On "Minimal Model Explanations": A Reply to Batterman and Rice

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Batterman and Rice offer an account of "minimal model explanations" and argue against "common features accounts" of those explanations. This paper offers some objections to their proposals and arguments. It argues that their proposal cannot account for the apparent explanatory asymmetry of minimal model explanations. It argues that their account threatens ultimately to collapse into a "common features account." Finally, it argues against their motivation for thinking that an explanation appealing to "common features" would have to explain the common features' own prevalence.

1. Introduction. Batterman and Rice (2014, hereafter "B&R") contend that there is a class of scientific explanations that are explanatory for reasons that have not been properly understood in the literature on scientific explanations. These explanations involve "minimal models." These "minimal model explanations" explain why a given system exhibits some macroscale behavior that is also exhibited by many other systems that are extremely diverse at the microscale. Philosophers must account for how these explanations manage to generate their explanatory power.

In particular, B&R argue that what makes a minimal model explanation explanatory is not well captured by what they call "common features accounts" of scientific explanation (including mechanistic, causal, and difference-making accounts), according to which what makes a model explanatory is its having some causal, mathematical, topological, or other structural features in common with the real system the behavior of which it explains. By

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contrast with "common features accounts," B&R propose that the explanatory power of a minimal model explanation does not arise from its accurately capturing relevant features of the target system: "We have argued that what accounts for the explanatory power of this model is not that it correctly mirrors, maps onto, or otherwise accurately represents the real systems of interest" (364).[1] I object that B&R's arguments do not provide strong support for this claim.

In section 2 I sketch B&R's account. In the following sections, I object to several of B&R's central arguments that their account is superior to "common features accounts." In section 3 I argue that B&R's approach cannot account for the explanatory asymmetry that they apparently ascribe to minimal model explanations: the fact that a minimal model can help to explain a target system's behavior, but the target system cannot help to account for the minimal model's behavior. In section 4 I argue that B&R's account threatens ultimately to collapse into a "common features account." Finally, in section 5 I object to one of B&R's criticisms of "common features accounts," namely, that an explanation appealing to a "common feature" would have to explain the common feature's own prevalence.

2. B&R's Account of Minimal Model Explanations. Here is a brief summary of B&R's account of how "minimal model explanations" work. Such an explanation begins with a space of possible systems. One point of this space represents the "minimal model" that will ultimately figure in the explanation, whereas other points represent various actual systems (including the system some macrobehavior of which is to be explained), and still other points represent various nonactual systems. The explanation includes an argument showing that certain systems in the space (including the target system and the minimal model), despite differing widely in their microdetails, belong to the same "universality class" in that they all exhibit the same macrobehavior. This part of the explanation—the argument that B&R call "the process of delimiting a universality class" (350)—shows why the heterogeneous microdetails are irrelevant to the behavior being explained. This argument is the heart of a "minimal model explanation," according to B&R.

1. Sometimes B&R treat the model as representing or describing the system it explains and so put their point in terms of the model's not deriving its explanatory power from its accuracy: "Many models are explanatory even though they do not accurately describe the actual causal mechanisms that produced the phenomenon" (352). In other places, B&R treat the model not as a representation, but instead as having components standing in causal and other kinds of relations. Then B&R put their point in terms of the model's not deriving its explanatory power from having features in common with the target system; for instance, in opposing what they call "common features accounts" of explanation, they deny that "the explanatory power of a model comes from its having these relevant causes in common with the model's target system(s)" (353). I will elide this distinction as B&R do.

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Furthermore, "a by-product of this process will be finding a set of features the members of the universality class have in common . . . As a result, we are able to see why those features are common features despite the various differences among these systems" (372). For instance, here are the "common features" in B&R's example of the explanation of certain large-scale features of fluid flow supplied by the minimal model called the "lattice gas automaton" (LGA): locality (that fluid particles are influenced only by nearby particles), conservation (of particle number and fluid momentum), and symmetry (e.g., that the fluid is rotationally invariant).

As B&R emphasize, this is a point where their account differs from "common features accounts." B&R agree that "all the systems within the universality class share the common features locality, conservation, and symmetry" (363).[2] But according to common features accounts, the minimal model's explanatory power derives from its and the target system's having common features: "A common features account would presumably argue that the reason the LGA works is because it shares locality, conservation, and symmetry with the real fluid we are targeting. . . . It is in virtue of including these common features that the model is explanatory" (360–61). By contrast, B&R regard the presence of these features in all members of the universality class as explained by the minimal model explanation as a "by-product" of its delimiting the universality class in the course of explaining the target system's macrobehavior: "by showing that all the systems in this universality class have these features, we answer [this question:] why are these common features necessary for the phenomenon to occur?" (372). A common features account regards possession of various "common features" as sufficient (or as making it highly likely) for a system to exhibit the macrobehavior being explained and regards the target system's possession of these "common features" as explaining why it exhibits that macrobehavior. By contrast, B&R's account regards possession of these common features as necessary for a system to exhibit that macrobehavior (and so present in all members of the universality class) and as failing to explain why the target system exhibits the macrobehavior.

Indeed, B&R hold that among the "shortcomings" (373) of common features accounts is that they incorrectly regard minimal model explanations as not having to explain three further facts: why the common features are necessary for the macrobehavior, why these features are present in all members of the universality class despite their microheterogeneity, and why those microdetails make no difference to whether the macrobehavior occurs.

2. In a passage (364) that I quote below, B&R say that minimal models "look nothing like" their targets. But this is not literally true. By B&R's own admission, the LGA model (for instance) shares certain "common features" (such as locality, conservation, etc.) with the target system, and so in these respects they look alike. B&R's point is that these common features are not the source of the model's explanatory power.

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So far, we have seen only that according to B&R, the minimal model explanation includes an argument showing that all systems in a given class (that includes the target system and the minimal model) are alike in their macrobehavior (despite their microheterogeneity). But we have not yet reached the particular macrobehavior to be explained, and we have not yet employed the minimal model. At this point, then, the explanation appeals to the fact that the minimal model exhibits the macrobehavior to be explained. Since the minimal model belongs to the same universality class as the target system, it follows that the target system exhibits the macrobehavior. This completes the explanation of why the target system exhibits that behavior: "Finally, we also see that [the] minimally accurate model is within the universality class containing the real systems whose behavior we want to explain. This allows us to understand how such a minimally accurate model could be used to explain and understand real systems. We not only answer the essential questions for explaining the ubiquity of the [macrobehavior], but we now understand why we are able to use [the] model to explain the patterns we observe in real . . . systems" (373).

3. An Objection from Explanatory Asymmetry. From the fact that the minimal model belongs to the same universality class as the target system, together with the fact that the minimal model exhibits the given macrobehavior, we can predict that the target system exhibits that macrobehavior. But not every argument that predicts is an argument that explains. Does this argument explain why the target system exhibits the macrobehavior? The target system and the minimal model are simply two systems in the universality class. Why does the behavior of one of these systems help to explain the behavior of the other?

As we have seen, a minimal model explanation (according to B&R) includes an argument showing that all systems in a given class (that includes the target system and the minimal model) are alike in their macrobehavior, whatever that macrobehavior turns out to be. This argument treats the target system and the minimal model alike; both are simply members of the universality class that this argument delimits. But the explanation apparently treats the target system and the minimal model differently: the target system's macrobehavior is explained, according to B&R, by the minimal model's macrobehavior. How does the minimal model's macrobehavior acquire explanatory priority over the target system's macrobehavior?

B&R's proposal offers no satisfactory account for the minimal model's explanatory priority over the target system. For instance, with regard to the explanation that appeals to the LGA, B&R write,

> The model is explanatory and can be employed to understand the behavior of real fluids primarily because of a backstory about why various details that

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> distinguish fluids and fluid models from one another are essentially irrelevant. . . . The renormalization group strategy, in delimiting the universality class, provides the relevant modal structure that makes the model explanatory: we can employ complete caricatures—minimal models that look nothing like the actual systems—in explanatory contexts because we have been able to demonstrate that these caricatures are in the relevant universality class. As such, one might as well use those minimal models for computational ease. The backstory guarantees that, with respect to continuum scale behaviors, it will reproduce the behaviors of real systems. (364)

But guaranteeing that the LGA will reproduce those behaviors is not the same as guaranteeing that the LGA will explain those behaviors. That "we have been able to demonstrate that these caricatures are in the relevant universality class" does not suffice to allow us to "employ [them] in explanatory contexts" if this means using the fact that the minimal model exhibits a certain behavior as part of the reason why the target system does so. If our demonstrating that the model and target system are in the same universality class were sufficient to allow us to use one to explain the other, then we might just as well use the target system to explain why the model exhibits the given macrobehavior.

At least, we might just as well do so except for the "computational ease" (that B&R mentioned above) of deriving that the minimal model exhibits the given macrobehavior. But computational ease is a merely pragmatic matter and so does not have any influence on explanatory priority. (I return to this point below.)

To my objection that their proposal does not account for the asymmetry of minimal model explanations, B&R might reply that minimal model explanations do not exhibit asymmetry. On their account, minimal model explanations do not work by accurately representing the causal processes underlying the explanandum. Since minimal model explanations inherit no explanatory asymmetry from the asymmetry of causation, B&R might propose that minimal model explanations are not asymmetric at all: although in some contexts (e.g., for some audiences) the minimal model's displaying a given macrobehavior helps to explain the target system's doing so, in other contexts (perhaps merely hypothetical ones) an explanation can run in the opposite direction. As long as no possible contexts allow both directions of explanation, there will be no explanatory circularity—that is, no case where system A's exhibiting a certain macrobehavior helps to explain why system B does so and vice versa.

However, minimal model explanations do seem to exhibit explanatory asymmetry. Scientific practice does not include cases where the macrobehavior of some austere, minimal model is explained partly by the mac-

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robehavior of some messy, real-world system. B&R might reply that this is because minimal models exhibit much greater "computational ease" than messy, real-world systems. We can completely represent minimal models and so can infer, using this representation, that the minimal model exhibits the given macrobehavior, whereas we cannot do this for the target system. If we had a complete representation of the target system, then we could (with enough computational power and background knowledge) use that representation to infer directly that the target system exhibits the given macrobehavior. We could then combine this inference with the "backstory" showing that all members of the universality class exhibit the same macrobehavior. B&R might claim that under these circumstances, we could use this combination to explain why the minimal model exhibits the given macrobehavior.

But under these circumstances, I see nothing to prevent explanatory circularity: in this context, the minimal model's macrobehavior would help to explain the target system's macrobehavior and vice versa. This seems implausible. Furthermore, even in actual scientific practice, there could well be cases where a given universality class contains two models, each of which is sufficiently computationally tractable to allow us to compute directly regarding each that it exhibits the given macrobehavior. (For example, one model might include every factor incorporated into the other, more minimal model plus only a single additional factor—one that turns out to make no difference to the macrobehavior being explained.) But presumably we would not regard each of these models as helping to explain why the other exhibits the given macrobehavior. Rather (as I will discuss in the next section), we would regard their "common features" as explaining the macrobehavior they share.

According to B&R, minimal model explanations do not work by accurately representing the target system's causal processes. Therefore, minimal model explanations derive no explanatory asymmetry from causal asymmetry. I suggested above that B&R might deem this a strong argument that minimal model explanations lack explanatory asymmetry. However, explanatory asymmetry is exhibited even by many noncausal explanations. For instance (adapting an example from Braine 1972, 144; see also Lange 2013), Mother's failure every time she tries to distribute her strawberries equally among her children without cutting any (strawberries or children) is explained noncausally by the fact that she has 22 strawberries and 3 children together with the mathematical fact that 3 does not go evenly into 22. But although this explanation is noncausal, it exhibits asymmetry: Mother's repeated failure cannot be used to help explain the mathematical fact or to help explain the numbers of her strawberries and children. Likewise, Nerlich (1991) argues that the global structure of space explains geometrically

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(rather than causally) why left and right hands cannot be superimposed. But facts about hands are not responsible for facts about the global geometry of the space in which they are embedded.

4. B&R's Account Appeals to "Common Features." A "common features account" presumably has an easier time accounting for the minimal model's apparent explanatory priority over the target system. The minimal model and the target system share the common features that are responsible for the macrobehavior. But unlike the target system, the minimal model involves no other features that might make for added complications. It isolates the common features' contribution, showing what those common features produce in the absence of other influences and by what means they produce it. On this picture, when the minimal model is apparently used to explain the target system, it is really the minimal model's possessing certain "common features" (and no disturbing factors) that explains its macrobehavior and the target system's possessing those same features (and small enough disturbing factors) that explains its exhibiting the same macrobehavior. The minimal model contributes to the explanation by showing how the macrobehavior results from the common features if they are the principal influences at work.

On such a "common features account," then, the minimal model's possessing the common features does not partly explain the target system's exhibiting the macrobehavior—unlike in B&R's "minimal model explanations." In particular, then, the minimal model's "computational ease" gives it no explanatory priority over the target system, unlike in B&R's account. However, in such a "common features account," the minimal model's possessing the common features in isolation from disturbing factors allows an explanation to use the minimal model to show how the macrobehavior being explained arises from the possession of those common features when disturbing influences are small enough. The minimal model is therefore explanatorily useful in explaining the macrobehavior of a messy, real-world system—but not vice versa. (Since the model's explanatory utility arises from its having certain features in common with the target system without the disturbing factors present in the target system, the model's explanatory utility arises from its representing accurately enough the target system, contrary to B&R's view.) The minimal model's explanatory utility derives not from its computational ease but rather from what gives it its computational ease: its possessing the common features without disturbing factors.

A slightly different version of a "common features account" regards a minimal model simply as describing certain features of the target system. The minimal model explains the target system's macrobehavior by representing the factors (e.g., causal processes or structural constraints) primarily responsible for that macrobehavior and showing how those factors (in

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the absence of disturbing influences) result in that macrobehavior. The minimal model is explanatorily prior to the target system's macrobehavior in that it depicts the properties the possession of which by the target system is responsible for that behavior. (Once again, in this account the minimal model's computational ease does not contribute to its explanatory power but rather results from what gives it its explanatory power: its capturing certain properties while leaving others out.)

B&R object to "common features accounts" on the grounds that "simply citing the common features" is "clearly insufficient to explain the behaviors" (361) of target systems. But a common features account need not say that simply citing the common features suffices; it can require that an explanation show how the common features result in the macrobehavior. It might, for instance, require an explanation to trace the way in which causes or laws possessing the common features give rise to the macrobehavior—thereby using the minimal model to describe processes occurring in the target system. But, of course, this picture runs contrary to B&R's view, according to which what accounts for the models' explanatory power "is not that they accurately mirror, map, track, or otherwise represent real systems" (373).[3]

But then from what does the model derive its explanatory power, in B&R's view? I argued in the previous section that its explanatory power cannot arise from its belonging to the same universality class as the target system together with its "computational ease" (364). Sometimes B&R write as if the model's role were merely to enable us to figure out what macrobehavior is exhibited by all of the members of the universality class: "Why does a fluid flowing in a pipe exhibit a parabolic momentum profile? Answer: the particular fluid and the LGA model are in the same universality class, and by analyzing LGA we can see that all fluids within this universality class will exhibit this profile" (364). But if the model's only role is to enable us to figure out what macrobehavior is exhibited by members of the universality class, then the responsibility for the target system's behavior would seemingly lie solely with the target system's membership in the given universality class. In that case, B&R's account would apparently say that the given fluid's macroscale behavior is explained by its possessing the

3. B&R (374–75) quote Michael Fisher (1983, 46–47) as supporting their view of how minimal models explain. But after the quoted passage, Fisher (1983, 47) says, "We may well try to simplify the nature of a model to the point where it represents a 'mere caricature' of reality. But notice that when one looks at a good political cartoon one can recognize the various characters even though the artist has portrayed them with but a few strokes. . . . [A] good theoretical model of a complex system should be like a good caricature: it should emphasize those features which are most important and should downplay the inessential details." Fisher here seems to be supporting a "common features account": the minimal model, despite being a caricature of some actual system, shares with it "those features which are most important."

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property of being such that it is brought to a certain fixed point in the state space (the same point for every member of the universality class) when it repeatedly undergoes a certain transformation "that has the effect, essentially, of eliminating certain degrees of freedom by some kind of averaging rule" (362). Since this property is common to all members of the universality class, it constitutes a "common feature" of the kind that B&R deny explains the system's macrobehavior. It seems to be no less a common feature than locality, conservation, and symmetry.

Yet B&R seem distinctly cool to the suggestion that locality, conservation, and symmetry (and also, presumably, the property I have just mentioned) could be "common features" for the purposes of explanation: "We think it stretches the imagination to think of locality, conservation, and symmetry as causal factors that make a difference to the occurrence of certain patterns of fluid flow" (360). Of course, this remark is correct if it means only that locality, conservation, and symmetry are not causes; after all, they are not even events. But this remark is incorrect if it means that these features of the world's causal structure (in particular, of the laws governing the causal interactions involved in fluid flow) cannot causally explain fluids' macrobehaviors (as a "common features account" says they do). When these explanations employ minimal models, the models work by virtue of sharing these "common features" with the target systems, contrary to B&R's claim that "what accounts for the explanatory power of this model is not that it correctly mirrors . . . the real systems of interest" (364).

Plenty of causal explanations appeal precisely to such properties to explain macrobehaviors. (Like the explanations that B&R discuss, these explanations abstract from microcauses and cover microcausally diverse systems.) For instance, let our explanandum be the fact that when something diffuses through a homogeneous, boundless, two-dimensional medium after having been released at the origin (0, 0) at time $t = 0$ (so the total finite quantity of the diffusing substance begins infinitely concentrated[4]), at every subsequent time t the concentration curve $\Phi(x, y, t)$ is a Gaussian (i.e., bell-shaped) curve—in other words, the concentration is proportional to $e^{-(x^2+y^2)/c}$ for some constant c. John Herschel (1850, 19–20) used various symmetries in the microcausal laws to account for the concentration function. Here is a sketch of Herschel's explanation:

> The concentration function is proportional to the function giving the likelihood of a diffusing parcel's being at a given location. A parcel's likelihood of having managed to make its way to some point or other with a given x-coordinate is equal to its likelihood of being then at some point or other with the negation of that x-coordinate. Therefore, the likelihood of

4. Its initial distribution is standardly given by a Dirac delta function localized at the origin.

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its being at a point with an x-coordinate between x and $(x + dx)$ is dx times some probability density function $f(x^2)$ so that the likelihood is equal whether x is positive or negative. The likelihood function is rotationally symmetric and so the same for the y axis. Hence, the same f figures in the likelihood of a parcel's being at a point with a y-coordinate between y and $(y + dy)$: $f(y^2)dy$. Given the time, a parcel's being at a point with a given x-coordinate is statistically independent of its being at a point with a given y-coordinate, so the likelihood of its being at a point with an x-coordinate between x and $(x + dx)$ and a y-coordinate between y and $(y + dy)$ is the product of their separate likelihoods: $f(x^2)\,f(y^2)dxdy$. But a location (x, y) that is not on either axis can equally well be taken to lie at the coordinates $(0, [x^2 + y^2]^{1/2})$ along a pair of axes rotated relative to the x and y axes, and since the likelihood function is rotationally symmetric, we can apply the same formula to these new axes, so the likelihood of the parcel's being at this location is $f(0)f(x^2 + y^2)dxdy$. Therefore, $f(x^2)\,f(y^2) = f(0)\,f(x^2 + y^2)$. Any function meeting this condition must take the form $f(x^2) = Ae^{cx^2+d}$.[5] Since the total probability must add to 1, the constant c must be negative. Hence we arrive inevitably at a Gaussian distribution.

Herschel originally used his argument to explain why the results of skillful archers aiming at a target form a Gaussian distribution. Later Maxwell (1860/1890, 377) used the same argument to explain why the velocity distribution of the molecules in an ideal gas forms a Gaussian distribution, remarking that whether a case involves archery errors or diffusion or ideal gas molecular velocities, "mathematical expressions of this exponential form are sure to make their appearance" (Maxwell 1871, 289).

In apparently appealing to, for example, Φ's rotational symmetry, Herschel's explanation is actually appealing to the fact that the laws governing the microcausal processes underlying diffusion (such as molecular collisions) privilege no direction. Likewise, when Herschel's derivation apparently appeals to the parcel's x-coordinate being statistically independent from its y-coordinate, Herschel is in fact appealing to the axes' independence in the underlying causal processes—as in the vector addition of forces. Similarly, in appealing to the fact that a parcel's likelihood of having a given x-coordinate is equal to its likelihood of having the negation of that x-coordinate, Herschel is appealing to features of the initial velocity distribution of the diffusing particles, the initial state of the diffusing medium,

5. Here is a simple proof. We have $f(a + b) = f(a)f(b)/f(0)$. So $f'(a) = \lim_{b\to 0} \frac{f(a+b) - f(a)}{b} = \lim_{b\to 0} \frac{\frac{f(a)f(b)}{f(0)} - f(a)}{b} = \lim_{b\to 0} \frac{\frac{f(a)f(b)}{f(0)} - \frac{f(a)f(0)}{f(0)}}{b} = \frac{-f(a)}{f(0)} \lim_{b\to 0} \frac{f(0) - f(b)}{b} = \frac{-f(a)f'(0)}{f(0)}$. Therefore, $f'(a) = kf(a)$ for some constant k. Therefore, $f(a) = Ae^{ka + d}$ for some constants A and k.

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and the laws governing the microcausal processes. In short, the various formal features to which Herschel's derivation appeals pertain directly to the microcausal processes underlying diffusion and in this way derive their explanatory priority over Φ's exponential character: the explanandum is an effect of causes described by the explanans.

These symmetries seem perfectly eligible to be "common features" to which a minimal model explanation might appeal. Like B&R's minimal model explanations, Herschel's explanation applies to many microphysically diverse systems, presenting a simple mathematical account that omits a host of petty microinfluences.[6] Admittedly, B&R never claim that the only way to explain the macrobehaviors common to microphysically diverse, messy systems is to provide the kind of "minimal model explanation" that B&R describe, and (despite calling the kind of explanation they describe "minimal model explanations") B&R never say that when "minimal models" explain, they always do so through the "minimal model explanations" that B&R describe and never by virtue of the "common features" that the models share with the target systems. However, it is not clear why the "common features account" can apply to Herschel's explanation but not to all of B&R's examples where minimal models explain.

In addition, B&R could concede that locality, conservation, and symmetry are "common features" for the purposes of explanation, but that the same cannot be said regarding the system's property of being such that it would be brought to a certain fixed point in the state space under repeated application of a certain transformation. B&R might suggest that this property (unlike symmetry) is not represented in the model but only arises when we apply the transformation. I disagree: both are simply properties of the model. Even before the model undergoes the transformation, the model has the property of being such that the transformation would bring it to the fixed point. Of course, the most direct way to ascertain that the model has this property is to see what happens to it under the transformation. But the same holds for a symmetry, which consists of invariance under a given transformation.

5. An Objection to B&R's Criticism of "Common Features Accounts." As we have seen, B&R say that among the "shortcomings" (373) of "common features accounts" is that these accounts incorrectly regard minimal model explanations as not having to explain why the common features are necessary for the macrobehavior being explained and why those features are

6. This leads to the quip about the Gaussian distribution of errors that Poincaré (1908, v) attributes to an eminent physicist: "Everybody firmly believes in it because the mathematicians imagine it is a fact of observation and observers that it is a theorem of mathematics."

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present in all members of the universality class despite their heterogeneous microdetails. Notice, however, that Herschel's explanation succeeds despite supplying neither of these explanations.

For instance, Herschel's explanation fails to show that in order for the explanandum to hold, the laws governing the microcausal processes underlying diffusion (such as molecular collisions) must privilege no direction. Presumably, the symmetries Herschel cites can help to explain why the concentration curve is Gaussian even if those symmetries are not necessary in order for the concentration curve to be Gaussian. Herschel's explanation succeeds without assuming that the laws' rotational symmetry is necessary for the macrobehavior being explained, and so perforce Herschel's explanation succeeds without explaining why the laws' rotational symmetry is necessary for that macrobehavior.

Furthermore, Herschel's explanation certainly fails to explain why the laws governing the microcausal processes underlying diffusion have certain symmetries. Nonetheless, the explanation succeeds.[7]

Why, then, do B&R insist that a minimal model explanation must explain why the common features are necessary for the macrobehavior to occur and why these features are present in all members of the universality class despite their heterogeneous microdetails?[8] B&R write, "In the LGA model we wanted to explain various continuum behaviors exhibited by different fluids. 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" (374). Of

7. Analogous points apply to other explanations that work through models that abstract from microdetails by giving caricatures that focus only on the major influences. For instance, MacArthur and Wilson's equilibrium theory of island biogeography appeals to such facts as that smaller islands offer smaller targets to off-course creatures and that a creature will find it more difficult to traverse a longer than a shorter distance of inhospitable terrain. But it explains none of these facts. For more on MacArthur and Wilson's explanation, see Lange (2002).

8. B&R could reply that their account applies only to explanations where the "common features" are necessary for the macrobehavior being explained and where the audience is interested in knowing why those common features are necessary. But this restriction would considerably limit the scope of B&R's account (so that many explanations involving "minimal models" would fall outside of it). In addition, this restriction would preclude B&R from regarding it as a "shortcoming" of "common features accounts" that what those accounts regard as explanations do not automatically explain why those common features are necessary. There is no reason why "common features" explanations could not be coupled with explanations of why those common features are necessary (when they are in fact necessary and the audience is interested in knowing why they are).

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course, it is perfectly legitimate to ask why the different fluids all possess "common features" such as some symmetry. But a proposed explanation of fluid behavior by some symmetry is not rendered void or incomplete just because it provokes but fails to answer the question of why the symmetry obtains. Oftentimes an explanation of G by F will lead us to wonder why F obtains. However, that the answer to one why question leads us to ask other why questions "is a psychological matter, and the problems thus raised are new explanatory problems" (Hempel 2001, 335). "Why F?" is not the same question as "Why G?" so an answer to the latter can appeal to F without answering "Why F?" As Hempel emphasized, "Why F?" is a "new explanatory problem."

Apparently, B&R disagree: "citing the features the different systems have in common is just to ask the very question we are trying to answer" (375). This is not the case. Suppose we ask, "Why is there a 1:1 sex ratio in a given population?" Suppose we answer this question by giving R. A. Fisher's famous explanation (which B&R cite as an exemplary minimal model explanation) and thus by appealing to facts such as that in the given population, sons and daughters cost the same amount of resources to produce. Suppose we then ask why they cost the same amount. To ask this question cannot be "just to ask the very question we" began with, namely, "Why is there a 1:1 sex ratio in a given population?" The latter question (as B&R emphasize) asked about different populations has the same answer, whereas the former question asked about different populations may well have different answers. So they cannot be the same question.[9]

6. Conclusion. I have objected to B&R's account of how "minimal model explanations" work and why "common features accounts" of these explanations fail. B&R have not succeeded in undermining the view that minimal models derive their power to help explain the macrobehavior of target systems by possessing certain of the target systems' features without others and thereby presenting in isolation the major influences at work in the target systems.

9. Suppose that the explanandum is not the fact that a given target system exhibits a given macrobehavior, but rather the fact that the macrobehavior is exhibited by all systems in a given category, despite their microheterogeneity. Even then, the common features cited by the explanans need not be necessary for the macrobehavior to occur (and so the explanation need not explain why they are necessary), and furthermore the explanation need not explain why these features are present in all of the systems in the given category. Once again, Herschel's explanation is a nice illustration. That certain symmetries are common to the mechanisms behind archery errors, diffusion, and ideal-gas molecular velocities explains why a Gaussian distribution is also common to these microphysically diverse cases. Herschel's explanation does not (need to) account for the symmetries or show them to be necessary for a Gaussian distribution.

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