalism. Moreover, Pincock offers only a tentative solution to the problem of finding an epistemology that renders mathematics a priori.

But by our lights the modesty of the book is one of its virtues, as well as a perhaps inevitable consequence of a more detailed and honest look at applied mathematics. More frustrating is a certain lack of cohesion of argument; the parts of the book are individually interesting but fall just short of forming a whole that is more than the sum of its parts. As far as we can ascertain, the admirably detailed discussions of part 1 do not do much work in the actual argumentation of part 2 on the indispensability argument.

The recent literature on the indispensability argument has, through the work of Colyvan, Baker, Pincock, and others, come to be increasingly informed by detailed case studies as well as by argument as to what scientific explanation requires when the explanans is mathematical in character. Prefaced by an overview in chapter 9 of the contemporary debate on the indispensability argument, chapter 10 contains Pincock's arguments that the "explanatory role" version of the indispensability argument is question-begging (sec. 10.2, 211). One argument goes through a sensitivity requirement, while the other goes through a claim that understanding requires belief for the math deployed in scientific explanations.

What does the explanatory role version of the indispensability argument say? In short, it says that one ought to believe in the truth of a certain mathematical claim when one knows that it plays an indispensable explanatory role in science (sec. 10.1, 207). The notion of indispensability of a mathematical claim in a scientific explanation is understood as follows: all the other competing explanations that lack the claim are inferior qua explanation (sec. 10.1, 205). Pincock then argues that one knows that mathematical claims play an indispensable explanatory role in science only if one already knows several mathematical claims.

Pincock's argument proceeds by way of an endorsement of what he calls the sensitivity requirement (sec. 10.2, 214). This requirement mandates not only that the explanations be indispensable but that their "explanatory contribution tell against some relevant alternative epistemic possibilities" (sec. 10.2, 214). It is not hard to see that it will be very difficult for mathematical explanations of observed phenomena to meet this sensitivity requirement. For, suppose the mathematical claim in question is a description of some infinite structure. Consider an alternative rival claim that says that this structure is finite but sufficiently large to deliver the same observations (cf. sec. 10.2, 214). While incompatible, it seems that neither of these alternative claims indicates that the other is a poorer explanation. Hence, the sensitivity requirement is not met.

It is not really obvious, however, that this kind of example is one in which the mathematical claims were indispensable in the relevant sense. For, as said above, a mathematical claim is indispensable to an explanation of some observed phenomena if, when one looks about at the other competing explanations that lack the claim, these are inferior qua explanation (again sec. 10.1, 205). If our two competing explanations pertain to the claim about the infinite mathematical structure and its finitistic rival, then it seems that these claims are not indispensable in this sense for the same reason that the explanations failed to meet the sensitivity requirement. If this is right, then it is not clear how examples such as these could be relevant to the evaluation of the explanatory indispensability argument in the first place. More generally, it is difficult to see how the sensitivity requirement on explanations is different from the indispensability requirement.

In this connection, it is perhaps useful to compare Pincock's argument at this specific juncture to Sober's well-known "Mathematics and Indispensability." Sober has often stressed the signature importance of the likelihood principle. This principle recommends assent in a hypothesis on the basis of an observation only if that hypothesis better predicts the observation than some rival hypothesis. The notion of prediction is usually rendered in probabilistic language as follows: hypothesis h predicts observation e to degree d if and only if $P(e|h) = d$.

Sober rejected the indispensability argument because he thought that (i) there were not any serious rival mathematical hypotheses, and (ii) if there were "could they be said to confer probabilities on observations that differ from the probabilities entailed by the propositions of arithmetic themselves?" (Sober 1993, 46). So one might see Pincock's sensitivity requirement as a nonprobabilistic analogue of Sober's likelihood principle. But a salient difference is that Sober suggests identifying the indispensability of an explanation with its rendering the hypotheses more likely than competing hypotheses (38), while Pincock views the sensitivity requirement as different in character from the indispensability requirement.

The way that sensitivity is related to the charge of question-begging seems to reside in the thought that the only way one is going to meet the sensitivity requirement is to presume that there is only one competing explanation. However, at the advent of the subsequent section (sec. 10.3, 217ff.), Pincock offers a separate line of argumentation for the question-begging charge. In particular, he argues that in order to understand how mathematics is employed in explanations of observed phenomena, one already has to believe to a high degree some of the crucial claims deployed in this mathematics.

One way to define analyticity is in terms of understanding-belief links (cf. Williamson 2007, 74). So Pincock might be viewed here as suggesting that some rarefied analyticity claim supports the contention that the explanatory-role indispensability argument is question-begging: the only agents who would understand how mathematics is used to explain observations would already be agents who believed in the relevant mathematics in the first place.

However, sometimes analyticity is also cast in terms of understanding-justification links (cf. Williamson 2007, 77). So one might suggest that the charge of question-begging could be lessened if one conceded that one needed to believe the math in order to see how it was used in explaining the observed phenomena but held fast to the thought that seeing this might give one a new reason for this belief. Pincock's rejection of this suggestion is tied to his rejection of confirmational holism. In particular, Pincock discusses and rejects a holist view according to which one provisionally accepts the mathematics and then allows it to inherit justification from the evidence for the model it is used in (sec. 10.3, 218–20).

His argument against this kind of holist view is basically that it goes against scientific practice—we do not in fact question the mathematics when the model is placed in question (sec. 10.3, 219). Furthermore, if we allow the distinction between constitutive and derivative representations, then scientific practice looks even more at odds with general principles of holism—in chapter 7, he takes himself to have discussed cases in which constitutive representations fail. In such cases, we easily identify the culprit, but if the confirmational holist were correct, we would consider rejecting the mathematics.

This argument, despite occupying a mere three pages (sec. 10.3, 217–20), is essential to the aims of the book. It forms, so far as we can see, the only positive support for the claim that mathematics is a priori. But it is far from clear that it can support the weight that rests on it; the holist has never denied that some beliefs are far less likely to be abandoned than others. Pincock dismisses talk of the centrality of mathematics to our web of belief as mere metaphor (sec. 10.3, 219), but there is surely more to it than this; in part, the metaphor is intended to convey the high degree of confirmation that accrues to beliefs that play a pivotal role in varied well-confirmed applications. One might see Pincock's own documenting of the many successful applications of mathematics precisely as defending this central role for mathematics.

But Pincock's statement of the case for understanding-belief links is of some independent interest. The idea is that an explanation of observed phenomena in science goes through building models of the phenomena, along with certain adequacy conditions that articulate when the model or structure accurately models the phenomena. This whole setup is conveniently abbreviated as a "representation" (cf. sec. 2.1, 26). Pincock's key claim here is, "So, for an agent to *understand* this sort of representation, he or she must *believe* that the claims describing this structure are true" (sec. 10.3, 217; cf. sec. 12.1, 243). So when one has a mathematical explanation of observed phenomena, the model in question will contain some part like the real numbers or natural numbers or some other mathematical structure. In this case, Pincock's key claim is that to understand the model, one needs to believe at least some basic claims about the mathematical structure.

In our view, Pincock does not provide any direct evidence for this key claim. However, he recognizes that fictionalists of all stripes will deny it. As one important fictionalist once put the motivating idea, "to fully understand a model one must see 'where' the sustaining positive analogy runs out" (Hodes 1984, 126). So indirect evidence might accrue for Pincock's key claim by virtue of his critique of fictionalism. This critique revolves around the "export challenge" for fictionalism: roughly, if the fictionalist applies math to the sciences by working with an augmentation of the physical systems by various kinds of mathematical abstracta, it is incumbent on the fictionalist to give some precise indication of what properties of the genuine physical system can be read off its mathematical augmentation (sec. 12.3, 252). Pincock is skeptical that there is really any "set of rules that are detailed enough" to solve the export challenge. Roughly, the concern is that the mathematical augmentation might include an entity like "caloric" or "mental substance," and it might be difficult to decide whether these should be attributed to the physical system or its mathematical augmentation (sec. 12.3, 254).

But it seems to us that there is a potential tension between Pincock's critique of fictionalism in chapter 12 and his critique of confirmational holism in chapter 10. With respect to holism, Pincock adverted to scientific practice's tendency to leave the mathematics fixed and thus not genuinely subject to disconfirmation. One wonders why the fictionalist could not similarly demur from specifying a "set of rules" to solve the export problem but note that the practice of applying mathematics is a fairly reliable guide to what may be permissibly exported from the various mathematical augmentations that have proved useful.

Finally, it is worth underscoring that Pincock's position vis-à-vis fictionalism is distinctive and new. While traditionally the defender of the indispensability argument has sought to argue that fictionalism is inviable, Pincock argues for the inviability of fictionalism with the aim in mind of buttressing support for his charge that the indispensability argument is question-begging. Likewise, while both proponents of the indispensability argument and proponents of fictionalism are united in a presumption that appeals to apriority and analyticity are unsuitable means by which to solve the deep problems in philosophy of mathematics, Pincock's considered position is the study of the applicability of mathematics in science is best understood by appeal to some measure of apriority and analyticity.

REFERENCES

Friedman, Milton. 2001. *The Dynamics of Reason*. Stanford, CA: CSLI.

Hodes, Harold. 1984. "Logicism and the Ontological Commitments of Arithmetic." *Journal of Philosophy* 81 (3): 123–49.

Kuhn, Thomas. 1962. *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.

Lewis, David. 1970. "How to Define Theoretical Terms." *Journal of Philosophy* 67:427–46.

Przelecki, Marian. 1969. *The Logic of Empirical Theories*. New York: Routledge & Kegan Paul.

Schilpp, Paul Arthur, ed. 1963. *The Library of Living Philosophers*. Vol. 11, *The Philosophy of Rudolf Carnap*. La Salle, IL: Open Court.

Sober, Elliott. 1993. "Mathematics and Indispensability." *Philosophical Review* 102 (1): 35–57.

Williamson, Timothy. 2007. *The Philosophy of Philosophy*. Malden, MA: Blackwell.