REVIEW

Metaphysics and Science

Stephen Mumford and Matthew Tugby

Oxford: Oxford University Press, 2013, £40 (hardback), 256 pp.
978-0-19-967452-7

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This is the penultimate version of a review forthcoming in the BJPS. Please only cite the final version.

Despite what was suggested (to me at least) by the title of this book, *Metaphysics and Science* is not a set of essays reflecting on the relationship between metaphysics and science. Rather, it is a set of essays on specific topics in the metaphysics of science, selected on the basis of Mumford and Tugby's proposed definition of what the latter is. Given the famed resistance of either metaphysics or science to definition individually it is certainly ambitious of the editors to try to define, in a brief introduction, the 'metaphysics of science', and I myself think that their attempt at demarcation fails. Partly as a result of this, and while many of the essays are individually rewarding, I was not fully convinced that they should have been gathered together instead of left to suffer a more scattered existence among the various journals. Since it is Mumford and Tugby's definition that is supposed to give shape to the book, my critical focus will initially be on it. From there, I shall summarize the four main sections of the volume before closing with a few remarks on the whole.

Mumford and Tugby's conception of the metaphysics of science, and thus their vision for the volume, is guided by their insistence that if there is one thing that metaphysics of science is *not* then it is theorizing about concepts that are specific to particular scientific disciplines. Thus they say that in contrast to philosophers of physics engaged in debating the viability of absolute space – a debate which concerns "specifically concepts developed within the discipline of physics" – metaphysicians of science are concerned with "concepts which are deployed in *all* the natural sciences, including the special sciences'' (p. 6). This emphasis on generality motivates their definition of the metaphysics of science as a discipline concerned with (1) whatever it is "that make[s] scientific enquiry as we know it possible", and (2) the "metaphysical relationship between the various scientific disciplines**"** (p. 14). The rationale for the first set of issues is, as they note, strongly Kantian in flavour, and since they take it that science – whatever else it may be concerned with – is in the business of furnishing predictions and explanations, it follows that the metaphysics of science concerns "those aspects of reality… which impose order on the world"; for "in a

chaotic, disorderly world, it would not be possible to make the kinds of predictions (not to mention *novel* predictions) or construct the kinds of explanations that we find in natural science" (pp. 11-12). Further, since it is the modal concepts of "kindhood, lawhood, causation and causal power" that have this feature, it is the study of these concepts that constitutes "the heart of the metaphysics of science" (p. 13). The motivation for the second set of issues, those concerning the relations between the disciplines, is that "scientists tend to specialize in their own branches of science… Even if scientists are interested in these broader questions about how the branches of science relate… it is beyond their remit to think about them" (p. 14). In contrast, "as philosophers, metaphysicians of science are well positioned to do this."[1]

While I doubt there is a single philosopher of science who would deny that modality and the relations between theories rank among its crucial questions, I myself found the editors' rationale for restricting the subject matter of the metaphysics of science to these questions somewhat unconvincing. I think, in fact, that we can take issue with their justification of the claim that modality is necessarily central, the idea that metaphysicians of science are well-placed to talk about the inter-theory relations in general, and even the more foundational claim that scientific metaphysics should be aiming at generality in the first place.

Let's start with the idea that the study of modal concepts such as laws, dispositions and causes, understood in sense sufficiently thick to "bring order to the world", occupies the centre ground in scientific metaphysics on the grounds that these are needed for there to be order and thus science at all. The criticism here is obvious, and it is that while they are right that science – which is in some minimal sense just the study of regularities – trivially has as a requirement the *existence* of regularity, it does not follow that anything needs to *bring that regularity about* in order for regularity to exist. However, by their criterion world-views in which nothing *does* bring it about will trivially fail to qualify as scientific metaphysics. But of course, a basic fault line in the discipline lies between Humeans and non-Humeans, and thus between positions that are opposed on precisely this issue; adopting a definition of the metaphysics of science according to which what was one of its chief debates is no longer a matter of dispute *within metaphysics of science* strikes me as an unsuccessful demarcation.[2] Nor is it by any means the case that all those who deny that substantive modal notions are necessary for science are simply unwilling to engage in metaphysics: there are after all plenty of realist Humeans. It seems to me that nothing fancier need be said to justify the importance of laws in the discipline than that they are ubiquitous in science, enigmatic, and richly rewarding philosophically, but of course there is nothing in that justification that prohibits non-modal notions from being regarded in the same way.

As noted, however, Mumford and Tugby do not think that the metaphysics of science is in the business of studying only modal concepts: its other strand of enquiry concerns "the metaphysical relationships between the various scientific disciplines". As noted also, the motivation for this second strand differs from that for the first: it is after all far from obvious

[1] I note that Ladyman and Ross (2007, section 1.5) defend a similar claim concerning the proper role of metaphysicians of science.

[2] There is also of course room for a position in which regularities *are* taken to need accounting for but not in terms of the thick modal notions the editors cite; Wheeler's speculation of 'law without law' is one such (Wheeler (1983)).

that metaphysical relations between the different fields are needed in order for science to exist, since the justification for categorizing them all as 'sciences' typically appeals to their shared *methodologies* instead of anything to do with metaphysics. We have seen that the reason offered for including this topic is that it is an issue that metaphysicians of science, as philosophers, are better placed to theorize about than scientists. But this is a puzzling justification. One would hope that philosophers would fare better than scientists at theorizing about pretty much *any* issue relating to science that we are apt to classify as philosophical, so this justification presumably warrants the inclusion of much more than just metaphysical relations between fields; further, since modal issues would clearly rank among these issues, is there still any need to dust off our Kant? However, while if sound their justification for including this topic would seem to justify many more, I myself am sceptical that metaphysicians of science *are* in general more able than scientists to talk about these relationships. It is after all said that Hilbert was the last mathematician to understand the whole of mathematics; it seems rather ambitious to think that contemporary philosophers can hope to be sufficiently proficient across all fields to talk about the relations between them, at least in a way that is supposed to compensate for the preoccupations of scientists.

This brings me to the idea that metaphysics of science should be aiming, definitionally, at generality in the first place. To see my thinking here, let's take it as read that the study of scientific ontology is a central component of scientific metaphysics (regardless of its status according to Mumford and Tugby's criterion). Is it so clear that there is work to be done on 'scientific ontology' *in general* – that is, work that is not simply the conjunction of work on ontology in molecular biology, dynamic systems theory, social psychology, or quantum physics – at least if it is to transcend insipid *a priori* debates on the nature of substance and property? I myself do not think this is obvious. Indeed, John Worrall's memorable remark that the ether and the classical electromagnetic field are more like chalk and cheese than chalk and cheese themselves should make us hesitant about the idea that there is even useful work of general scope to be done concerning ontology just in *physics* (and I haven't even mentioned anything quantum-mechanical yet).[3] This general point may moreover carry over to the volume's central topic, namely laws and related topics. Consider the fact that laws in quantum field theory are subject to such demanding logico-mathematical consistency constraints that one may argue they are necessary without invoking any primitively modal notions.[4] As such, it seems one can claim that fundamental regimes can tolerate a view at once Humean and necessitarian – suggesting in turn that that the modal landscape can look very different between different scientific domains. Is it therefore safe to assume that there is some general modal metaphysics of science that works across the board, as opposed to a broad set of questions of interest to metaphysicians in general, each needing different treatments in different contexts?

It will be clear, then, that I was not compelled by the justification for grouping papers on these particular topics together in a volume purporting to represent 'Metaphysics and Science'. That said, the topics covered are indisputably major themes in the discipline, and the poor framing of the concept behind the book needn't compromise the quality of the essays themselves –indeed several of them *are* very good. Let me therefore move away

[3] Worrall (1989), p. 107-8.
[4] I at least try to argue this in McKenzie (2014).

from Mumford and Tugby's own written contribution to focus now on the book's four main parts.

Part 1 of the book concerns laws, and contains papers by John Roberts, Jim Woodward and Marc Lange. Roberts' paper makes the radical proposal that what explains the counterfactual resilience of laws is the counterfactual resilience of measurement methods. Woodward's essay looks closely at the kind of reasoning scientists actually deploy to infer laws and causal claims, arguing that that – despite its centrality to the Lewisian 'best system' analysis – the idea that this involves the weighing up of virtues such as simplicity and strength is woefully inadequate. Rather, intervention and invariance principles are deployed, and the modal content of these principles blocks any obvious route to modal reduction. In the third contribution, Lange uses the framework developed in his *Laws and Lawmakers* to articulate the claim, made by Einstein and many since, that Lorentz symmetry constitutes a 'constraint' on force laws – clearly an interesting project, since this necessity allegedly stronger than nomic but weaker than logical is far from familiar. Of these three essays, Woodward's struck me as elegant and compelling, and the relevance of it to the broader debate over laws was clear in spades. Lange's paper also clearly engaged with issues of interest to metaphysicians of physics and possibly also metaphysicians interested in notions of grounding (though prior familiarity with Lange's elegant but intricate system is probably required to get to grips with the paper fully). Roberts' paper, on the other hand, was more of a struggle to connect up with the big issues in the debate, but that might reflect more a lack of patience on my part than anything about his argument (I did find it rather a dense read). I certainly can't say that I figured out while I was reading it how to block its radical-looking conclusion, so no doubt there is something useful to be mined in there.

Section 2 of the book is on the subject of dispositions and causes. In the first of its two chapters, Andreas Hüttemann gives an overview of his theory of causation based on dispositions. Being, I take it, a condensed version of his book on the topic this is probably the most panoramic essay in the volume. After arguing from considerations of composite systems' behaviour that dispositions are needed in physics, he develops a theory according to which causes are identified with that which diverts systems from their 'default' evolution. The resulting picture grants the notion of cause its distinctive interest-relativity while also letting it be something objective; it also allows for causes to be manifest in all domains instead of being confined within physics. Though I got off the bus when he invoked the 'essences of composite systems' in explaining the origins of causal modality, and though I felt that something cried out to be said about how 'vegetarian' the dispositions he attributed to composite systems appeared to be (to my mind at least), I found that his orientation of issues from Russell and Mackie within a dynamical framework an expansive and stimulating read. His essay is followed by Jennifer McKitrick's meditation on what it takes to activate a power, where she holds that the need for powers to be activated if they are to do any work plagues the dispositional monist with a nefarious regress. While she references Hüttemann's essay – making for a rare moment of cohesiveness within the volume – her focus on such dispositions as fragility and flammability did rather seem to extricate us from the scientific context Hüttemann set up and place us back in the 'world as pinball machine' that is the proclivity of analytic philosophers. Thus while McKitrick's paper is sustained and meticulous, and recommended for the dedicated student, my tastes are such that I got rather less excited about it than the preceding piece.

Section 3 of the book concerns natural kinds, and contains essays by Helen Beebee, Emma Tobin, and L.A. Paul. Beebee's essay argues against the idea that it follows from the Kripke-Putnam thesis that there are necessary *a posteriori* truths that strong essentialist views about natural kinds are thereby justified: in particular, she denies that it follows that natural kinds cannot 'cross-cut' or that they are incompatible with either Kuhnian relativism or species pluralism. While I must say that she failed to convince me that anyone actually holds that the cited strong essentialist views really follow from Kripke-Putnam semantics (since the quotations she provided in support of that view did not strike me as having those implications), her paper brought some big themes into contact in a way that left me feeling better oriented within the debate as a whole. Tobin's essay concerns whether, in addition to natural properties, we need a notion of natural kinds, and though I started out assuming that the key question here was whether a primitive notion of property 'grouping' is needed in addition to that grouped, I fear that I actually felt less sure after 18 pages as to whether that was right or wrong. Suffice to say that, although it brought interesting new issues to the table, this chapter didn't help me to order to my thoughts on the issue. Paul's essay took us back to Putnam, and here she argues that the attempt to block Putnam's Paradox by invoking natural kinds *á la* Lewis only works if we assume that the world lacks kinds "which would allow for permutations involving the predicates of current theory" (p. 195). But while the conclusion she draws from this is that scientific realism will only be well-justified if we "reject the idea that the world has deep or fundamental unbreakable symmetries" (*ibid*.) – something that should seem very threatening given the importance of symmetry concepts in fundamental physics – I couldn't help but feel rather unmoved by the alleged catastrophe that she visits upon us. The reason is that we have (in a sense) seen this play out in real life, but both the reaction to and the consequences of it could hardly have been more different. For example, the data on binding energies for isobaric nuclei (nuclei with the same number of nucleons), together with the near-equal masses of neutron and proton, suggested that these two natural kinds were very much interchangeable from the point of view of nuclear theory. However, rather than infer a disastrous referential failure, Heisenberg postulated that as far as the nuclear interactions were concerned we were not dealing with two natural kinds at all but rather one kind able to take on two states that were equivalent from the point of view of the dynamics – a move that paved the way for the symmetries revolution in particle physics and everything that has been entailed by that. Now, I am not here saying that physical theories with significant symmetries cannot in principle be plagued by referential problems. But what I am saying is that the relevant issues here *cannot even be posed* if we continue to imagine, as Paul and seemingly virtually everyone in this debate does, that theories of physics are framed in terms of predicate logic and not natural mathematics. For me, the expressive inadequacy of the former in this case only underlines that we often need to engage with the real languages of science to do real philosophy of science.[5]

Part 4 is on emergence. Unlike the other sections, the topic here does not concern "phenomena which bring order to the world" but rather (a specific concept relevant to) the relations between the sciences. As such, the one paper in this section does rather stick out.

[5] For further discussion of how wedding debates about realism in physics to predicate logic can obscure or distort the relevant issues, see Saunders and McKenzie (2014).

Nevertheless, Jessica Wilson's cross-section of historical and contemporary work on emergence, plus the defence of her own view, works as a stimulating stand-alone piece (if challenging at times in its presentation). In it, Wilson lays out her account of ontological emergence, according to which a composite system can be claimed to be "ontologically autonomous" from the composing entities on which it depends when the degrees of freedom needed to characterize it are fewer in number than those needed to characterize its components when relatively unconstrained: by Leibniz's law, the former must then be ontologically distinct from the latter and as such, she claims, may be regarded as emergent in a robust ontological sense (p. 226). But while there is much to ruminate on in this essay, I was a little unconvinced that mere distinctness according to Leibniz's law was sufficient for real "ontological autonomy" (one need only recall the dreaded statue-lump cases to generate that suspicion). Furthermore, I was unconvinced that all the examples of "elimination" of degrees of freedom she provides really did involve elimination in an ontological sense. She mentions, for example, how the degrees of freedom required to characterize condensed matter systems at criticality are greatly reduced relative to the number required to characterize their composing systems. But it strikes me that to say that fewer degrees of freedom are "needed to characterize" a system is a different from saying that they have been "eliminated": indeed, it seems to me that in the condensed matter case the degrees of freedom are still present but simply *unimportant for all practical purposes*. If that is right, then her account may not after all be so different from the "straightforwardly epistemological**"** (p. 214) accounts she takes hers to be contrasted with. I note, however, that not all of her examples suffered from this problem: in particular, the classical charged sphere seemed to work much better for her purposes – something somewhat ironic, given its relative mundanity in comparison with the systems typically discussed in the context of emergence.[6]

With a roster of contributors like that Mumford and Tugby have assembled, there couldn't fail to be something worth taking away from this book. The quality of most of the individual papers notwithstanding, however, the sections of the book are too slight to be able to give representative outlines of the state of play in the relevant fields, and nor (by and large) do the papers 'talk' to one another in a way that really warrants having them gathered together. I should say too that it is a little ragged round the edges with respect to copyediting, and some of these errors were important (I could not, for example, make sense of the equation on page 206, and elsewhere the word 'not' seemed to have disappeared from a sentence or two). Thus if there is a paper in here on a topic that interests you, by all means go ahead and read it; but I would shy away from saying that this was a book that represents more than the sum of its parts.

References

[6] Thanks to James Fraser for pointing out that these cases are arguably different.

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