Brit. J. Phil. Sci. **69** (2018), 117–137

Minimal Models and the Generalized Ontic Conception of Scientific Explanation

Mark Povich

ABSTRACT

Batterman and Rice ([2014]) argue that minimal models possess explanatory power that cannot be captured by what they call 'common features' approaches to explanation. Minimal models are explanatory, according to Batterman and Rice, not in virtue of accurately representing relevant features, but in virtue of answering three questions that provide a 'story about why large classes of features are irrelevant to the explanandum phenomenon' ([2014], p. 356). In this article, I argue, first, that a method (the renormalization group) they propose to answer the three questions cannot answer them, at least not by itself. Second, I argue that answers to the three questions are unnecessary to account for the explanatoriness of their minimal models. Finally, I argue that a common features account, what I call the 'generalized ontic conception of explanation', can capture the explanatoriness of minimal models.

1 Introduction

While acknowledging the widespread use of causal explanation in science, a number of prominent philosophers of science have recently begun exploring its limits (see Batterman [2002a], [2002b]; Huneman [2010]; Rice [2012], [2015]; Woodward [2013]). Batterman has been investigating the ways in which neglect of causes contributes to explanatory power in physics, particularly in statistical mechanics. Rice has been engaged in similar investigations of the neglect of causes in optimality modelling in biology. Recently, Batterman and

doi:10.1093/bjps/axw019

Advance Access published on July 29, 2016

Rice ([2014]) have combined their efforts in an articulation of their common project. Their work brings important and successful modelling techniques to bear on the philosophy of scientific explanation. Nevertheless, there are significant limitations to their project. It is my aim here to spell out these limitations and provide an alternative proposal.

Batterman and Rice focus on minimal models, which are 'used to explain patterns of macroscopic behaviour across systems that are heterogeneous at smaller scales' ([2014], p. 349). This widespread class of models, they argue, has explanatory power that cannot be captured by what they call 'common features' approaches to explanation. According to common features approaches, (i) explanations accurately represent all and only the features relevant to their explananda,[1] and (ii) the explanatoriness of a representation consists in its representing relevant features ([2014], p. 351).[2] Common features approaches include not only mechanistic approaches (Craver [2006]; Glennan [2002]; Kaplan [2011]) and causal and difference-making approaches (Salmon [1984], [1989]; Strevens [2008]; Woodward [2003]), but also Pincock's ([2012]) structuralist or mapping account, which explicates the explanatory role of mathematics in terms of its ability to mirror certain ontic structures. Any philosophical theory of explanation according to which accurate representation is responsible for explanatory power is a common features approach, whether or not the features represented are causes (Batterman and Rice [2014], p. 351).

Batterman and Rice argue that common features approaches fail to capture the explanatoriness of minimal models because even when a minimal model is minimally accurate, it is not its accuracy that accounts for its explanatoriness. Rather, minimal models are explanatory in virtue of 'there being a story about why large classes of features are irrelevant to the explanandum phenomenon' ([2014], p. 356).

In this article, I argue for a negative and a positive thesis. My negative thesis is that Batterman and Rice's account of the explanatoriness of minimal models fails. They require that three questions be answered in order to provide the above-mentioned story about why large classes of features are irrelevant. I will henceforth refer to these as the 'three questions':

> Q1. Why are these common features necessary for the phenomenon to occur?

[1] Depending on the explanatory representation used, some irrelevant features must be represented. For example, if our explanatory representation is pictorial, it must be coloured some way, even if colour is not relevant to the explanandum phenomenon. Ideally, the modeller will flag any potential confusions. See (Weisberg [2013], Section 3.3) for a related discussion of the role of modellers' intentions in determining what Weisberg calls 'representational fidelity criteria': standards for evaluating a model's representational accuracy.

[2] (i) is not just a restatement of (ii). One could hold that accurate representation is necessary but not sufficient for explanation. This appears to be close to Batterman and Rice's view ([2014], pp. 351, 356).

> Q2. Why are the remaining heterogeneous details (those left out of or misrepresented by the model) irrelevant for the occurrence of the phenomenon?
>
> Q3. Why do very different [fluids and populations] have features [. . .] in common? (Batterman and Rice [2014], p. 361)[3]

My negative thesis consists of two parts. First, the method they propose to answer the three questions is unable to answer them, at least by itself. Second, answers to the three questions are unnecessary to account for the explanatoriness of minimal models. I argue for this second claim in two ways. First, I draw an analogy between their strategy and a similar strategy in a more commonplace case of multiple realizability. In the case I present, it is evident that answering analogues of the three questions is unnecessary to explain multiple realizability. Second, I argue that if answers to the three questions were necessary, a regress would loom. Batterman and Rice need to explain why, if the three questions are necessary, we should stop asking where they say we should. Of course, according to Batterman and Rice, the three questions are not further questions, in addition to the question of what makes minimal models explanatory; the three questions just are those that need to be answered in order to account for the explanatoriness of minimal models. My analogy is intended to show that that is not the case.

My positive thesis is that a common features approach can account for the explanatoriness of minimal models.[4] Batterman and Rice are (probably[5]) right that mechanistic and difference-making accounts cannot do the job, but an account much like the one proposed by Bokulich ([2011]), Rice himself ([2015]), and Saatsi and Pexton ([2013]) can. They follow Woodward ([2003]) in requiring that an explanation represent counterfactual dependence relations between the explanandum phenomenon and the features on which it depends, but they drop the requirement that these counterfactual dependence relations be construed causally. The reason for this is that the counterfactual dependence relations represented by some models, such as Batterman and Rice's minimal models, cannot very plausibly be given a causal interpretation.

On this view, explanatory power consists in the ability to answer what-if-things-had-been-different questions ('w-questions'). I argue that this requires commitment to an ontic conception of scientific explanation (Salmon [1984])

[3] I have slightly altered the wording of the third question from the original in order to capture both models, thereby avoiding unnecessary repetition.

[4] While I was finishing this manuscript, Lange ([2015]) also made this point, although he does not develop the positive proposal I do. He also made an objection to Batterman and Rice similar to one of mine about regress. These and any other commonalities were arrived at independently.

[5] It is somewhat plausible that at least some of the common features in Batterman and Rice's minimal models can be given a causal interpretation. On the account proposed here, though, this is not what makes these features explanatory. I briefly expand on this at the end of Section 4.

and that philosophers of science have been mistaken in equating the ontic conception with the causal–mechanical account of explanation. As we will see, Salmon seems not to have equated them.

My proposal is consistent with many things Batterman and Rice have themselves written in the past.[6] It seems that their desire to avoid anything like a common features approach has driven them too far, apparently past things they have said before. In the present atmosphere in philosophy of science, it is a significant enough achievement to have brought to philosophical focus important modelling methods in physics and biology that emphasize the systematic neglect of causal detail. Batterman and Rice have rightly stressed the importance of this neglect, but this importance need not drastically change our account of scientific explanation.

The rest of the article is organized as follows: In Section 2, I present the minimal models whose explanatoriness Batterman and Rice argue cannot be accounted for by a common features approach. These are the lattice gas automaton (LGA) model of fluid dynamics and Fisher's model of one-to-one sex ratios. In Section 3, I present and critique Batterman and Rice's account of the explanatoriness of these minimal models. According to Batterman and Rice, any such account must answer the three questions, and answers are provided by the renormalization group (RG) and universality classes.[7] I argue that the three questions cannot in fact be answered by RG alone. I then argue that regardless of whether RG answers the three questions, they do not need to be answered in order to give an account of the explanatoriness of LGA and Fisher's model. I give two arguments for this. First, I show that answers to analogues of the three questions are unnecessary in an analogous case of multiple realizability. Batterman ([2000]) has argued that RG explains multiple realizability generally, so I take it that my analogy is apt and generalizable to Batterman and Rice's models. Second, I argue that if answering the three questions were necessary for an account of the explanatoriness of Batterman and Rice's minimal models, a regress would loom.

In Section 4, I provide my own common features account of the explanatoriness of Batterman and Rice's minimal models: the generalized ontic conception. I argue that they are explanatory because they accurately represent the relevant dependence relations, that is, the objective features of the world on which the explanandum phenomenon counterfactually depends. My account is an ontic conception, in Craver's ([2014]) sense (to be explained more fully below). I argue, for reasons different than Wright's ([2012]), that it is a

[6] For examples, see Footnote 24 for Batterman's remarks on pain below, and Rice ([2015], p. 20): 'in some cases counterfactual information can be explanatory without tracking any relationships of causal dependence'.

[7] Of course, in biological contexts some mathematical method(s) other than RG must be employed, though Batterman and Rice are silent on what these methods might be.

mistake to equate the ontic conception of scientific explanation with the causal–mechanical account of explanation (Craver ([2014]) gestures at this idea in his defence of the ontic conception). A viable general theory of scientific explanation can be constructed by combining insights from Salmon ([1984], [1989]) and Woodward ([2003]), while realizing that there are non-causal kinds of ontic dependence.

Nevertheless, I do briefly consider the idea that some of the dependence relations in Batterman and Rice's minimal models can be given a causal interpretation. I do this simply because I do not think a causal interpretation is as obviously wrong as Batterman and Rice imply. A causal interpretation is more plausible for some common features than others, though I do not commit myself here to a causal interpretation of any of them.

On my account, RG plays a central role in discovering explanatorily relevant features and demonstrating that they are relevant (Section 3 shows how). This means that RG is not a kind of explanation distinct from common features explanation, but an essential method scientists use to construct common features explanations.

2 Batterman and Rice's Minimal Models

Batterman and Rice present two minimal models whose explanatoriness they argue cannot be captured by a common features approach. These are the LGA model of fluid dynamics and Fisher's optimality model of one-to-one sex ratios.

LGA accurately predicts macroscopic fluid behaviour that is described by the Navier–Stokes equations ('Navier–Stokes behaviour', for short). The model consists of a hexagonal lattice on which each particle has a lattice position and one of six directions of motion (momentum vectors). Each particle moves one step in its direction of motion and if some collide, so that their total momentum adds to zero, then those particles' directions of motion rotate 60°. With thousands of particles and steps, and some smoothing out of the data, an overall pattern of motion emerges that is incredibly similar to real fluid motion (Goldenfeld and Kadanoff [1999], p. 87).

The second model presented by Batterman and Rice is Fisher's model of the one-to-one sex ratio. The biological question that Fisher's ([1930], pp. 141–3) model was designed to answer is why population sex ratios are often one-to-one. Hamilton ([1967], p. 477) provides a succinct summary of Fisher's argument: If males are less common than females in a population, then a newborn male has better mating prospects than a newborn female. In this situation, parents genetically disposed to have male offspring will tend to have more than the average number of grandchildren. This will cause the genes for the tendency to have male offspring to spread. As male births become more common and a one-to-one sex ratio nears, the advantage of the tendency to produce males disappears. Since the same reasoning holds if females are the more common sex, one-to-one is the equilibrium sex ratio.

If male and female offspring cost the same amount of resources on average, then a one-to-one sex ratio will result. More generally, any sex ratio can be calculated as $C_M/(C_M + C_F)$, where C_M is the average resource cost of one male offspring and C_F is the average resource cost of one female offspring (Batterman and Rice [2014], p. 367).

3 Batterman and Rice's Account of the Explanatoriness of Minimal Models

Batterman and Rice's account of the explanatoriness of their minimal models makes use of the concepts of the RG and universality classes. Here I explain these concepts and how they fit into Batterman and Rice's account.

RG is a method of coarse-graining, reducing degrees of freedom or the number of details. Batterman and Rice ([2014], p. 362) discuss one such procedure: Kadanoff's block spin transformation. Consider a lattice of particles, each with an up or down spin. Group the spins into blocks of, for example, four spins and average over each block. One averaging procedure is called 'majority rule', in which a block of four spins is replaced by the most common spin in the block. If there is no most common spin, choose one randomly (see McComb [2004]). This reduces the number of spins in the lattice by a factor of four. The length between spins, or the lattice constant, is greater after averaging, so it is then rescaled to the old lattice constant. Near a critical point, the length across which spins are correlated, or the correlation length, increases and eventually diverges to infinity. When this is the case, averaging over correlated blocks of spins and then rescaling the lattice preserves the macroscopic behaviour of the lattice with fewer degrees of freedom (microscropic details) (Huang [1987], pp. 441–2). The irrelevant details are thereby eliminated.

With the concept of RG in hand, we can define a universality class. After repeated application of RG, certain systems will reach the same fixed point, a state at which RG no longer has an effect. The class of all systems that will reach the same fixed point after repeated application of RG is a universality class.

Using RG, it can be discovered that all systems exhibiting Navier–Stokes behaviour, including LGA, form a universality class that shares the following three features:

(1) Locality: A fluid contains many particles in motion, each of which is influenced only by other particles in its immediate neighborhood.

(2) Conservation: The number of particles and the total momentum of the fluid is conserved over time.

(3) Symmetry: A fluid is isotropic and rotationally invariant. (Batterman and Rice [2014], p. 360; Goldenfeld and Kadanoff [1999], p. 87).

Similarly, an RG-type story would show that all populations exhibiting a one-to-one sex ratio, including Fisher's model, form a universality class and share the feature of linear substitution cost—that is, the average resource cost of male offspring is equal to the average resource cost of female offspring.

According to Batterman and Rice, although RG demonstrates that diverse systems share features with their minimal models, it is not this fact that accounts for the explanatoriness of their minimal models. An account of why minimal models are explanatory must, according to them, answer the three questions presented above. Batterman and Rice argue that RG answers Q2, for both LGA and Fisher's model, because the RG transformation eliminates details that are irrelevant. They write, 'By performing this [RG] operation repeatedly, one can answer question Q2 because the transformation in effect eliminates details or degrees of freedom that are irrelevant' ([2014], p. 362). However, RG alone does not answer this. Q2 asks why the heterogeneous details are irrelevant and RG only shows us that the details are irrelevant. The answer appears to be, 'The details are irrelevant because, as RG shows, the same macro-behaviour results no matter the details'. But this is uninformative.[8]

RG is also supposed to answer the third question by demonstrating that all the fluids within LGA's universality class share the common features of locality, conservation, and symmetry, and that all populations in Fisher's model's universality class share linear substitution cost ([2014], pp. 363, 372). Batterman and Rice ([2014], p. 363) write that,

> A derivative, or *by-product*, of this [RG] analysis is the identification of the shared features of the class of systems. In this case, the by-product is a realization that all the systems within the universality class share the common features locality, conservation, and symmetry. Thus, we get an explanation of why these are the common features as a by-product of the mathematical delimitation of the universality class.

The by-product is merely the identification of the shared features, not why they are shared. Again, RG merely shows that these features are shared across

[8] An anonymous referee suggests the possibility that in this case there is no clear distinction between showing why and showing that the details are irrelevant. I agree that in the LGA case the distinction seems blurry. However, there are clear cases. For example, the entire cerebellum appears to be irrelevant to consciousness, even though it contains more neurons than the cerebral cortex. Knowing this does not tell one why the cerebellum is irrelevant—according to one popular theory, it has to do with the cerebellum's lack of informational integration (Tononi and Koch [2015]).

diverse systems, not why they are shared. Perhaps Batterman and Rice's suggestion is that the fact that RG demonstrates that the details are irrelevant explains why the common features are shared. But this boils down to, 'these features are shared across diverse systems because no other features are shared'. This is also uninformative. RG alone does not explain why locality, symmetry, and conservation are present in, for example, water and LGA, but not anisotropic liquid crystals. Answering that question requires investigation of specific fluids. One reason why liquid crystals are not in the same universality class as LGA and water is that their often rod-shaped particles result in directional preference and lack of symmetry (Priestley *et al.* [1975]). Liquid crystals thus cannot be accurately modelled using the unmodified Navier–Stokes equations. The addition of a stress tensor or coupling with a Q-tensor system is required to take into account the anisotropy of liquid crystals (Badia *et al.* [2011]; Paicu and Zarnescu [2012]). Similarly for Fisher's model: RG alone does not explain why the average resource cost of male and female offspring is equal in, for example, sheep, mule deer, and so on, but not, for example, in bees.

Finally, the answer to the first question follows from the answers to the second and third questions. Obviously, if Batterman and Rice are mistaken about their answers to the second and third questions, then they are also mistaken about the first question.

Perhaps I have interpreted Batterman and Rice too narrowly, and they do not mean that RG alone can answer their three questions. If I am right about RG, Batterman and Rice are wrong merely about how to go about answering the three questions, not that answers are required. Next, then, I present two arguments that such a story is not required, that answering their three questions is unnecessary for an account of the explanatoriness of LGA and Fisher's model.

The first argument rests on an analogy with a commonplace case of multiple realizability. Batterman ([2000], p. 129; see also [2002b], Section 5.5) has plausibly argued that universality just is multiple realizability:

> That microstructurally different systems fall in the same universality or equivalence class, is the physicists' way of saying that the upper level universal behavior is multiply realized. And so, the explanation of the universality of the behavior is an explanation of the behavior's multiple realizability.

The diverse systems in a universality class multiply realize some universal behaviour. Therefore, Batterman argues, RG or similar methods can explain cases of multiple realizability. The following analogy, then, is apt, and the lessons derived therefrom should generalize to Batterman and Rice's account of LGA and Fisher's model. If the lessons do not generalize, Batterman and Rice need to explain why.

Diverse fluids exhibit similar behaviour (for example, critical behaviour) under certain conditions (for example, near critical points). Similarly, diverse objects, such as apples, tomatoes, and bowling balls, exhibit similar behaviour (for example, rolling) under certain conditions (for example, on an incline plane[9]). Rolling under these conditions is universal, or multiply realizable, in apples, tomatoes, and bowling balls; apples, tomatoes, and bowling balls are in the same universality class with respect to rolling. We would like to know why this is, namely, why apples, tomatoes, and bowling balls all roll on an incline plane. These diverse objects behave similarly in certain conditions in virtue of possessing a similar property: (approximate) sphericity. It is their (approximate) sphericity that disposes them all to roll when placed on an incline plane. That fact could be discovered by some RG-like method. That they all share the relevant property of sphericity and that all of their other properties, such as size and colour,[10] are irrelevant to rolling on an incline plane is what explains this similar behaviour and allows us to answer w-questions about it. A minimal model of spherical objects would be in the same universality class as apples, tomatoes, and bowling balls, and would explain their similar behaviour in certain conditions in virtue of accurately representing the relevant property of (approximate) sphericity. Why should our account of the explanatoriness of Batterman and Rice's minimal models differ from this one?

The further question—why are the remaining heterogeneous details, such as the size, material, and colour of these objects, irrelevant for the disposition to roll?—which is analogous to Batterman and Rice's second question, is unnecessary for an account of the explanatoriness of our minimal model of spherical objects. For example, why the colour of an object does not matter to its rolling on an incline plane is a question that can only be answered by a physical investigation into the dispositions bestowed by colour. An investigation in colour physics would reveal why the disposition to roll on an incline plane is not one of the dispositions bestowed by colour. Such an investigation would be unnecessary for knowing or showing that colour is irrelevant to the disposition to roll and, thus, unnecessary for an account of the explanatoriness of our minimal model of rolling.

The question analogous to Batterman and Rice's third question is, 'why do very different objects, such as apples, tomatoes, and bowling balls, all have sphericity in common?'. Intuitively, an answer to this question is beside the point to answering the question of why these objects behave similarly in certain conditions, for example, why they all roll when placed on an incline plane. Furthermore, this question seems to have no good answer. Yet the absence

[9] And in a suitable gravitational environment and so on.

[10] Obviously there are limits in the example as described. For example, if the size of the bowling ball (or apple or tomato) were too large, it would crush the incline plane, unless the plane is sufficiently strong. Assume all these deviant cases are excluded.

of an answer does not suggest that there is no explanation of these diverse objects' disposition to roll on an incline plane. Similarly, there may be no good answer to the question of why some diverse fluids share locality, conservation, and symmetry, or why some diverse populations share linear substitution cost. The story required by Batterman and Rice about why large classes of features are irrelevant may not be available. This analogy should motivate the claim that such a story is unnecessary to answer the question of what makes LGA and Fisher's model explanatory. Batterman and Rice need to say why answers to the three questions are necessary in the cases of LGA and Fisher's model, but not in my rolling case or similar cases of multiple realizability.

The above analogy is entirely consistent with Batterman's ([2000], p. 133; see also [2002b], Section 5.5) own remarks on the multiple realizability of pain:

> Suppose that physics tells us that the physical parameters α and γ are the (only) relevant physical parameters for the pain universality class. That is, that N_h, N_r, and N_m have these features in common when certain generalizations or regularities about pain are the manifest behaviors of interest observed in each of humans, reptiles, and martians. Equivalently, physics has told us that all the other micro-details that legitimately let us think of N_h, N_r, and N_m as heterogeneous are irrelevant. We then have our explanation of pain's realizability by wildly diverse realizers.

This appears to be a common features explanation of exactly the type given above for the multiple realizability of rolling on an incline plane. N_h, N_r, and N_m are the realizers of pain in humans, reptiles, and martians, respectively. They are all in the pain universality class. An RG-type procedure might discover that α and γ are the only relevant common features shared by these realizers. This would be enough to explain the multiple realizability of pain in humans, reptiles, and martians. Further questions such as why humans, reptiles, martians, sentient robots, and everything else in the pain universality class have the pain-conferring features α and γ in common may have no good answer. Answers to the three questions are thus unnecessary for an explanation of the multiple realizability of pain.

There is another reason why answering the three questions is unnecessary. Were answers necessary for an account of the explanatoriness of LGA and Fisher's model, a regress would loom. They write, 'Simply to cite locality, conservation, and symmetry as being explanatorily relevant actually raises the question of why those features are the common features among fluids' ([2014], p. 361). Similarly,

> Common features accounts would likely cite the fact that the different fluids have locality, conservation, and symmetry in common as

> explanatorily relevant and maybe even as explanatorily sufficient. However, [. . .] this is a mistake. The fact that the different fluids all possess these common features is also something that requires explanation. ([2014], p. 374)

Common features are insufficient to explain macroscopic fluid behaviour because, Batterman and Rice argue, they do not answer the further question of why these features are common. With respect to one-to-one sex ratios, Batterman and Rice ([2014], p. 374) write,

> Were we simply to cite the fact that all these populations have the common feature of linear substitution cost, we would fail to explain this universal behavior. The reason for this is that we can equally well ask why the populations of different species distinguished by different mating strategies, and so on, all exhibit a linear substitution cost and why they display the 1:1 sex ratio.

This appears to be an injunction against explanations that appeal to things that also require explanation.[11] But if it is a mistake to explain something by appeal to something else that requires explanation, then nearly all explanations are mistaken. Batterman and Rice need to explain why the chain of explanation should stop where they say it should.

To conclude this section, I have found two problems with Batterman and Rice's account of the explanatoriness of their minimal models. First, it does not appear that RG alone can answer the three questions. Perhaps they did not mean to imply as much. The second problem is that answering the three questions is unnecessary. I gave two arguments for this. First, it is plausible that answers to analogous questions in similar cases of multiple realizability are unnecessary (and potentially unavailable, without thereby threatening explanation), and, second, were answers to the three questions necessary, a regress would loom. Having argued against Batterman and Rice's account, I now present my own common features account.[12]

4 Generalizing the Ontic Conception

The account I propose is similar to the accounts proposed by Bokulich ([2011]), Rice himself ([2015]), and Saatsi and Pexton ([2013]), though I give

[11] This point is also made by Lange ([2015], pp. 303–4).

[12] Perhaps it will be said that I have missed the distinctive feature of Fisher's model: that it is an equilibrium explanation. According to Sober ([1983], p. 202), 'Where causal explanation shows how the event to be explained was in fact produced, equilibrium explanation shows how the event would have occurred regardless of which of a variety of causal scenarios actually transpired'. Equilibrium explanations show how many of the causal details are irrelevant to the explanandum. This presents no challenges I have not already discussed here at length. The common features account given here is much like Rice's ([2015]) own account of equilibrium explanation.

my account an ontic spin.[13] These authors follow Woodward ([2003]) in requiring that an explanation answer what-if-things-had-been-different questions ('w-questions'). According to Woodward ([2003], p. 11), an explanation 'must enable us to see what sort of difference it would have made for the explanandum if the factors cited in the explanans had been different in various possible ways'. This requires the accurate representation of the objective relations of dependence between the explanandum phenomenon and the features on which it depends.

Woodward ([2003], pp. 210–20) is explicit that it is in virtue of conveying counterfactual information that causal claims are explanatory. Since non-causal dependence relations can also convey counterfactual information, they can, therefore, also be explanatory.[14] For example, Saatsi and Pexton ([2013]) present an explanation of Kleiber's law, an allometric scaling law that relates an organism's body mass to a biological observable (West *et al.* [1999]). The precise details of the explanation are irrelevant for our purposes. What matters here is that there is a feature, the scaling exponent, that counterfactually depends on the dimensionality of the organism. It is plausible that this counterfactual dependence relation contributes explanatory power, yet it is implausible that the dimensionality of organisms is a causal variable that can, in practice or in theory, be intervened upon (Saatsi and Pexton [2013], p. 620).

Salmon ([1984], [1989]) distinguished between epistemic, modal, and ontic conceptions of explanation. These are conceptions of what a scientific explanation aims to show of the explanandum phenomenon: that it is expected to occur, that it had to occur, and that it fits 'into a discernible pattern' ([1984], p. 121), respectively. For Salmon, the 'discernible pattern' into which the explanandum phenomenon is fit is structured by causal processes, causal interactions, and causal laws ([1984], p. 132). '[...] we explain', wrote Salmon ([1989], p. 121), 'by providing information about these patterns that reveals how the explanandum-events fit in'. Explanation is not about nomic expectability or nomic necessity, but about fitting the explanandum into 'discernible patterns', 'relationships that exist in the world'. This need not be construed solely causally—it is a mistake to equate the ontic conception with the causal–mechanical account of explanation. Salmon ([1989], p. 184) actually did not think causation was essential to the ontic conception:

> It could fairly be said, I believe, that mechanistic explanations tell us how the world works. These explanations are local in the sense that they show

[13] See also Ruben's ([1990]) 'realist' account of explanation that emphasizes determinative and dependency relations and Thalos's ([2002]) discussion of causal dependence as only one form of explanatory dependence.

[14] Woodward ([2003], Section 5.9) himself suggests dropping the causal requirement in certain cases where an interventionist interpretation is implausible; see also (Strevens [2008], pp. 177–80). I use 'interventionist' and 'manipulationist' interchangeably.

> us how particular occurrences come about; they explain particular phenomena in terms of collections of particular causal processes and interactions—or, perhaps, in terms of non-causal mechanisms, if there are such things.[15]

For Salmon, what was essential to the ontic conception was that, 'the explanation of events consists of fitting them into the patterns that exist in the objective world' ([1989], p. 121). We can and should hold on to the ontic conception while accepting many of the criticisms and limitations of causal explanation, including those provided by Batterman and Rice. There are non-causal dependence relations in which an explanandum phenomenon can stand to other worldly items. Explanation remains, then, a matter of fitting the explanandum phenomenon into 'discernible patterns' and 'relationships that exist in the world', all while acknowledging that these worldly patterns and relationships can be non-causal.

The ontic–epistemic debate has shifted twice since Salmon (Illari [2013]). Salmon framed the debate in terms of what explanations do. After Salmon, the debate was framed metaphysically, as a debate about what explanations are. The ontic conception was associated with the claim that scientific explanations are (almost always causal) dependence relations in the world; the epistemic conception was associated with the claim that scientific explanations are epistemic states or representations. Craver's ([2014]) most recent formulation of the ontic conception backs away from the metaphysical claim that explanations are ontic structures in the world and focuses on demarcatory and normative constraints on explanation.[16] Craver ([2014]) writes that according to the ontic conception, 'in order to satisfy these two objectives [of explanatory demarcation and explanatory normativity], one must look beyond representational structures to the ontic structures in the world' (p. 28). That is, attention to ontic structures, rather than epistemic or representational form, is required in order to demarcate explanation from other scientific achievements, like prediction, and to distinguish good from bad explanations, how-possibly from how-actually explanations, and explanatorily relevant from irrelevant features ([2014], p. 51).[17]

[15] See also, for example, 'the ontic conception focuses upon the fitting of events into natural regularities. Those regularities are *sometimes*, if not always, causal' ([1989], p. 120; my emphasis) and, 'explanations reveal the mechanisms, causal *or other*, that produce the facts we are trying to explain' ([1989], p. 121; my emphasis). Salmon then says that Railton's ([1978], [1981]) account is an ontic conception even though, 'His view is more lenient than mine with regard to noncausal explanation' ([1989], p. 121). Salmon also clearly thought that laws, construed as ontic regularities, can be explanatory (see, for example, [1984], pp. 17–8, 121, [1989], p. 120). See especially (Salmon [1989], pp. 120, 129) for explicit claims that focus on the laws themselves, rather than law-statements, leads to the ontic conception.

[16] According to Illari ([2013], p. 241), Craver (in personal communication with Illari) holds that this has always been the debate.

[17] Under this framing of the debate, Wright ([2012]) overemphasizes the role that lexical ambiguity plays in the case for the ontic conception. The argument, which I do not have space here to

The generalized ontic conception, then, is an ontic conception because it embraces Craver's claim that achieving the objectives of explanatory demarcation and normativity requires attention to the ontic. It is generalized because it says that attention to more of the ontic than just the causal–mechanical is required to achieve those objectives—attention to all ontic structures on which the explanandum depends is required.[18]

The ontic conception, unhindered by a strictly causal–mechanical interpretation, retains the ability to demarcate explanation from description and prediction. Explanations provide information about relations of ontic dependence, causal and non-causal, which can be used to answer w-questions about the explanandum phenomenon. Understanding is possessing this information, and, therefore, knowing answers to w-questions.[19] Norms of explanation immediately fall out of this account: the more relevant dependencies that are represented for a given phenomenon and the more irrelevant dependencies that are not, the more w-questions can be answered, the better the explanation of that phenomenon.

Let me clarify the relation between the aspect of my account that emphasizes dependence relations and the counterfactual aspect that emphasizes the ability to answer w-questions. These aspects are tightly intertwined, but relations of dependence are not analysed in terms of counterfactuals or reduced to counterfactuals. Analysis and reduction apply to terms, concepts, or theories, not the things to which they refer. Rather, relations of counterfactual dependence hold in virtue of, or are grounded in, relations of ontic dependence. Like supervenience, counterfactual dependence is a modal concept (Heil [2003], p. 37). Different relations of ontic dependence could ground supervenience, including, among others, identity, constitution, and causal sufficiency ([2003], p. 67). Supposing that what grounds counterfactual dependence relations also makes (descriptions of) them true, we can put this in terms of truthmakers: relations of ontic dependence provide truthmakers for counterfactuals.[20]

It is only with information about dependence that one can answer w-questions. This is why the ontic aspect of my account is inseparable from the counterfactual

defend, for Craver's claims about explanatory demarcation and normativity does not require any lexical ambiguity of the term 'explanation'.

18 I thank an anonymous referee for pressing me to clarify the ontic conception and my account's relation to it.

19 More needs to be said about understanding than I am able to say here; see, for example, (Strevens [2013]) for the kind of view to which I am sympathetic.

20 Though I think this way of putting it is illuminating, it is controversial both in light of possible-world semantics for counterfactuals and in light of disagreement about the relation between grounding and truthmaking. For a survey of possible relations between grounding and truthmaking, see (Griffith [2014]).

aspect. This is why one cannot say that explanation is a matter of answering w-questions, but not a matter of accurately representing dependencies. Bokulich ([2011]), Rice ([2015]), Saatsi and Pexton ([2013]) emphasize the importance for explanation of the ability to answer w-questions and are silent about ontic relations, but these issues cannot be separated. Consider the counterfactual, 'If population P had lacked linear substitution cost, it would not have a one-to-one sex ratio'. What grounds this counterfactual is the (perhaps causal) dependence between the population's linear substitution cost and its one-to-one sex ratio. Those who think of explanation in terms of the ability to answer w-questions should thus embrace the account presented here.

The ontic aspect of my account also allows one to distinguish explanatorily relevant from irrelevant counterfactuals. The length of a flagpole's shadow can be derived from the height of the pole and the angle of elevation of the sun (Bromberger [1966]). This derivation is symmetric. That is, one can also derive the height of the flagpole from the length of its flagpole's shadow and the angle of elevation of the sun. It seems, then, that if the shadow had been longer and the sun in the same position, then the flagpole would have been higher. Yet it does not seem true that this explains the height of the flagpole. Here it is plausible that the explanatory asymmetry is provided by causal asymmetry: the derivation of the length of the pole's shadow counts as explanatory because that derivation, but not the reverse derivation, tracks causes (Hausman [1998]; Woodward [2003]). This lesson can be generalized to cases of non-causal dependence: in general, when there are explanatory asymmetries, these are due to asymmetries in ontic dependence.

Symmetry provides a nice example of something on which fluid behaviour non-causally depends. As I mentioned above, there are fluids, like anisotropic liquid crystals, that have a preferential alignment due to their banana- and rod-shaped molecules and thus cannot be accurately modelled using the unmodified Navier–Stokes equations. The dependence of the macro-behaviour of liquid crystals on the shape of their particles is plausibly not a causal dependence or mechanistic dependence. A feature or disposition of the whole liquid, its macro-behaviour, depends on the features of its mereological parts, so construing this dependence causally is inappropriate (assuming, plausibly, that parts and wholes cannot stand in causal relations to each other; see Craver and Bechtel [2007]). Yet, it is also plausible that the particles are not a mechanism that produces, underlies, or maintains the fluid's macro-behaviour. Mechanisms are organized in a way that mere aggregates are not (Craver [2001]), and while I recognize that there is something of a continuum here, fluid particles do not appear to have the requisite organization to constitute a mechanism. Here, then, is an instance of ontic dependence that is neither causal nor mechanistic, but is asymmetric and can be used to answer w-questions about fluid behaviour.[21]

Batterman and Rice ([2014], p. 360) remark only in passing that it 'stretches the imagination' to think of locality, symmetry, conservation as causally relevant.[22] I agree, but I do think it is plausible that linear substitution cost can be given a causal interpretation, though I do not think a causal interpretation is required for that feature to be explanatory. Woodard ([2003]) has given the most influential account of causal relevance. Very briefly, according to Woodward, x is causally relevant to y if and only if a sufficiently surgical manipulation (or 'intervention') of x would change y. Here, 'sufficiently surgical' means that a manipulation of x that would change y would do so only via the pathway from x to y.

It is important to note that on Woodward's view, the manipulation need not be physically possible. All that is necessary is that relevant scientific theory be able to answer what would happen under the imagined intervention. For example, considering the counterfactual claim that changes in the position of the moon cause changes in the motion of the tides, Woodward ([2003], p. 131) writes,

> Newtonian theory and familiar rules about the composition of forces tell us how to subtract out any direct influence from such a process so that we can calculate just what the effect of, say, doubling of the moon's orbit (and no other changes) would be on the tides, even though it also may be true that there is no way of actually realizing this effect alone. In other words, Newtonian theory itself delivers a determinate answer to questions about what would happen to the tides under an intervention that doubles the moon's orbit, and this is enough for counterfactual claims about what would happen under such interventions to be legitimate and to allow us to assess their truth.

If physical and biological theories can tell us what would happen under hypothetical interventions, then causal relevance can be established.

A causal interpretation of linear substitution cost is plausible on a manipulationist account. Recall that linear substitution cost is equality between the average resource costs of male and female offspring. Here is a hypothetical intervention on average resource cost: inject all and only the males of a population with a fluid that has the only effect of raising their metabolism and increasing their average resource cost. Do this over many generations in a

[21] I suspect that many explanations of dispositions in terms of their micro-bases will have this non-causal, non-mechanistic structure.

[22] Lange ([2015], p. 300) points out that this is plausible if it means that locality, symmetry, and conservation are not causes, but implausible if it means that they cannot figure in causal explanations; see also Footnote 23.

population that initially had a one-to-one sex ratio and you will eventually see a deviation from a one-to-one sex ratio.

One might object that this hypothetical intervention does not show that linear substitution cost is causally relevant to one-to-one sex ratios, only that metabolism is causally relevant, since this is what was manipulated. This objection is conceptually confused. In the case at hand, manipulating metabolism just is manipulating average resource cost. It does not matter if manipulating metabolism were but one way among many of manipulating average resource cost. There are usually many different ways to manipulate a variable. Although, according to the generalized ontic conception, linear substitution cost need not be causally relevant to be explanatorily relevant, it plausibly is causally relevant on the manipulationist account.

It is much less plausible that conservation and locality are causally relevant to the macro-behaviour of fluids. Conservation is a paradigm law of nature. It is hard to imagine any hypothetical interventions that would alter this regularity. One can imagine 'local miracles', local speedings up, slowings down, and poppings into and out of existence of a fluid's particles, and this would certainly change the macro-behaviour of the fluid. Physical theory might even be able tell us what would happen in such a contra-nomic or counter-legal scenario, but construing laws as causally relevant in the interventionist sense is highly implausible because laws are not events or objects and particles are mereological parts of the fluid.[23]

According to the generalized ontic conception, then, LGA explains Navier–Stokes behaviour and Fisher's model explains one-to-one sex ratios in virtue of accurately representing all and only the relevant features: symmetry, locality, and conservation for fluid behaviour, and linear substitution cost for one-to-one sex ratios. Knowing that these features alone are the relevant ones allows one to answer w-questions about fluid behaviour and one-to-one sex ratios. The essential role RG plays is in discovering and demonstrating that these are the relevant features. RG and universality classes do not provide a kind of explanation distinct from common features explanations. Rather, RG

[23] This is not to deny that conservation laws are causally relevant in the sense that they govern or constrain all causal interactions, in Salmon's ([1984], pp. 169–70) sense of that term. Nor am I denying that citing a law can provide information about a phenomenon's causal history (Skow [2014]). I am only denying that conservation laws are causally relevant in the interventionist sense. See (Lange [2007], [2011]) for valuable discussions of the nature and explanatory status of conservation laws.

and similar procedures are necessary methods used in the construction of common features explanations.[24,25]

5 Conclusion

Batterman and Rice are at the forefront of a philosophical exploration of the limits of causal explanation. They have argued forcefully and plausibly that certain models in physics and biology are not explanatory in virtue of accurately representing causes (for example, Batterman [2002a], [2002b]; Rice [2012], [2015]). In their recent paper, Batterman and Rice ([2014]) use the minimal models to critique the explanatory requirement of accurate representation, regardless of whether the features accurately represented are causal.

[24] Cf. (Batterman [2000], p. 128): 'The RG type analysis illuminates those physical features that *are* relevant for the upper level universal behavior, *and at the same time demonstrates that all of the other details which distinguish the systems from one another are irrelevant*'. More compactly, '[RG] is a *method for extracting structures* that are, with respect to the behavior of interest, detail independent' ([2000], p. 128; added emphasis). Also: '[T]here are a number of techniques for *demonstrating that* a large class of details of particular systems is irrelevant to their macroscale behavior' Batterman and Rice ([2014], p. 371; added emphasis). These quotations are consistent with my account of the role of RG.

[25] Reutlinger ([2014]) has argued that RG explanations are non-causal because they are a kind of 'distinctively mathematical explanation', although they do not exploit mathematical necessity, in contrast to Lange's ([2013]) account of distinctively mathematical explanation. Rather, the mathematical operations involved in RG account for RG's explanatory power. She writes:

> The mathematical explanatory power is derived from [...] the [RG] transformations and flow of Hamiltonians [to a fixed point]. Both the transformations and the 'flow' are mathematical operations, which, ultimately, serve the purpose to reveal something that two fluids have in common despite the fact that their 'real physical' Hamiltonians (or 'initial physical manifolds') are strikingly different. ([2014], pp. 1166, 1168)

I agree that the mathematical operations of RG reveal common features, but I do not agree that those operations are the sole contributors of explanatory power. If that were true, we would seem to have a case where representing the things on which an explanandum depends does not contribute explanatory power, but the method(s) used to reveal, discover, or demonstrate the relevance of those things does. This seems false in my multiple realizability of rolling example, in which case it cannot be true of explanations of multiple realizability in general. That is, it seems false that representing their shared (approximate) sphericity does not contribute explanatory power to the explanation of the multiple realizability of rolling by apples, tomatoes, and bowling balls, but that the method(s) used to reveal, discover, or demonstrate that (approximate) sphericity is the only relevant, common property does contribute. Similarly for the multiple realizability of pain, briefly discussed above. Rather, representing the only relevant common features on which our explanandum depends is what contributes explanatory power, by allowing us to answer w-questions about it. The methods, mathematical or not, that we use to discover that (approximate) sphericity is the only relevant property do not contribute any explanatory power in themselves; they are simply tools used in the construction of the 'common features' explanation. This is how I see the role of RG. Note that if Reutlinger's distinctively mathematical account is to be extended to other minimal models, some analogues of the mathematical operations of RG must be specified, since those are the operations that he argues contribute explanatory power. In biological contexts, for example, it is unclear what such operations could be.

According to Batterman and Rice, the explanatoriness of LGA and Fisher's model is captured by a story about why heterogeneous details are irrelevant, a story that answers the three questions. I identified two problems with this account. First, RG alone cannot answer the three questions. Perhaps RG in conjunction with other methods can. Even so, the second problem is that answers to these questions are in fact unnecessary. I argued for this by showing (i) that answers to analogous questions in an analogous case of multiple realizability are unnecessary, and (ii) that if answers to the three questions were necessary, a regress would loom.

Batterman and Rice have rightly stressed the significance of RG explanation, but have misplaced where that significance lies. These methods do not provide novel kinds of explanation. RG is a unique method that is necessary to extract the relevant features of the world that explain the phenomena in which physicists are often interested. The explanatoriness of the minimal models they present, LGA and Fisher's model, can be adequately captured by a common features approach, the generalized ontic conception. These minimal models explain by accurately representing the features on which their explananda depend, causally or non-causally. These accurate representations can then be used to answer w-questions about the explananda, which contributes to their explanatory power.

Acknowledgements

Thanks to Carl Craver, Dylan Doherty, Eric Hochstein, Anya Plutynski, two anonymous referees, and audience members of the 2014 St. Louis Area Philosophy of Science Association (SLAPSA) conference for invaluable feedback.

Philosophy-Neuroscience-Psychology
Washington University in St. Louis
St. Louis, MO, USA
mapovich@wustl.edu

References

Badia, S., Guillén-Gónzalez, F. and Gutiérrez-Santacreu, J. V. [2011]: 'An Overview on Numerical Analyses of Nematic Liquid Crystal Flows', *Archives of Computational Methods in Engineering*, **18**, pp. 285–313.

Batterman, R. [2000]: 'Multiple Realizability and Universality', *British Journal for the Philosophy of Science*, **51**, pp. 115–45.

Batterman, R. [2002a]: 'Asymptotics and the Role of Minimal Models', *British Journal for the Philosophy of Science*, **53**, pp. 21–38.

Batterman, R. [2002b]: *The Devil in the Details: Asymptotic Reasoning in Explanation, Reduction, and Emergence*, Oxford: Oxford University Press.

Batterman, R. and Rice, C. [2014]: 'Minimal Model Explanations', *Philosophy of Science*, **81**, pp. 349–76.

Bokulich, A. [2011]: 'How Scientific Models Can Explain', *Synthese*, **180**, pp. 33–45.

Bromberger, S. [1966]: 'Why Questions', in R. G. Colodney (*ed.*), *Mind and Cosmos*, Pittsburgh, PA: University of Pittsburgh Press, pp. 86–111.

Craver, C. F. [2001]: 'Role Functions, Mechanisms, and Hierarchy', *Philosophy of Science*, **68**, pp. 31–55.

Craver, C. F. [2006]: 'When Mechanistic Models Explain', *Synthese*, **153**, pp. 355–76.

Craver, C. F. [2014]: 'The Ontic Account of Scientific Explanation', in M. Kaiser, O. Scholz, D. Plenge and A. Hüttemann (*eds*), *Explanation in the Special Sciences: The Case of Biology and History*, New York: Springer, pp. 27–54.

Craver, C. F. and Bechtel, W. [2007]: 'Top-down Causation without Top-down Causes', *Biology and Philosophy*, **22**, pp. 547–63.

Fisher, R. A. [1930]: *The Genetical Theory of Natural Selection*, Oxford: Clarendon Press.

Glennan, S. [2002]: 'Rethinking Mechanistic Explanation', *Philosophy of Science*, **69**, pp. 342–53.

Goldenfeld, N. and Kadanoff, L. P. [1999]: 'Simple Lessons from Complexity', *Science*, **284**, pp. 87–9.

Griffith, A. M. [2014]: 'Truthmaking and Grounding', *Inquiry*, **57**, pp. 196–215.

Hamilton, W. D. [1967]: 'Extraordinary Sex Ratios', *Science*, **156**, pp. 477–88.

Hausman, D. [1998]: *Causal Asymmetries*, New York: Cambridge University Press.

Heil, J. [2003]: *From an Ontological Point of View*, Oxford: Oxford University Press.

Huang, K. [1987]: *Statistical Mechanics*, New York: Wiley and Sons.

Huneman, P. [2010]: 'Topological Explanations and Robustness in Biological Sciences', *Synthese*, **177**, pp. 213–45.

Illari, P. [2013]: 'Mechanistic Explanation: Integrating the Ontic and Epistemic', *Erkenntnis*, **78**, pp. 237–55.

Kaplan, D. M. [2011]: 'Explanation and Description in Computational Neuroscience', *Synthese*, **183**, pp. 339–73.

Lange, M. [2007]: 'Laws and Meta-laws of Nature: Conservation Laws and Symmetries', *Studies in History and Philosophy of Modern Physics*, **38**, pp. 457–81.

Lange, M. [2011]: 'Conservation Laws in Scientific Explanations: Constraints or Coincidences?', *Philosophy of Science*, **78**, pp. 333–52.

Lange, M. [2013]: 'What Makes a Scientific Explanation Distinctively Mathematical?', *British Journal for Philosophy of Science*, **64**, pp. 485–511.

Lange, M. [2015]: 'On "Minimal Model Explanations": A Reply to Batterman and Rice', *Philosophy of Science*, **82**, pp. 292–305.

McComb, W. D. [2004]: *Renormalization Methods: A Guide for Beginners*, Oxford: Oxford University Press.

Paicu, M. and Zarnescu, A. [2012]: 'Energy Dissipation and Regularity for a Coupled Navier–Stokes and Q-Tensor System', *Archive for Rational Mechanics and Analysis*, **203**, pp. 45–67.

Pincock, C. [2012]: *Mathematics and Scientific Representation*, Oxford: Oxford University Press.

Priestley, E. B., Wojtowicz, P. J. and Sheng, P. [1975]: *Introduction to Liquid Crystals*, New York: Plenum Press.

Railton, P. [1978]: 'A Deductive–Nomological Model of Probabilistic Explanation', *Philosophy of Science*, **45**, pp. 206–26.

Railton, P. [1981]: 'Probability, Explanation, and Information', *Synthese*, **48**, pp. 233–56.

Reutlinger, A. [2014]: 'Why Is There Universal Macrobehavior? Renormalization Group Explanation as Noncausal Explanation', *Philosophy of Science*, **81**, pp. 1157–70.

Rice, C. [2012]: 'Optimality Explanations: A Plea for an Alternative Approach', *Biology and Philosophy*, **27**, pp. 685–703.

Rice, C. [2015]: 'Moving Beyond Causes: Optimality Models and Scientific Explanation', *Noûs*, **49**, pp. 589–615.

Ruben, D. H. [1990]: *Explaining Explanation*, London: Routledge.

Saatsi, J. and Pexton, M. [2013]: 'Reassessing Woodward's Account of Explanation: Regularities, Counterfactuals, and Noncausal Explanations', *Philosophy of Science*, **80**, pp. 613–24.

Salmon, W. C. [1984]: *Scientific Explanation and the Causal Structure of the World*, Princeton, NJ: Princeton University Press.

Salmon, W. C. [1989]: 'Four Decades of Scientific Explanation', in W. Salmon and P. Kitcher (*eds*), *Scientific Explanation*, Minneapolis, MN: University of Minnesota Press, pp. 3–219.

Skow, B. [2014]: 'Are There Non-causal Explanations (of Particular Events)?' *British Journal for the Philosophy of Science*, **65**, pp. 445–67.

Sober, E. [1983]: 'Equilibrium Explanation', *Philosophical Studies*, **43**, pp. 201–10.

Strevens, M. [2008]: *Depth: An Account of Scientific Explanation*, Cambridge, MA: Harvard University Press.

Strevens, M. [2013]: 'No Understanding without Explanation', *Studies in History and Philosophy of Science*, **44**, pp. 510–15.

Thalos, M. [2002]: 'Explanation Is a Genus: An Essay on the Varieties of Scientific Explanation', *Synthese*, **130**, pp. 317–54.

Tononi, G. and Koch, C. [2015]: 'Consciousness: Here, There, and Everywhere?', *Philosophical Transactions of the Royal Society B*, **370**, p. 20140167.

Weisberg, M. [2013]: *Simulation and Similarity: Using Models to Understand the World*, Oxford: Oxford University Press.

West, G., Brown, J. H. and Enquist, B. [1999]: 'The Fourth Dimension of Life: Fractal Geometry and Allometric Scaling of Organisms', *Science*, **284**, pp. 1677–9.

Woodward, J. [2003]: *Making Things Happen*, Oxford: Oxford University Press.

Woodward, J. [2013]: 'Mechanistic Explanation: Its Scope and Limits', *Proceedings of the Aristotelian Society Supplementary Volume*, **87**, pp. 39–65.

Wright, C. D. [2012]: 'Mechanistic Explanation without the Ontic Conception', *European Journal of Philosophy of Science*, **2**, pp. 375–94.