Humean Metaphysics and the Philosophy of Science

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The candidate confirms that the work submitted is his own and that appropriate credit has been given where reference has been made to the work of others.

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Abstract

Humeanism is often taken to be a prime example of metaphysics which has failed to be sufficient attention to contemporary science. I argue that these claims have been made too hastily: there are moves available to Humeans which bring the account closer to scientific practice while still preserving the spirit of the view. The thesis comprises two parts. In the first half, I deal with the Best System Account of laws, and consider how it ought to treat initial conditions. From there, I turn to the question of whether Humean laws can explain events. This has recently been a topic of renewed interest in the literature and I disentangle the various claims philosophers have made on behalf of Humeanism. From these, I identify three promising responses to the argument that Humean explanations are circular. In the second half of the thesis, I consider how the Humean approach to laws can be extended to cover symmetry principles when the latter are understood as laws of laws. In response to a problem concerning properties and language, I suggest that the account go language-relative. The result of this is a regularity-based approach that can incorporate both laws and their symmetries into a single unified framework. Finally, I draw upon some examples from biology to indicate how the account can deal with the special sciences.

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Introduction

> As philosophy is essentially a meta-activity, there can be philosophies of just about anything, and science is no exception … It [philosophy of science] has enjoyed a boom in the last 50 years or so, partly because it gives philosophers the unfamiliar sensation that what they are doing is of some relevance to something.[1]

This thesis works at the intersection of metaphysics and the philosophy of science. The area is something of a minefield: specialists from both of these areas of philosophy have been guilty of drawing battle lines around their respective subjects and declaring the middle ground to be a no man's land. On the one hand, we find philosophers of science taking metaphysicians to task over a lack of scientific engagement. They accuse metaphysicians of indulging in debates that are sterile or empty, of ignoring parts of science clearly relevant to their projects and, on those occasions where metaphysicians remember to throw in a scientific reference, of using only radically simplified 'domestications' of science. In their influential critique, Ladyman and Ross draw a clear lesson from these failings, claiming that 'contemporary analytic metaphysics, a professional activity engaged in by some extremely intelligent and morally serious people, fails to qualify as part of the enlightened pursuit of objective truth, and should be discontinued'.[2] There are echoes of Hume here, and his insistence that such works of sophistry and illusion ought to be cast into the flames.

In the face of such criticisms, some metaphysicians have dug their heels in and doubled down on what they have already been doing. This sentiment is nicely brought out – although not entirely endorsed – by Conee and Sider:

> The revised view is that metaphysics is about the most explanatorily basic necessities and possibilities. Metaphysics is about what could be and what must be. Except incidentally, metaphysics is not about explanatorily ultimate aspects of reality that are actual. Metaphysics is about some actual

[1] Hankinson (1985) p. 40.
[2] Ladyman and Ross (2007) vii.

> things, only because whatever is necessary has got to be actual and whatever is possible might happen to be actual.[3]

Metaphysics, on this view, really has separated itself from the practice of science. But this is a justified separation, since the two subjects are concerned with different areas. It is no surprise that metaphysicians pay little attention to the findings of science, since their concerns are far broader than those of science (which is concerned merely with what is actual).

I am not alone in thinking that the space between metaphysics and the philosophy of science is not so much a terrifying gulf as rather fertile land. In Callender's pleasing slogan, 'metaphysics is best when informed by good science, and science is best when informed by good metaphysics'.[4] French and McKenzie take a broadly similar view: for them, metaphysics serves as a toolbox for philosophers of physics seeking to understand fundamental physics.[5] Of course, committed metaphysicians will object to this characterisation and its suggestion that metaphysics only gains value through its utility to philosophers working on the *real* issues! Perhaps they might even reverse the metaphor: philosophy of science has a use beyond merely explaining the findings of science when it serves as a muse to metaphysicians studying what could be the case. Ultimately, the metaphor is not important. The view I advocate in this thesis is that there is no sharp distinction between the areas of metaphysics and the philosophy of science, but rather that there is considerable overlap (the literature engaging with theories of time and space is a clear example of this). From that perspective, thinking of oneself as a philosopher of science plundering metaphysics, or vice versa, is simply unhelpful. Members of both disciplines have a shared interest in what there is and what it is like; the primary difference in the work is its engagement with empirical data.

While I have taken to calling this thesis a work of metaphysics informed by the philosophy of science, it might just as easily be thought of as philosophy of science from a metaphysical angle. There is little in the name here. What it aims to do is

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[3] Conee and Sider (2005) p. 236.
[4] Callender (2011) p. 48.
[5] French and McKenzie (2012).

tackle issues relating to laws in a way that is both metaphysically and scientifically informed. I take giving an account of laws to be one of the aforementioned areas of overlap and so this is a good opportunity to show how these different kinds of considerations can be incorporated into a single view. Of course, the available literature on laws is far too large to attempt to do justice to every interpretation.[6] For this reason, I have chosen to focus on Humeanism and what it has to say about lawhood. This is in part due to my philosophical leanings: while I may not be an uncompromising adherent, I am certainly strongly sympathetic. But it is also partly due to an interesting tension within the account. Humeans, much like Hume, tend to be respectful of science. Thus, we find a suspicion of there being metaphysically inexplicable entities doing any work; there are no idle metaphysical wheels and there is nothing going on behind the scenes. What you see (via science) is what you get. Yet Humeans are often first in the firing line when philosophers of science get restless. Sometimes the charge is that they have failed to realise that the world is not classical. Other times it is that they cannot adequately capture the practices and proclamations of scientists. Either way, Humeanism is supposed to serve as an example of how not to do metaphysics.

There are two main aims in this thesis. The narrower aim is to reconcile Humeanism with the relevant philosophy of science. Through examining some of the apparent conflicts with scientific practice, I will draw out what I take to be the most promising options available for scientifically inclined Humeans to proceed. This feeds into the broader aim: to contribute towards a more productive engagement between metaphysics and the philosophy of science. The thesis is intended as an example of the benefits that result from doing the ‘right’ kind of metaphysics.

The thesis has two main parts. The first half is concerned with the usual Humean account of laws and is spread over two chapters. In chapter one, I begin with some scene-setting. Humean accounts of law are seldom held entirely separate from other metaphysical commitments. At the very least it would be surprising if the laws had nothing to do with non-Humean entities like fundamental dispositions should those

[6] An accessible overview is provided by Demarest (2015).

entities be available for use! For this reason, it is worth locating the account within the wider Humean framework which includes, most notably, the doctrine of Humean Supervenience. Given the considerations above, that's a somewhat controversial doctrine to appeal to – not for nothing is it held up as a prime example of scientifically disinterested metaphysics! While the matter is not nearly as black and white as is often presented, I restrict myself to only indicating how Humeans have attempted to develop it rather than diving into which development is best (this is for the very simple reason that I think a full defence of a suitably modified Humean Supervenience doctrine requires a thesis in itself). With a broad overview of the mosaic in place, I turn to the reduction of laws to regularities and the Best System Account. This leads into the interesting question of what to do about initial conditions: the original account says very little about this aspect of scientific practice and I use the opportunity to examine the modifications we might make to give them their proper place.

In chapter two, I tackle a long-standing objection to regularity accounts of laws. The basic idea is straightforward: if the laws are just patterns in the underlying mosaic, how can they explain the mosaic? As the explanation of phenomena by laws is a role assigned to them by scientists, this is another example of the way in which Humeans are supposed to have failed to properly observe scientific practice. But while the question can be stated simply, getting a good philosophical argument out of it is less easy. I am influenced here by Marc Lange's recent writings on this topic and his placing of a principle of transitivity as the central issue. Perhaps because this objection is not often laid out in detail, some responses to it have fallen short. After examining these and showing that they merely motivate a stronger circularity argument, I argue that there are three viable replies Humeans can make. At this point, selection of a reply becomes a matter of looking at one's prior commitments and then weighing the associated costs. While I favour one response, as I believe that it sheds some light on how Humeans ought to be thinking of explanations, I recognise that not all Humeans need agree with this. The important thing as far as the thesis is concerned is that the regularity account is not dead in the water. Despite the popularity of the claim that Humeans cannot make sense of scientific explanations, there are options available here.

The second half of the thesis, consisting of chapters three and four, looks beyond the familiar Best System Account. Chapter three introduces symmetry principles and indicates how these are connected to laws in modern physics. There is something of a lacuna here in most discussions of Humean laws. While symmetries play a distinguished role in scientific theorising and are often viewed as a kind of constraint on the form that the laws can take, metaphysicians have said little about how these are related to the Best System Account. Marc Lange's work is drawn upon again, and I pursue his suggestion that we conceive of symmetry principles as metalaws, or laws of laws. Viewed in this way, the existence of symmetries motivates a move to a second-order Best System Account that captures the patterns in the patterns in the mosaic. This provides an opportunity to discuss their role in areas that laws are often appealed to, such as counterfactual reasoning. Despite a promising start, however, I show that there remains a problem for this extension of the Humean account. The Best System Account is tied to a particular choice of vocabulary, one that does not mesh well with the systematisation of metalaws.

Chapter four lays out what I take to be the best response to this language problem. A similar issue arises for Humeans seeking to capture the laws of the special sciences, and I suggest that the solution is the same in both cases. If Humeans are willing to adopt a permissive stance towards the language of the best system, then the Better Best System Account can be extended in the way suggested in chapter three to allow for both nonfundamental laws and metalaws. Once the position has been laid out, I then put it to work. Its first use is to respond to the complaint from the philosophy of science that symmetries represent exactly the kind of constraints or necessary connections that Humeans are unable to stomach. Not so, I reply, for under the account that has been developed they are no more problematic necessities than the usual laws are. Reading off metaphysical conclusions from science is more difficult than that. The second use is directed towards the special sciences, using biology as my example. I claim that the account helps Humeans to say what marks out the laws of biology as different from those of physics by translating talk of universality into talk of higher-order regularities that hold over the laws. I also suggest that there looks to be rough biological equivalents to the symmetry principles of physics, with a principle of natural selection being the most obvious example. I conclude that, with suitable modifications, the Humean account of laws can both cleave more closely to scientific practice and resolve some metaphysical issues regarding how best to understand those practices.

This thesis is situated within a large area of philosophical work and there are inevitably interesting issues that I do not discuss. Some of these are indicated in the text and typically remain unexplored on account of being tangential to the main thrust of the chapter in which they are located. I have not attempted to do justice to the full range of Humean positions here, although I have tried to indicate the options open to different accounts. It will quickly become apparent how much influence the work of David Lewis has had on this thesis. As such, the Humeanism that I am considering is very often a Lewisian Humeanism. I make little apology for this. While there are many ways in which one could part ways with Lewis while remaining recognisably Humean, his work has had such a large impact on this area of philosophy that it is difficult to engage in a discussion of the regularity account of laws without paying tribute. As Humeanism's most famous modern defender, he has both set down the closest thing we have to a canonical account and in many ways determined the agenda for the ensuing debates.

A more fundamental question that I do not tackle is what justifies or motivates a Humean philosophy in the first place. There are, of course, almost platitudinous things that could be said here. I might express a desire to do without the sort of metaphysics that looks mysterious to Humean eyes, like the notion of governance in connection with laws. Alternatively, I might appeal to Occam's razor and protest that there is no need to introduce non-Humean entities to give an account of the world. But I suspect that those who ask 'Why be Humean?' will remain unsatisfied.[7] (If one is sceptical of the success of the Humean project, then neither of these claims look to amount to much.) Aside from considerations of parsimony and empiricist leanings, I think that the main reason for adopting Humeanism is in its aesthetic value. In Quinean fashion, Humeans are often lovers of desert landscapes and find attractive the idea of accomplishing much with very little. It is appropriate, given the

[7] As does Maudlin (2007a), who takes this as the title of his second chapter in a plea for enlightenment.

aim of this thesis, that we find a similar preference in physicists. One need only recall Einstein’s famous disdain for ugly equations or search for books on physics whose titles mention beauty to find evidence of this. If what I suspect about the basic Humean motivation is true, then Humeans are in good company.

Chapter 1 Humeanism

To properly discuss the Humean approach to laws, it is necessary to first set out exactly what we will be taking its claims to be. That is the purpose of this chapter: to lay the foundation for the later modifications and extensions. I begin with some background to the view by placing it within the wider 'Humean package', the most widely adopted statement of which is Lewis'. One does not need to adopt every Humean view to regard laws in a Humean way, but the wider project is important nonetheless. After discussing Humean Supervenience, I turn to the regularity view of laws. While I am primarily concerned with setting out the canonical account, this is not done in an uncritical way. There are aspects of the view that need further work, such as how best to understand the theoretical virtues it appeals to. After providing some preliminary thoughts on this substantial project, I consider a substantive modification to the Humean account suggested by Ned Hall, among others, to distinguish between the best system's dynamic laws and initial conditions. I set out what changes might need to be made to do this, and then look at how this sort of split interacts with the usual standards by which a system is judged to be the best.

1.1 Humean Supervenience

The primary focus of this thesis is the Humean account of laws. Those who are Humean regarding laws are often Humean in other metaphysical matters too. It is worth, therefore, doing some scene-setting at the outset to give a more complete picture of both contemporary Humeanism and the sorts of claims that friends of Humean laws are sympathetic to. The obvious place to start is with Humean Supervenience, the central project that so much of David Lewis' work was a defence of. The following statement of it is by now something of a classic:

> Humean supervenience is named in honor of the greater denier of necessary connections. It is the doctrine that all there is to the world is a vast mosaic of local matters of particular fact, just one little thing and then

> another. (But it is no part of the thesis that these local matters are mental.) We have geometry: a system of external relations of spatiotemporal distance between points. Maybe points of spacetime itself, maybe point-sized bits of matter or aether or fields, maybe both. And at those points we have local qualities: perfectly natural intrinsic properties which need nothing bigger than a point at which to be instantiated. For short: we have an arrangement of qualities. And that is all. There is no difference without difference in the arrangement of qualities. All else supervenes on that.[1]

Humean Supervenience is one way of spelling out the claim that truth supervenes on being. More specifically, it sets out a collection of fundamental entities which serve as a base that all other facts about the world supervene on. It is a physicalist position in that it is the world's fundamental physical state that the truths are supervenient upon. This fundamental physical state is comprised of perfectly natural properties and relations. The properties are intrinsic and are supposed to serve the joint-carving role: possession of perfectly natural properties marks out genuine objective differences between things (as opposed to the sort of 'similarity' one gets when two objects possess especially disjunctive or grue-ish properties).[2] To find what these fundamental properties are, we look to physics or perhaps some future development of that science. Physics might not be equipped to discover all of the perfectly natural properties that exist simpliciter, since there seems to be little way for actual physicists to study perfectly natural properties that are alien to our world. But Lewis endorses the empiricist claim that physics is the best way of finding those properties that are actual. Philosophers tend to use the term 'mosaic' in slightly different ways to one another. Some of this is due to inequivalent formulations of what they take Humean Supervenience to be. Some of this is due to how strictly true they take the thesis to be: those who take a more permissive stance will be happy to call a fundamental base the mosaic if it is appropriately Humean, even if it contains elements that Lewis' original formulation did not. I will be adopting such a permissive approach in this thesis. I will be taking 'mosaic' to refer to the underlying fundamental base, so long as it does not contain anything that violates the spirit of

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[1] Lewis (1986a) ix-x.
[2] Although the claim that they are instantiated at points makes for trouble with vectors. See Butterfield (2006) for suggestions on how to modify the thesis.

the Humean campaign.[3] Attempting to stick more strictly to the official line strikes me as a mistake. As will be discussed shortly, there are reasons to think the original account needs modification, and we even see acknowledgement from Lewis that some of the precise claims are really not all that important.

We might expect Humean Supervenience to be a supervenience claim. If so, it would look something like the claim that for any two worlds (that are appropriately like our own), if their mosaics are the same then all their other facts are the same.[4] The world is something like a television screen: we look at it and see sequences of picture – meaning composite objects and patterns of them. But underlying these pictures on the screen, there is just an arrangement of coloured pixels. One cannot have a change in the picture without there being a change in the pixels. Similarly, one cannot have a change in the large-scale patterns that we observe without there being a change in the fundamental properties.

While this interpretation of Humean Supervenience is supported by various passages in the work Lewis has published on it, there are also some issues with it. First, Humean Supervenience is supposed to be a contingent thesis. This is its empiricist character coming to the fore: it is the job of science, and physics in particular, to determine whether Humean Supervenience is true at our world. It is not the stronger claim that there is something about the relevant metaphysics that prevents other worlds from having different supervenience facts. On the contrary, we are told that some worlds have emergent natural properties and so cannot have all of their truths supervene on a base like ours. So characterising its claims in terms of other possible worlds runs into immediate difficulty: supposing that some other world has a mosaic like our own but is not a world where Humean Supervenience holds (perhaps there are properties instantiated at it that are not perfectly natural but nevertheless are in no way connected to those that are), it will not have all the same contingent facts as

[3] This is somewhat vague, since I am not attempting to specify what this spirit is. Vague, but not disastrously so since I take it that we can still get a grasp on what counts as Humean even without explicit definitions.
[4] Roughly, a world is like ours in this sense if it has no alien perfectly natural properties.

ours. That said, perhaps there are ways around this; we are only working with a rough definition after all.

A more serious problem is that supervenience is not an asymmetric relation. Neither is it symmetric, for it is nonsymmetric. The fact that the *A*s supervene on the *B*s does not by itself rule out the *B*s supervening on the *A*s. We should be concerned by attempts to characterise the fundamental as a supervenience base, since the base in question might supervene on the nonfundamental stuff too. The surface area of a cube supervenes on its volume, but it would be odd to therefore claim that a cube's area is somehow more fundamental than its volume. As the volume also supervenes on the area, we would end up committed to both the volume and the area being more fundamental than each other. This suggests that something stronger than supervenience is going on. The usual replacement candidates are the likes of reduction, dependence, ontological priority and grounding. This is not the place to undertake the substantial question of which notion fits the Humean project best. Instead, we shall have to be content with a rough and ready characterisation of the position and an acknowledgement that there is still important work to be done on exactly what the relationship is between the mosaic and the nonfundamental elements of the world. Fortunately, this is a promising affair as there exists a range of resources in the metaphysical literature available for plunder by the Humean.

One might wonder why it is that Lewis did not adopt one of these other notions instead of the oddly weak supervenience. That's not an easy question to answer given that he is not available to ask, and that this work is not intended to be an exegetical one. Some suggestion is given in a passage where he takes the supervenience claim to be broadly reductionist, but notes that it is 'a stripped-down form of reductionism, unencumbered by dubious denials of existence'.[5] That fits with the generally commonsensical approach Lewis took; it is unlikely he would have wanted to deny that tables and chairs *really exist*. But it is perhaps also overly cautious, since the usual fundamentality relations do not need to be interpreted in that way. Grounding, for example, is not typically associated with the elimination of

[5] Lewis (1983) p. 358.

the grounded entities. I take contemporary Humeans to be thinking of something stronger than supervenience when they declare their allegiance to the cause, but there is still room for a weaker version of the account that sticks more closely to Lewis' original writings.

It is worth saying a few words about the elephant in the room: quantum physics. It is not unusual to find philosophers of science chastising Lewis and Humean Supervenience for a failure to pay enough attention to actual twentieth-century physics.[6] This is a bit of an easy target for those who have anti-metaphysical leanings and an axe to grind. Lewis admitted outright that this picture of the world was inspired by classical physics, so it is little surprise that it is better suited to a classical world than a quantum one. If it were just the source of inspiration that was falling behind the real science, this might not be the worst problem to have.[7] But the objection is much stronger than that: we are supposed to look to physics to discover whether Humean Supervenience is a true description of the world, and it turns out that physics has said 'no'. So why care about a philosophical position that has been empirically refuted? Let us briefly rehearse the argument from quantum theory and then consider what Humeans might want to say in reply. I focus here only on quantum mechanics and ignore further developments like quantum field theory. That is not because those developments are completely unproblematic, but because the debate thus far has largely focused on quantum mechanics alone.

While single particles present no particular issues for Humeans, systems of multiple interacting particles do. Or, at least, some do. Systems whose joint state can be represented in the form of product states create no issues. Product states assign a determinate value for some property to each of the particles in that system. As such, the composite state is separable: it can be reduced to a combination of the states of the individual particles. However, there are states that are not factorizable in this manner. The classic example is of two electrons prepared in the Singlet state. If we wish to make a spin measurement on one of the particles, we cannot be certain in

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[6] Maudlin (2007a) is a clear and forceful statement of this kind of anti-Humeanism.
[7] Given Einstein's famous lack of comfort with some of the advances made by quantum physics, we might even have been willing to say that Lewis was in good company!

advance whether it will be measured as spin up or spin down with respect to the measurement direction. Instead, we must assign a 50% probability to each of the two possible outcomes. But once we have measured the spin value of one of the electrons, we can now be certain what the spin value of the second electron is in that direction: it will be the opposite of the first's. The state of the particles is antisymmetric in this way. This statistical behaviour holds true for any direction of measurement, each electron has an even chance of being up or down with respect to that direction and the second electron will always have opposite spin to the first. The problem for Humeanism is that there is no pure single particle state that shows the same statistical behaviour as this. So the entangled state of the electrons cannot be factorised into product states.[8] This suggests that the behaviour is emergent in the sense that it cannot be 'broken down' into properties ascribed to each electron individually. But Humean Supervenience appears to require that this happen: the mosaic is made up of *local* matters of particular fact. Hence there is an apparent conflict with modern physics.

There are various ways the Humeans can respond to this challenge, although none comes without a cost. First, one might attempt to simply ignore the problem. As odd as it sounds, this head-in-the-sand approach was suggested by Lewis:

> Really, what I uphold is not so much the truth of Humean supervenience as the *tenability* of it. If physics itself were to teach me that it is false, I wouldn't grieve.
>
> That might happen: maybe the lesson of Bell's theorem is exactly that there are *physical* entities which are unlocalized, and which might therefore make a difference between worlds – worlds in the inner sphere – that match perfectly in their arrangements of local qualities. Maybe so. I'm ready to believe it. But I am not ready to take lessons in ontology from quantum physics as it now is. First I must see how it looks when it is purified of instrumentalist frivolity, and dares to say something not just about pointer readings but about the constitution of the world; and when it is purified of doublethinking deviant logic; and – most of all – when it is purified of supernatural tales about the power of the observant mind to make things

[8] I have just been considering pure states here for ease of exposition. Impure states are generally not thought to provide any assistance to Humeans here.

> jump. If, after all that, it still teaches nonlocality, I shall submit willingly to the best of authority.[9]

Of course, the critics are quick to point out that all of this has already been done. There are interpretations of quantum mechanics that satisfy Lewis' requirements but, in assigning some form of physical interpretation to the wave function, still violate the kind of separability required for Humean Supervenience to function. Some of these interpretations were available while Lewis was still working in that area! Writing later, Lewis repeated what he took to be the focus of this work:

> The point of defending Humean Supervenience is not to support reactionary physics, but rather to resist philosophical arguments that there are more things in heaven and hell than physics has dreamt of. Therefore if I defend the *philosophical* tenability of Humean Supervenience, that defence can doubtless be adapted to whatever better supervenience thesis may emerge from better physics.[10]

So there is a sense in which the criticisms from the anti-Humean philosophers of science are misdirected. Lewis was never trying to provide a philosophical interpretation of our current science or to read off what it says about the way the world is. Using him as an example of a metaphysician who misinterprets physics is wrongheaded: one cannot misinterpret a subject that one is not trying to interpret in the first place. That said, there is still something to those objections. While defending physicalism is a noble endeavour, why conjoin that defence to a metaphysical account that looks to be false? It would be better if the defence were accompanied by a true account, or at least one that cleaved more closely to actual physics. Resisting philosophical arguments that we need necessary connections or nonlocal fundamental properties is all well and good, but if we are then required to add them in our ontology by physics, we might well wonder whether we have been wasting our time.

If we take the challenge from physics more seriously, we might instead offer an interpretation of quantum mechanics that fits better with the motivation behind Humean Supervenience. One way to do this is to turn to the de Broglie-Bohm pilot

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[9] Lewis (1986a) xi, italics in original. We might note that the lesson Lewis associates with Bell's theorem is normally taken to encapsulate the consequences of entanglement.
[10] Lewis (1994a) p. 474, italics in original.

wave interpretation. As is well known, Bohmian mechanics is a hidden variables interpretation of the quantum theory. On one way of setting up the view (leaving aside older presentations that involved a quantum potential), our ontology consists of a configuration of *N* particles with determinate positions and trajectories. The positions of the particles change over time according to a guidance equation, which relates the particle velocities to the wave function. The wave function evolves according to Schrödinger equation in the usual way. All of this evolution is deterministic, probabilities only enter the picture due to our epistemic ignorance. So far so classical one might think. But we should remember that the interpretation is still nonlocal as the guidance equation relates the velocity of each particle to the behaviour of every other particle in the system, no matter how distant.

To see whether this ontology is appropriately Humean, we must consider the particles and wave function in some more detail. If the change in the particle positions is related to the wave function, is the wave function acting on the particles in some way? Conceiving of it as a field runs into immediate difficulty: the wave function is defined on a configuration space of 3*N* dimensions, and it is not altogether clear how a field living in such a space can interact with particles in regular space. One solution, pressed by Albert and Loewer, is to take the configuration space as the fundamental one.[11] The ontology then would be the wave function conceived of as a field and a single world-particle for it to act on (we can normally represent the N particles in 3-space as a single particle in 3*N* space, but on the Albert-Loewer suggestion that is entirely backwards). Humean Supervenience is thus preserved, since the nonlocality drops out of the theory altogether. Each point in configuration space has a (local) property giving a value for the wave function at that point, and one of the points is further privileged by being occupied by the world-particle. The cost of this approach is twofold. First, since it involves taking what is usually considered to be a mathematical representation as physical reality, it comes into tension with scientific practice. Something needs to be said about why we can collapse the distinction between representation and reality in this case, but why the distinction is a good one in other areas. Second, there is the illusion problem: our

[11] Albert (1996) and Loewer (1996).

usual experiences of a world with a fairly low number of dimensions is completely illusory since the world really has a very high number of them. Recovering our usual experiences from the exceedingly austere base is a significant challenge for the account.

Alternatively, to solve the wave function-particle interaction problem, we might wish to reconsider how we interpret the wave function. If we no longer take it to be a physical entity of any kind in a Bell-inspired fashion, then we do not need to show how entities living in different spaces can interact. The most prominent suggestion here is to give the wave function a nomological interpretation.[12] There are some differences between the various suggestions that fit into this mould. Miller, for example, explicitly appeals to Humean treatments of chance. What it is for there to be objective chances in the world on Lewis' view is for our system of laws to talk in terms of chances. Roughly speaking and in a similar vein, what it is for there to be a wave function or entanglement is for our system of laws to talk in those terms. The risk, as Miller acknowledges, is instrumentalism: we are dangerously close to saying that what it is for there to be any *X* is for our laws to talk about *X*s. The question then is where and how the line in the sand should be drawn. We might see Bhogal and Perry's suggestion as a way to respond to this worry: they look to and modify the language of the best system of laws to provide some constraints on this. The next section of this chapter introduces laws more fully. It is also worth noting that there is a reason that Esfeld calls this account 'physicalism without properties'. The physical entities in this sort of Bohmian ontology are very bare indeed. There are particles, they have positions and those positions change over time. That might well fit the letter of Humean Supervenience, but it does mean that there are considerably fewer perfectly natural properties than first thought. Properties like mass or spin are nonfundamental for Bohmians, since Bohmian particles do not have any such properties in an intrinsic way. This might not be an issue if we are only worried about ontology. But Humean Supervenience and natural properties are commonly put to a

[12] Bhogal and Perry (2015), Callender (2015), Esfeld (2014), Miller (2014).

variety of metaphysical uses; it is not clear how many of these uses survive the austerity of the Bohmian interpretation.

There is another option if we do not wish to ignore the problem but have doubts about how successful the Bohmian turn is. Confront the issue directly: the problem is that quantum mechanics looks to suggest a form of holism, where the whole fails to supervene on the features of the parts. This is commonly described as a relational holism, where the challenge is that there appear to be 'entanglement relations' that do not supervene on intrinsic properties of particles.[13] Darby has suggested the seemingly straightforward solution of simply adding such relations to the mosaic.[14]

The official Lewisian position is generally taken to be that the only perfectly natural relations are the spatiotemporal ones. This fits the mosaic metaphor: if the mosaic is an arrangement of fundamental properties, the spatiotemporal relations are how they are arranged. All other relations and patterns are formed from this arrangement. To change a mosaic, I need to either change the colour of the tile or where the tile is placed. I do not need to try to change other relational facts directly, since all of them are dependent upon the underlying arrangement. But as nicely as this fits the metaphor, we need to remember that it is just a metaphor. To draw philosophically substantive conclusions from a useful illustration would be to put the cart before the horse. After all, even if Humean supervenience is a completely true thesis, the metaphor will look strained! Different arrangements of tiles most naturally correspond to different spatial arrangements of properties. We could get time in by imagining this spatial arrangement changing over successive moments. But an important lesson to be drawn from relativity theory is that space and time are not so easily disentangled. A mosaic that differs from one moment to the next does not adequately capture the idea that the arrangement is to be spatiotemporal, as opposed to there being distinct spatial and temporal arrangements. We could instead think of the tile positions corresponding to both spatial and temporal locations, but then the result is a 'mosaic' that does not match the mosaics we are used to. The point of all of this is not that we should drop the mosaic metaphor

[13] Teller (1986) and French (1989).
[14] Darby (2012) and Darby (2015).

entirely or that Humean supervenience is incompatible with there being a spacetime. Rather, it is merely the reminder that even attractive metaphors should not limit our philosophical options.

In fact, Lewis himself was willing to consider the possibility of there being perfectly natural relations other than the spatiotemporal ones. Since he was officially neutral regarding whether there are fundamental entities that occupy the spacetime points, he left open the possibility that appropriate occupancy relations are also perfectly natural.[15] Darby's suggestion is that we treat entanglement in a similar way: physics has taught us that there is another kind of perfectly natural relation instantiated at our world. Far from refuting Humean Supervenience, modern physics has shed new light on how we should think of the mosaic, in exactly the way that it is supposed to do. The difficulty lies in showing that such relations are harmless by Humean lights. Given that the nature of entanglement relations is sometimes taken to be a motivation for ontic structural realism and its acceptance of objective modality in the world, this is less than straightforward.[16] Issues of individuality are closely connected: if a particle's individuality is closely connected to this relation (see chapter three for a brief overview), does that imply some sort of restriction on the free recombination required to generate the plurality of worlds envisaged by modal realism?

While each approach to incorporating the findings of quantum mechanics within Humean Supervenience has difficulties to overcome, I see little reason to think that these difficulties cannot be overcome. A request for further development and elaboration of the ideas involved is quite different from a demonstration that the responses are fatally flawed. I end this section on an optimistic note: the broad thesis of Humean Supervenience can be sharpened in various ways, which way looks to be most attractive will likely depend on the price one is willing to pay to be Humean.

[15] Lewis (1994a) p. 474.

[16] For example, French and Ladyman (2003) and French (2014).

1.2 Laws as regularities

Now that we have set out the mosaic that is supposed to serve as a base for reductions of troublesome notions, let us turn to one of those reductions. Laws are a central topic in metaphysics and the philosophy of science. Some of this is due to a desire to say something about what science is concerned with. Some of it is due to the uses that laws are often put to: they are commonly taken to be connected to causes, chances and counterfactuals. Shedding some light on the nature of the laws will hopefully help to illuminate these other areas.

If one wishes to stick to the Humean denial of necessary connections, then something will need to be said about the traditional roles that laws are supposed to fill. Laws are normally thought to hold necessarily, to constrain what is possible and to govern the worldly goings-on. Of course, none of this can be taken at face value since these roles appear to require some form of unreduced modality. Humeanism about laws involves rejecting that sort of ordinary thinking, typically because it is thought to be unreflective or insufficiently explanatory. Governance of the phenomena makes sense if we think of the world as created and maintained by divine will. The laws might enforce God's will upon the world, and it is in virtue of God's nature that the world must oblige. Such theistic musings have little place in contemporary science, and Humeans can claim with some force that if something must govern in that way in order to be a law, then perhaps nothing truly answers to 'law'.[17]

While many contemporary accounts do not make explicit appeal to God, they still attempt to hold on to the governing aspect. The Humean complaint then is that the governance has become mysterious. It made sense on the older theistic picture, but if God is not governing the world then what exactly is involved in this governance? The existence of a penal system suggests that we cannot use our legal laws as a basis for the governance in question! It does not help to point out that, strictly, laws are not the governors on this picture.[18] Laws may be the content of the governance

[17] See Beebee (2000) for discussion.
[18] Roberts (2008) pp. 46-47.

rather than the governors, but this distinction simply pushes the question back a step. Who or what is occupying the governor role? Not only do we need an answer to this question, we need for that answer to make the modal implications un-mysterious. Whoever or whatever occupies that role must be the sort of occupier that makes clear the source of the constraining of possibilities commonly appealed to.

The difficulty of this challenge pushes Humeans to adopt an alternative view of laws. Both sides agree that laws correspond to regularities in nature. Anti-Humeans add something extra to that, whereas Humeans take the regularities to be all that there is. In slogan form: the laws are descriptions of the world's patterns. Rather than governing, laws merely describe the world's history. We don't need to appeal to anything metaphysically mysterious to understand their nature.

Before we get into the details, let us pause for a clarificatory note. Our talk of laws of nature can be understood to refer to one of two things. The first kind of meaning is that laws are propositions, of the kind that can be written down upon a page (of course, the physical marks on the page themselves are not the law: for starters, they came into existence a short time ago whereas laws are not the kind of thing that can be intentionally created by human activity). The second is that the laws are whatever state of the world is responsible for making true the propositions in the first kind of meaning. It is arbitrary which meaning we adopt due to the tight connection between propositions and the aspects of the world that they refer to. As Lewis talks of laws as propositions, the most natural route for this thesis to take is to talk of laws in the same way. Laws, then, are propositions that express certain special regularities in the world.

The claim that the laws are just the propositions expressing the world's true regularities is known as the naïve regularity theory. As with many philosophical positions prefixed with 'naïve', this sort of account is commonly taken to be an abject failure. There is little contemporary support for the view, but it is worth discussing a couple of its problems so that the virtues of Lewis' more popular development become apparent. The first problem is that it gets deeply counterintuitive results. Suppose that all the coins in my pocket are British. Then there is a regularity in the world that is captured truthfully by the proposition ‘All coins in my pocket are British’. But it is absurd to think that we have just discovered a new law of nature, the Nobel prize is not so easily achieved. That all these coins happen to be of the same form of currency is a contingent fact, whereas laws are supposed to be necessary. Accidental everyday regularities are not suited to be laws, but the naïve account lacks the resources to prevent them from being lumped together with the genuine articles. Suppose that we let the naïve regularity theory have a language whose predicates match up with the sorts of things we expect to turn up in our laws – leave aside the question of exactly how to specify this language. So the regularities we are considering are regularities of, say, particle positions and properties appropriate to those particles. Then we get the second problem: the number of particles at our world turns out to be physically necessary. Even in the restricted language just gestured towards, we can still form sentences like ‘Everything that exists is one of *n* particles’. But then, according to the naïve theory, it is a law that our world has *n* particles. This seems to be the wrong result; the number of particles looks like an accidental fact.[19] The lesson from all of this is not that the laws cannot express regularities, but that we need some additional resources if we are to pick out the right propositions. Not all propositions are created equally, some are much more important for our purposes.

The naivety of this account is found in its permissiveness. Its core failing is that it lets any regularity-expressing true proposition count as a law. What regularity theorists need, then, is for their theory to be able to pick out some such propositions for special treatment. The best-known attempt is the Mill-Ramsey-Lewis Best System Account, or BSA. The rough idea is that there are many different ways of systematising the world’s nonmodal facts. Of these ways, one is the best. Theorems that turn up in this best systematisation get to be counted as laws. This provides a way to distinguish between the accidental and lawful regularities. Of course, that is just a rough overview, the interesting work is in the details. Let us look at them. Here is a canonical statement of the BSA:

[19] The relationship between laws and initial conditions will be dealt with in more detail later.

> Certainly not just any regularity is a law of nature. Some are accidental. So an adequate regularity analysis must be selective. Also, an adequate analysis must be collective. It must treat regularities not one at a time, but rather as candidates to enter into integrated systems. For a given regularity might hold either as a law or accidentally, depending on whether other regularities obtain that can fit together with it in a suitable system. (Thus I reject the idea that lawhood consists of 'lawlikeness' plus truth.) Following Mill and Ramsey, I take a suitable system to be one that has the virtues we aspire to in our own theory-building, and that has them to the greatest extent possible given the way the world is. It must be entirely true; it must be closed under strict implication; it must be as simple in axiomatisation as it can be without sacrificing too much information content; and it must have as much information content as it can without sacrificing too much simplicity. A law is any regularity that earns inclusion in the ideal system. (Or, in case of ties, in every ideal system.)[20]

Whether we want to say that, in the case of ties for best system, the laws are those regularities included in all tied systems (assuming any are!) or that the world is too disordered for there to be any laws is a tricky matter. It also changes nothing central to the account. For expositional ease, let us set aside these cases and assume that one system really does come out as best.

A quick word of warning: it is easy to state the account in such a way that it looks like a thesis based on *our* knowledge: how we might organise information about the world or perhaps how God might present it to us.[21] But we should not fall into the trap of mistaking this presentational trick for a substantial claim about the account. The BSA is not concerned in any direct way with the counterfactual behaviour of omniscient beings. Deductive systems exist independently of what anyone happens to know, and the laws are based on the ranking of such systems.

The standard BSA has descriptions of the world competing against one another. The winner of this balancing act gives us the laws. "A best balance of what?" we might ask. Strength and simplicity. Strength may have been glossed as information content in the quotation above, but is standardly given meaning in terms of possible world consequences. A system is stronger to the extent that it is compatible with fewer

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[20] Lewis (1983) pp. 366-367.

[21] For example, Hall (2015) takes the unofficial line to be that the laws are whatever a 'Limited Oracular Perfect Physicist' says they are and even Lewis appeals to the divine in his (1973) presentation of the account.

possible worlds; the more worlds that a system rules out as being actual, the stronger it is. Clearly, some systems are stronger than others: the more that a system says, the fewer possible ways there are for things to be consistent with that system. At the extreme end, a maximally strong system would list all of the world's truths and so rule out all non-actual worlds (barring any worlds indiscernible to our own, if there are any).

Strength is in tension with simplicity, since the more that is added to a system the more complex that system becomes. Very simple systems contain very little information, perhaps only the truths of logic. Simplicity is to be understood in a broadly syntactic sense where, for example, a linear function is simpler than a quadratic function. Simplicity in the BSA ends up being a language dependent notion, and so we must specify a language to express candidate systems in if we are to assess how simple each is. As an easy way to see the motivation for this claim, imagine how different this chapter would look if it was written without using words whose first letter lies in the second half of the alphabet! The issue of language will come back to cause problems in chapter three, and we will see how some modern developments part ways with Lewis on this issue in chapter four. For now, let us merely note that Lewis has a preferred language in mind for candidate systems: one where all of its predicates refer to perfectly natural properties.

The BSA in its early form involved a two-way balancing act between strength and simplicity. Lewis later sought to incorporate chance into his reductive project by asking the BSA to do a bit more work.[22] A third virtue, fit, was introduced and systems that included probabilistic laws were allowed to compete. The regularities in the best system might talk about the chances of some outcome in various situations (with decay probabilities being the standard example here). In addition to trying to be as simple and strong as possible, candidate systems should also try to fit the history of the world, in the sense of assigning a high chance to the world's history occurring as it did. With chance included in the reduction alongside laws, the best system must balance three different virtues against one another. However, despite this

[22] Lewis (1994a).

amendment to the structure of the account, little else changes. For this reason, I will adopt the usual convention of those writing about the BSA without a specific focus on chances and largely set this amendment aside. Since the core discussions later in the thesis are not dependent on whether chance is reduced via the best system, there is no need to complicate matters by continually bringing it up.

The BSA aims to capture the fundamental laws, typically taken to be those discovered by certain branches of physics. But what about the other sciences and nonfundamental laws? We should not expect these to enter into the best system as axioms due to the vocabulary requirement the BSA imposes. Since all regularities must be expressed in terms of perfectly natural properties, any regularity concerned with the distribution of nonfundamental properties must be given an appropriate translation in order to be included in a system. This translation may be rather long and complicated. Take an ordinary object like a cat. Now try to describe it and its behaviour in terms of particle positions, spin states and the like. It is hardly plausible that the result will be a simple one. The widespread agreement that even attempting this has more than a whiff of madness to it indicates general opinion that translations of the special sciences into the language of fundamental physics would be an extremely messy affair. (We might still want to say that an in-principle description of the feline behaviour exists even if it is not one that we will ever grasp. This would be to affirm a commitment to there being some fundamental level to which everything else reduces. Since the cat is nothing more than patterns in this base level, a summary of the relevant fundamental goings-on is sufficient to capture the cat's life. There is something obviously attractive to this thought if one accepts something like Humean Supervenience.)

The defender of the BSA can make two points here. First, it is a somewhat controversial matter whether the special sciences genuinely have laws. If they do not, then the absence of such laws from the best system is no problem for the account. Second, if the special sciences have laws then there is still a space for them in the BSA. Since Lewis claims that every regularity-expressing theorem in the best system is a law, there is the possibility that the best system will contain derived laws. If some theorem is a consequence of the best system's axioms, it will also count as a law. We can then draw a distinction between different kinds of laws: those that are axioms are the fundamental laws, those that are derived from the axioms are nonfundamental laws. These might not be immediately apparent to us, which is why we need branches of science other than physics to help us discover them. For Humeans favouring the regularity view but who lack Lewis' firm commitment to natural properties, there is another option open. We could modify the vocabulary requirement in order to let the generalisations of the special sciences be simple enough to enter the best system directly. This is a major motivation for the Better Best System Account, which we will examine more closely in chapter four.

It is not uncommon to find complaints that Lewis, and the BSA's later advocates, do not say enough about the notions of strength, simplicity, fit and balance.[23] The notions are unclear, so the complaints go, and sometimes problematic. So Humeans ought to say more about precisely what it is they mean when they appeal to these. I agree. The notions are often left vaguer than we could wish, and there is a need for clearer statements of what these are and how they balance against one another. But note also that this is not a knockdown problem: a claim that Humeans still have work to do is very different from a claim that there is work Humeans cannot do. Perhaps part of why this work has not been completed to the extent we might wish is that thought that the relevant virtues are the ones found in scientific practice. The virtues used in theory building are constitutive of lawhood, and the usual virtues mentioned are just first approximations to what the correct virtues are. Something about this seems right. Given the lack of a better alternative set of virtues, I will continue to refer to strength and simplicity as the ones being balanced, but we should keep in mind the thought that it might not really be those specific virtues (understood in the usual way) that are found to an optimal extent in the best system.

As we have just noted, there might well be issues with the virtues of strength and simplicity: there is a need to either give further details about them or replace them with more appropriate virtues. A more worrying concern is that scientists do not

[23] For example, Hall (2015) considers strength to be an embarrassment, Woodward (2014) is bothered by being unable to find this kind of simplicity in scientific practice, and Elga (2004) shows that fit leads to problems.

engage in the balancing act that the BSA thinks is inspired by scientific practice. Woodward has given voice to this as the claim that scientists adopt something closer to a threshold view: theories might be dismissed on account of being too complex or ad hoc, but they must first meet a certain value of strength to even be considered.[24] This threshold value is quite straightforward, as it is just the requirement that a theory accounts for all (or perhaps virtually all) of the available data. Any theory that is not strong enough to do this is inadequate, no matter how simple it may be. So there is really no balancing at all, and the laws of the BSA do not reflect the scientific laws.

Let us set aside the issue of whether the BSA-laws and the scientific laws have to match up (that's a substantive question on its own). Let's also set aside a dismissive response to all of this, that in stating what laws are constituted by, Humeans are not tied to any specific scientific practice. The thought behind that kind of response is that the nature of laws is a metaphysical matter and if actual scientific practice is misaligned with that nature, so much the worse for scientists: the scientific laws may fail to be the 'genuine' laws. But while this is a possible response, it is not one that fits comfortably with other Humean commitments.[25] Humeans are typically respectful of science and deeply suspicious of additional metaphysics that operates behind the scenes, inaccessible to our empirical investigations. We can see this in the quotation from Lewis above, that the best system has 'the virtues we aspire to in our own theory-building'. Claiming that there are laws that, for example, underwrite our counterfactual statements but that the scientists who take themselves to be discovering these fundamental laws are looking in the wrong direction, would be in considerable tension with this.

Instead, let us focus on whether Humeans adopting the BSA can say anything about the practices Woodward has brought up. There are really two cases that need to be given a Humean explanation. The first is the apparent lack of balancing: if scientists

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[24] Woodward (2014) pp. 100-102.

[25] Although the Lewisian way I characterise the Humean position here is not the only way to approach matters. Swartz (1985) is an example of an author with Humean commitments who draws a distinction between scientific laws and underlying laws of nature.

look unwilling to sacrifice strength for simplicity, then how are they balancing virtues? The second is that many fundamental laws look to be both simple and strong. If laws like those of Newtonian mechanics can be both highly simple and highly strong, what need is there for a balancing act?[26] We will take these in order. First, the threshold view that Woodward mentions is not the only way to interpret scientific practice. That scientists appear unwilling to sacrifice strength for simplicity does not immediately mean that there is a minimum level of strength required for a theory to be considered. This is where the term 'balance' might be accused of being misleading. It is no part of the BSA that strength and simplicity are equally valued, only that the best system has achieved an optimal trade-off of them. The metric by which a system is judged to be best might be weighted towards one of the virtues that it considers. If the metric prizes strength to a greater degree than simplicity, then small gains in strength might be valued as highly as large gains in simplicity. This is a possible explanation for the practices Woodward draws our attention to. With a weighted metric in place, it might still be possible for the best system to sacrifice some strength for a correspondingly large amount of simplicity. But, as a practical matter, none of the theories that scientists consider plausible show this in action. That is, none are so much simpler than the others that they can afford to miss out on any available strength. (That we don't see scientists offering up radically simpler but weaker theories could be explained by an implicit acceptance by the community that strength is to be favoured, even if the scientists don't think of themselves as actively trying to achieve a kind of balance.)

In fact, the second point Woodward makes could be viewed as a good example of the balance the BSA envisages – contra his initial impression! Consider a system of Newtonian mechanics that lacks Newton's second law of motion. Such a system is simpler than one which contains that law, for it has fewer axioms. But it is massively weaker, for it says nothing about how an object should behave when acted upon by a force. Adding the second law to the system brings a large increase in strength at a

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[26] Of course, the laws of Newtonian mechanics are neither fundamental nor, strictly speaking, what the BSA would class as laws. Woodward is undoubtedly aware of this, so I take his point to be that the fact that scientists were willing to consider them as such indicates that scientists may think the genuine best system is relevantly similar.

small price to simplicity, and so it buys its way in. This is all really as Humeans would expect here, perhaps unsurprisingly given then classical flavour of Lewis' Humeanism. That serious candidate systems are both strikingly strong and simple is not a bug of the account, it is very much a feature. This consideration ties back into Woodward's first concern, that scientists seem unwilling to sacrifice any strength. The reality is that that observation is not quite accurate: there is a straightforward way that scientists do give up on strength. Notice that our scientific theories tend not to contain within them a list of all of the details of every relevant experiment. They might be formed from that data, but Newtonian mechanics does not consist of Newton's laws of motion plus the details of every experiment involving forces in any way. Such a system would be absurdly large and complicated. But it would also be stronger, since it makes specific claims about what happens at the world. One might want to object at this point that it would be no stronger than Newton's laws plus the initial state of the world, since this system is deterministic and so implies every successive world state. Perhaps so, but the same cannot be said for any system that contains probabilistic laws. Those sorts of scientific theories can be made stronger by including experimental results rather than merely probabilities for those results. But including the results brings a high complexity cost for its strength gains. If scientists are balancing strength and simplicity with respect to a metric weighted towards strength, they might still prefer the system with just the laws of motion (and perhaps the initial conditions – see the following section). We have then an example of potential strength that scientists are willing to leave out of a system, and balancing considerations could plausibly be part of an explanation of why this happens.

1.3 Initial conditions

Let us turn to an amendment to the structure of the BSA suggested by Hall.[27] With regards to physical possibility, the aim of the best system is to set out which histories of fundamental events are possible. Supposing that our world was Newtonian and

[27] Hall (2015) and in more detail in Hall (unpublished).

its fundamental existents were point-particles, the world's best system would aim to tell us which particle trajectories through spacetime are nomologically possible. When looking to the practices of working scientists, we notice that one thing theories pick out are dynamic laws. These tell us how a system will behave: if it is in one state now, the theory will predict what state it will be in in the future. But this alone is not enough to tell us which distributions of fundamental matter are possible. In our Newtonian example, the dynamic laws will tell me how a particle can behave, given its location and properties. But they provide no information regarding what properties that particle actually has, or where it is located right now. In order to determine a particle's trajectory, I also need information regarding its state at some point in time (such as its mass, charge, position and velocity). If the laws are deterministic, then it makes no difference which point in time is supplied, for both its past and future trajectories will be determined. To simplify, we will talk of initial conditions, taken to mean the state of the world's fundamental constituents at some initial time.

When it comes to determining physically possible histories, the best system comes in two parts. The initial conditions hypothesis (ICH) makes a claim regarding the world's initial conditions. The dynamic hypothesis (DH) makes a claim about how these conditions change. Together, they tell us how the world's perfectly natural properties can be distributed across spacetime. That this sort of division can be found in our scientific theories is expressed by Wigner:

> The regularities in the phenomena which physical science endeavours to uncover are called the laws of nature. The name is actually very appropriate. Just as legal laws regulate actions and behaviour under certain conditions but do not try to regulate all actions and behaviour, the laws of physics also determine the behaviour of its objects of interest only under certain well -defined conditions but leave much freedom otherwise. The elements of the behaviour which are not specified by the laws of nature are called initial conditions.[28]

One motivation for including initial conditions in the best system has just been given: doing so better reflects scientific practice, something that the BSA seeks to align itself

[28] Wigner (1967) p. 39.

with. Another reason is the use that they can be put to. A particularly well-known problem that this may help solve is that of the world's temporal asymmetry. The laws provided by classical mechanics are time-symmetric, such that if a history is compatible with the laws then so too is the reversed ordering of that history. But our experiences are quite clearly not time-symmetric. There is a subjective difference between those of our experiences that occurred in the past and those that will occur in the future (most obviously, I have memories of some past events, but no memories of future events). When I place an ice cube in warm water it melts, and I am confident that I can distinguish this from the sequence of events where an ice cube forms in the warm water. Time, then, has a distinctive direction that the classical laws look ill-placed to explain. Our world is not a classical one, but there is no easy solution to this problem to be found by appealing to quantum, rather than classical, mechanics. The dynamics of quantum mechanics are captured by Schrödinger's equation, which is also time-symmetric.[29]

It has been suggested that an answer to why we experience this asymmetry can be formed from thermodynamics. The second law of thermodynamics states that the entropy of a system – a measure of the system's energy unavailable for useful work – will increase over time until the system reaches an equilibrium (at which point its entropy is maximised). This is taken to form an explanation for why my ice cube melted in the warm water. The ordered molecules that form the ice cube have lower entropy in that state to the state in which those molecules form part of the warmer water. The water in the glass reaches thermal equilibrium when the ice cube has melted so that the temperature of the water is more evenly distributed. (Pretending that the glass of water is a closed system. A similar process that occurs between the water and the surrounding air in the room will result in the warm water eventually cooling to room temperature, and so forth.) Unlike the (fundamental) classical laws gestured at earlier, the second law of thermodynamics is not time-symmetric. It would seem that the arrow of time is connected to this thermodynamic asymmetry, if not for an immediate problem. As thermodynamics is not fundamental, its laws do

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[29] Quantum mechanics will be set aside in the following discussion for simplicity. Albert has argued that his proposal extends to the GRW interpretation of quantum mechanics, although this is not without difficulties.

not describe the fundamental processes that are occurring. But how can time-symmetric fundamental processes give rise to time-asymmetric processes? If the melting of the ice cube is compatible with the mechanical laws, then so is the spontaneous formation of that ice cube in warm water!

Boltzmann is credited with taking the first step to answer this through statistical mechanics. Relative to a measure over the microstates of some system, a majority of microstates of the macrostate are on trajectories increasing in entropy and therefore approaching equilibrium. Taking this measure to specify a probability measure, the probability of a system increasing in entropy is close to 1. This grounds the thermodynamic and associated temporal asymmetries in the more fundamental mechanics. But it alone does not solve the problem. The same reasoning that showed the entropy of future states was very likely to be greater can be run in reverse to show that the entropy of past states was also very likely to be greater. The absurd conclusion is that entropy increases no matter which direction of time we look in. Yet – again – we do not observe ice cubes forming in warm water.

Albert and Loewer have recently advocated introducing an additional postulate to capture the required asymmetry.[30] If the universe started in a low entropy state, we can apply the statistical mechanical reasoning to show that it will tend to increase in entropy over time. A boundary condition concerned with the early universe is sufficient to generate the required asymmetry when combined with time-symmetric laws. If our fundamental scientific theory includes three elements, the claim is that we can recover the asymmetry of time from them. Taking their inspiration from the film *A Serious Mind*, Albert and Loewer call these three elements the Mentaculus:

[30] Albert (2000) and Loewer (2012).

(i) The fundamental dynamical laws.

(ii) The Past Hypothesis: the claim that the world's early macrostate had low entropy.[31]

(iii) The Statistical Postulate: a law specifying uniform probability over the microstates that make up this macrostate.

A full discussion of the consequences of accepting the Mentaculus would take us far afield from the present concerns. That said, it is worth pointing out just how important a role the Mentaculus is supposed to play. We have already mentioned that it grounds time's arrow as a non-fundamental phenomenon. It is also supposed to ground the asymmetry of action, the fact that I do not appear able to influence past events while I do seem able to influence the future. Since the Mentaculus assigns probabilities to every proposition regarding microstates, and macrostates are realised by these microstates, it assigns probabilities to propositions concerning macrostates. Albert and Loewer have argued that these can play the roles that we expect objective chances to play. Despite its only dynamical laws being the fundamental ones, the Mentaculus is also taken to explain the successes of the special sciences. Since it assigns probabilities to all propositions concerned with the world's macrostates, it must assign probabilities to all special science generalisations. All probabilistic explanations made by these sciences will be underwritten by the Mentaculus. This is an impressive amount of work, if it succeeds.[32]

Albert and Loewer's proposal is a specific example of the more general project of elevating the status of initial conditions in the BSA. It won't work with the standard formulation of the BSA as the Mentaculus has no real chance of being counted as the best system under those rules. Candidate systems must be stated in a language whose predicates refer to perfectly natural properties. But entropy is not fundamental and, when put in terms of microstates possessing such properties, would need to be cashed out as an infinite disjunction. This means that the

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[31] That this needs to be added to the laws was also suggested by Feynman (1965) p. 116.

[32] Unsurprisingly, not everyone thinks that all of this can be done. For some criticisms, see Earman (2006), Frisch (2011) and Elga (2001).

Mentaculus scores horribly on simplicity and so will not be the best system.[33] Let us set this aside for now, since the relationship between the BSA and language will be explored in more detail later when we encounter the so-called Better Best System Accounts, and these provide Albert and Loewer with the resources necessary to respond to this concern.

Once we have set aside concerns arising from the non-fundamentality of the notion of entropy, one might wonder whether the claim that the Mentaculus is the best system is really a modification of the usual BSA at all. After all, the best axiomatisation of the world's truths might well include more than just regularities. This point is noted by Lewis in his presentation of the BSA:

> The ideal system need not consist entirely of regularities; particular facts may gain entry if they contribute enough to collective simplicity and strength. (For instance, certain particular facts about the Big Bang might be strong candidates.) But only the regularities of the system are to count as laws.[34]

So the original BSA allows for there to be information concerning initial conditions in the best system. Statements regarding initial conditions will have to earn their keep in the usual way: by contributing to the system's strength without costing too much simplicity. But it is easy to imagine how this might go, since such propositions will be assessed in the same way as those making generalisations about the mosaic. In a world of Newtonian point-particles which are neither created nor destroyed, it is plausible that a statement of the total number of particles might enter the best system. Such a statement would not need to be particularly complex, given that it only assigns a (potentially very large) number to the particle distribution. It would also contribute much strength, given that it would cut down considerably on the number of possible worlds compatible with that system. The only caveat is that this information won't be counted as a law, since it is only the regularities that can be marked out as special in that way.

[33] This objection appears in Schaffer (2007).

[34] Lewis (1983) p. 367. That the laws could mention particular places or things is considered in the postscripts to Lewis (1980), reprinted in his (1986a) p. 123.

This last point marks a crucial difference between the Mentaculus and the orthodox BSA. Loewer intends for all axioms that enter into the best system to count as laws, not just those expressing regularities. Both the Past Hypothesis and the Statistical Postulate gain lawhood through this move, despite both being concerned with how the world was at a particular time, as opposed to how it develops over time. The question we should ask is whether this extension of lawhood is well motivated. On the one hand, this does not appear to violate Humean scruples. Since laws are not taken to have any kind of metaphysically productive role, it is not as if allowing initial conditions to be laws raises any difficult questions regarding exactly what is being produced or generated. Laws are particularly important and striking parts of the best description of the world; one might well want to say similar things about the initial conditions. As the world's initial state is just another part of the mosaic to be described, should a strong yet simple description of it be available, why not give that description the same metaphysical role as the parts of the system describing the other features of the world?[35]

The problem is not so much that the Humean account cannot make this move, but that it drags the account into difficulties. First and foremost, taking all of the best system's axioms to be laws is in tension with scientific practice. There is typically thought to be a difference between the laws of a theory, expressed by its dynamic equations, and the contingent initial conditions that one plugs into these equations in order to generate predictions. But if those initial conditions are laws, then scientists are mistaken to think of them as contingent, since they will be physically necessary. Cosmological features of the world, such as the large-scale flatness of the universe might end up as laws as they plausibly score well on the strength-simplicity balance. But cosmologists do not take these to be laws, instead treating them as contingent uniformities that hold because of how the early universe happened to be. For an account that is explicitly intended to be respectful of science, this is a heavy price to pay. There is also tension with how we normally treat counterfactuals and possibility. When assessing questions of how things might have been different,

[35] That Humeans might as well make this move in the probabilistic context is suggested by Maudlin (2007b) pp. 280-281.

we typically try to hold the laws fixed and vary the contingent particulars. But if the initial conditions hold as a matter of law then there is far less physical possibility than we usually think, especially if the dynamical laws are deterministic. It would be physically impossible for the universe to have evolved such that our solar system didn't form, or that I were not writing this or that there was one fewer particle in the world.[36] That the laws are not 'active' in some metaphysical sense does not mean that it is harmless to allow for there to be more of them.

Due to these difficulties, we ought not relax the requirement that the laws express only regularities.[37] Woodward has claimed that this restriction will not be of any help to Humeans.[38] Cosmological facts, like the world's having a low entropy past, might look like they avoid getting counted as regularities (and hence laws) on account of only being singly instantiated. However, he points out, many such facts can be written as though they are regularities in the form 'For each region of spacetime…'. As such, he does not see how the BSA can avoid having many statements of initial conditions turn out to be laws. The response to Woodward should be to acknowledge that wording can be an issue, but to deny that rewriting some fact has any influence on whether it is a candidate for lawhood. The language surrounding properties can often be misleading, this is nothing new. Take the single-place predicate *is a father* as an example. The surface appearance of this predicate suggests that, on account of it having a single subject, it singles out a monadic property. But this is not a required conclusion, for we might also note that the predicate can only be truthfully ascribed to someone if that person stands in a relation. One must be a father to someone else in order to be a father. Expressing this state of affairs in terms of single-place predicates does not change the

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[36] This depends on how much of the initial conditions get set in the best system; systems that set fewer initial conditions end up as more permissive regarding later states. The situations I mention might not end up being regarded as impossible if the initial conditions are only roughly characterised, but the risk remains.

[37] This is a place that I part company with Hall, who is unconcerned about extending the lawhood franchise to non-regularities. While I agree that the BSA ought to pay more attention to initial conditions, particularly when it comes to applying the standards for judging which system is best, letting those initial conditions play the law role causes too much trouble for too little gain.

[38] Woodward (2013).

underlying reality: it is a relation that we are really concerned with. In the case of cosmological facts, that it is possible to express them in a manner that might suggest a regularity holds does not thereby make such facts genuinely express regularities. This is just one more occasion where we need to be careful that our ways of speaking do not lead us into making mistakes about what is going on.

The requirement in the original BSA does a better job of aligning with scientific practice and protecting the account from the charge that, say, statements of the universe's total energy enter into it as laws in a mistaken manner. Even sticking with this original requirement, it is not entirely clear that the Mentaculus will be the best system. Set aside concerns about whether Albert and Loewer can recover all that they wish to from the Past Hypothesis and Statistical Postulate, there are issues relating to the choice of best system that concern us here. Suppose that we compare a system consisting of just the dynamic laws with one that adds the world's precise initial conditions to those laws. The first will be simpler by far, but will be massively lacking in strength when compared to the second system. Assuming deterministic laws, the second system will entail the world's entire history, while the first will not imply any particular matter of fact. The Mentaculus will fall somewhere between these two extremes as it adds some – but not all – of the initial conditions to the system. The question then is which of these will be counted as the best.

Frisch is willing to agree with Loewer that the Mentaculus might beat out the system of the dynamic laws alone: if we ignore the issue of how to state entropy in a language of perfectly natural properties, then the Albert-Loewer proposal adds a small amount of complexity in return for a significant gain in strength.[39] But he finds it less clear how to adjudicate on the question of whether the Mentaculus will beat out the system of the laws plus the precise initial conditions. Both the gains in strength and the cost in simplicity are very high in such cases. There is a fair concern here, but the target is mistaken. Frisch is picking up on a long-standing issue concerning how strength and simplicity are balanced. The metric that is used to select the best system has not ever appeared in print. As far as I know, no-one has a

[39] Frisch (2014).

clear understanding of precisely how it works and exactly how much it favours strength or simplicity. That is an extremely difficult question to answer, and the lack of details is a cost to accepting the BSA. Some might be tempted to handwave the issue and declare it a problem for the scientists to solve when selecting theories, not one that philosophers ought to be deciding from the outside. But ignoring the problem will not make it go away: if a balance of theoretical virtues is a core feature of one's account, then one ought to have some desire to say something about that balance. So there is an outstanding problem here, but it is not with the Mentaculus. Albert and Loewer do not need to change any of the details regarding how systems get to be best, so concerns relating to that metric are really concerns about the wider Humean BSA. (Frisch also has the stronger worry that the gains in both strength and complexity are so great that there may not even be a determinate winner. That is premature, since he does not provide any additional reason to think that the metric cannot cope with large trade-offs. In order to properly argue for or against this criticism, we would need to possess more details about the metric.)

Schaffer suggests that we should expect the Albert-Loewer package to lose to the laws plus precise initial conditions package.[40] His thought is that the cost of introducing two somewhat simple axioms is roughly equivalent to that of adding one more complex one, but the strength gained by adding precise initial conditions far outweighs what the Mentaculus can achieve. Hence the Mentaculus will not be the best system. Given that this involves a specific claim regarding how simplicity should be weighed, it is difficult to fully assess without having a more complete metric for simplicity in the BSA. But we can point out that it is not obvious Schaffer is right to think that the two packages are on a par with regards to simplicity. The axiom in the precise package is going to be very complex indeed, perhaps even infinitely so. (Schaffer is aware of this and suggests falling back to systems that include some information but not the entirety of the initial conditions. He may well be correct that one of these will beat out the Mentaculus, but from the perspective of the current project it doesn't really matter. The important thing is that the best system will strike

[40] Schaffer (2007) pp. 130-131.

a balance that takes the initial conditions into consideration; which particular system does this best is another matter.)

Might the initial conditions be simple? Well there are certainly ways in which this could occur. For one, it might be the case that some of the initial conditions happen to have a neat mathematical representation. If the number of particles in the world happened to be describable in a mathematically concise manner, there would be a good argument that adding an axiom concerning that particle number to a system would not come at a high price. But perhaps we don't need to rely on the world being 'fortunate' in this way. Hall has suggested that there is a way in which the state of the world at some time can be easily expressed, this forms what he calls the problem of the phony fundamental constant.[41] Take all of the physical magnitudes possessed by the world's particles and express them in decimal notation. Then the phony constant can be generated by combining all of these into a single number and interleaving the digits. This constant now contains a massive amount of information regarding our world: its precise fundamental state at some point in time. As such, it can buy its way into candidate systems by adding to their strength.

If adding the initial conditions to our candidate systems is this easy, then we should expect the best system to be one in which the precise initial conditions are added to the dynamical laws. There would be little need to consider more moderate systems like the Mentaculus. Hall (rightly to my mind) considers this a problem and modifies the way that initial conditions interact with the theoretical virtues in order to overcome it. His modification is both interesting and independently motivated; it will be dealt with shortly. But we do not need to appeal to the problem of the phony fundamental constant to motivate it, because there is no such problem for the BSA. The obvious way to respond to it – by claiming that adding a hugely long number is actually a substantial increase to complexity – does not work well. As Hall is quick to point out, while it is part of the practice of physics to appeal to fundamental constants, there is no good reason to think that these happen to take mathematically tractable values. It might well be the case that our universe happens to have

[41] Hall (2015) pp. 269-270.

especially long constants. Rejecting the problem on those grounds creates tension with the practice of physics.

A better way to respond to the problem is to remember what exactly it is that the phony constant mathematically represents. We get the number by sticking together the values particles have for certain properties. These properties are good candidates for being perfectly natural properties. The information in the constant is really information concerning the instantiation of all of these natural properties. To say that the world has this constant is to say that it has a particular distribution of properties. The phony fundamental constant is just a way of disguising a massively (perhaps infinitely) conjunctive claim regarding the instantiation of natural properties. It is not fit to enter a best system as-is, since it is not perfectly natural and all systems must be stated in a language of perfectly natural properties. But once we have translated it into the appropriate vocabulary, we end up with a very large conjunction. And that is plausibly not a simple addition to any system.

While the phony fundamental constant is not a problem, there is still good reason to adjust how the Initial Conditions Hypothesis interacts with the virtue of a system's strength. If we apply the same standards to the ICH as we do to the Dynamic Hypothesis, we get the result that a system is better the more strictly it specifies our world. Systems compatible with fewer possible worlds are stronger, regardless of whether the lack of compatibility arises from the dynamic laws or the initial conditions. The issue here is the tension with scientific practice. As Hall points out, it is not uncommon for physicists giving explanations to appeal to counterfactuals that concern other worlds which have different initial conditions to our own, but have the same dynamic laws.[42] A representative example of just this is Comins' *What If the Moon Didn't Exist?*, which considers various counterfactual ways in which the Earth might have differed.[43] It is clear from the way in which he treats the counterfactuals that it is the initial conditions which are taken to differ while the relevant dynamics remain unchanged. These are typically taken to be physical possibilities that simply happened not to obtain: we are not considering some distant

[42] Hall (2015) p. 270.
[43] Comins (1995).

world with different physics. But that is exactly the result that we would get if the initial conditions were specified precisely in the best system. This lets us see the phony fundamental constant in a new light. Its philosophical relevance comes not from the problem that it creates for the BSA (after all, the BSA has the resources to deal with that problem already), but from the way in which it highlights an issue with one of the theoretical virtues that candidate systems are judged by. Strength – at least in its usual form – cannot be applied in the same way to the system as a whole. If we attempt to do so, we get the result that a system is better when both its ICH and DH narrow down the possible worlds. But what we want is a more nuanced account. As a first attempt: a system is better when its DH narrows down the worlds, but worse when its ICH does that same thing.

Let us look at this suggestion, that a system is improved when its ICH is uninformative. There is certainly a sense in which this is true: a candidate system that requires precisely specified and seemingly arbitrary initial conditions in order to capture the phenomena looks to be worse than an alternative system that makes no such requirement of its initial conditions.[44] Wigner for example claims that we want the initial conditions to impose as little structure on the world as we can:

> [The] existence of regularities in the initial conditions is considered so unsatisfactory that it is considered necessary to show that the regularities are but a consequence of a situation in which there were no regularities.[45]

We can say a bit more than Hall does about how the resulting account goes. A system is judged according to how well it balances simplicity and strength, where this latter virtue is taken to divide into the informativeness of the DH and uninformativeness of the ICH. Uninformativeness, however, is easy to achieve. Simply take a system whose ICH says nothing at all. A system making no requirement on what the initial conditions be like will be both maximally strong and simple with regards to its ICH. So it is important to note that the two components of a candidate system are not judged separately and then their scores summed together for the final tally. For if that were to happen, then we could be sure that the best system would say nothing

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[44] This is nicely brought out in Roberts (2008) pp. 16-23.
[45] Wigner (1964) p. 996.

at all about the world's initial conditions since that would be the best strategy for maximising its score on the ICH portion of the competition. The system is judged for its simplicity as a whole in the usual manner. The system, rather than either of its two parts, is assessed for its strength, again in the usual manner. The informativeness of the ICH is a trade-off that systems make, much like complexity. Adding in additional information concerning initial conditions allows a system to buy some strength – albeit at the cost of losing simplicity and uninformativeness of ICH. A system does not increase in strength through having an uninformative ICH, it loses strength. But that is just a part of the normal balancing act that candidate systems must go through: strength is in tension with both simplicity and uninformativeness.[46]

So including an ICH in our candidate systems pushes us towards assessing how the strength of systems is judged and getting clearer on what is being balanced. Might we also do the same thing with the DH? Hall suggests that it is better to claim that we search for two-way determinism of the DH, rather than strength in the sense of ruling out possible worlds. As was just mentioned, that cannot be quite right since the strength that counts is a property of entire systems; we are not judging the strength of system components directly. Pedantry aside, does the move to determinism change anything? Or, to phrase things differently, does judging a system according to how deterministic it is differ in some substantive way from judging according to the usual notion of strength? To make things plain, I don't think that it does. To start with, notice that a maximally deterministic system will rule out all nonactual worlds, while a maximally strong world is consistent with the world evolving in one way only. Now take the first direction of implication: does moving a DH towards full determinism also decrease the range of possible worlds that it is compatible with? The answer here is clearly that it does. If some process is deterministic then there is only one possible outcome to that process. If all processes are deterministic then there is only one possible state of affairs compatible with those processes occurring as they did. A system whose DH is fully deterministic will

[46] In fairness to Hall, he aims to replace the notion of strength with explanatory power. But he takes that latter notion to be comprised of the uninformativeness of the ICH and the informativeness of the DH. As just indicated, I don't think that is the best way to set up the balance.

be compatible with only those worlds that give the same outputs as our own does to specific inputs. The space of possibilities is not shrunk to one, worlds can still differ due to different initial conditions. But we have ruled out worlds which match our own up to a point and then differ, or which used to differ and then began to perfectly match.

Now take the second direction: does having a DH rule out more worlds make it more deterministic? One set of worlds to rule out are those that match up to a point and then differ. Another set of worlds to rule out are those that differed and then started matching. Incompatibility with such worlds makes a system stronger, but it also makes its DH more deterministic. There are also worlds which do not have any spatiotemporal regions that match our own. A system that rules these worlds out would be stronger in the usual sense, although not necessarily deterministic. Adding in probabilistic propositions to a system might well make the system incompatible with such worlds, although it would not make the system deterministic. But there are ways to increase the system's degree of determinism without going all the way: a system that specifies smaller ranges of probabilities is arguably stronger than on which is more permissive about the permitted probabilities.[47] So while increases in a system's strength might not always be obtained by adding fully deterministic propositions, even the addition of chancy ones can make the system as a whole more deterministic. As such, I see little advantage in abandoning the usual talk of strength as the ruling out of possible worlds.

To summarise: the practice of physics pushes us to include a split between a system's Initial Conditions Hypothesis and its Dynamic Hypothesis. The original presentation of the BSA actually allowed for initial conditions to enter the best system, although not as laws. I have urged that we retain this constraint; Lewis was right to restrict lawhood to the universal generalisations. The real change to the BSA that is required by this move is a better awareness of how the theoretical virtues that pick out a system as best interact with both parts of candidate systems: while both strength and simplicity are applied in the usual way, the system's strength must also be

[47] I am assuming here that a system which specifies probabilities for some process is closer to full determinism than one which remains completely silent.

balanced against how informative an ICH it requires. This is not without precedent, as the notion of fit plays a similar role when the BSA is extended to include probabilistic laws and play a role in the reduction of chance. Albert and Loewer's Mentaculus project can be viewed as one substantive suggestion on what the best system might look like in this more general framework (bearing in mind that we needn't view the Past Hypothesis or the Statistical Postulate as laws).

1.4 Targets of scientific inquiry

Despite suggesting that we amend the BSA to include a distinction between the ICH and DH, Hall suggests that there is a problem with doing so.[48] The concern is that once we have made this distinction, it is hard to see exactly why scientists ought to care about the laws generated by the BSA. That is worrying, since scientists quite clearly do try to find what the laws are! Let us examine what the problem is supposed to be. Anti-Humean views that do not attempt to reduce the laws have a straightforward way to explain why scientists ought to care about them. If the laws genuinely do have a role to play in guiding and constraining what happens at the world, then any account that correctly identifies them has picked up on a central feature of the world that has a deep relation to the nonmodal facts. Conversely, any account which ignores them fails to pick up on an important aspect of the way the world is. It is easy to see, therefore, why scientists should care about finding anti-Humean laws since, by doing so, they uncover a vitally important part of the world's structure. If we have any desire to find out about the world around us, we ought to be interested in laws like these. In short, this is why anti-Humeans have an easy time explaining why the laws are, in Hall's phrase, distinctively appropriate targets of scientific inquiry (DATSIs).

The Humean story must be somewhat different, since they must deny that sort of deep metaphysical connection between laws and nonmodal facts. Humean laws are a useful part of our investigation into the world's nonmodal facts. We do not use

[48] Hall (2015) pp. 272-273.

experiments to discover patterns that point to something behind the scenes that directs the worldly phenomena; there is no director behind the curtain! The mosaic and its patterns are not just all that we can observe, they are all that is in the picture at all. The role that laws play in our scientific investigations must be rather different. The usual evocative metaphor that suggest what this role is has been given by Beebee:

> So the idea is something like this. Suppose God wanted us to learn all the facts there are to be learned. (The Ramsey-Lewis view is not an epistemological thesis but I'm putting it this way for the sake of the story.) He decides to give us a book – God's Big Book of Facts – so that we might come to learn its contents and thereby learn every particular matter of fact there is. As a first draft, God just lists all the particular matters of fact there are. But the first draft turns out to be an impossibly long and unwieldly manuscript, and very hard to make any sense of – it's just a long list of everything that's ever happened and will ever happen. We couldn't even come close to learning a big list of independent facts like that. Luckily, however (or so we hope), God has a way of making the list rather more comprehensible to our feeble, finite minds: he can axiomatize the list. That is, he can write down some universal generalizations with the help of which we can derive some elements of the list from others. This will have the benefit of making God's Big Book of Facts a good deal shorter and also a good deal easier to get our rather limited brains around.[49]

This is a rather pragmatic role, for it suggests that the only reason that we care about what the laws are is that they help us to learn more about the world. That they are laws is, on its own, not sufficient to make us care about them. Rather, we care because they are useful to us (the contrast here with the more privileged anti-Humean laws becomes clear as there is reason to care about them beyond merely gathering nonmodal information in an efficient manner). The anti-Humean laws are DATSIs due to their metaphysical nature, while the opposite is true for Humean laws. Those laws must be DATSIs for some independent reason – the passage from Beebee suggesting the reason is that they are particularly useful in summarising the world's nonmodal facts – and their lawfulness is entirely dependent on them playing this role.

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[49] Beebee (2000) p. 547.

Returning to Hall's worry, it appears that the standards of strength used by scientists come into tension with the Humean claim that we care about laws due to their summarising role. The best system of laws will more recognisably be a DATSI if it is best because it is especially good at summarising nonmodal facts. After all, if finding out such facts is the purpose of scientific inquiry, Humeans will have a straightforward story to tell about why scientists ought to care about the best summary of those facts. This points towards Lewis' suggested way in which the virtue of strength should be evaluated: the more possible worlds that a system rules out as being actual, the more informative it is about the actual world's nonmodal facts. But if this is the case, it is no longer clear why scientists should look for their initial conditions to be as non-specific to our world as possible. The more uninformative the ICH, the less the system as a whole will rule out possible worlds. So if we aim to adopt the standards used by actual scientists, then why should we care about the laws generated by the Humean account? Once we make the split between the ICH and DH in the BSA, Hall's worry is that we can no longer explain why the resulting laws are DATSIs.

This can be expressed in the form of a dilemma. On the first horn, we look to the practices of scientists to inform the standards by which candidate systems are judged. But then it becomes a mystery as to why we ought to care about the laws the best system will generate. On the second horn, we stick to Lewis' original standards and so explain why it is that laws are DATSIs. But then we struggle to give a reason why scientists appear to be using rather different standards in their investigations. As Hall nicely puts it, this choice 'between a guilty intellectual conscience and insane revisionism is not a happy one.'[50]

The dilemma, however is a false one. While those may be the two available options available if one accepts this way of setting up the problem, there is a third option: reject this characterisation of scientific inquiry and the roles that laws play in it. It is fair to expect Humeans to give an account of laws that allows us to see why scientists might care about them. It is also right to require that if this Humean account shows

[50] Hall (2015) p. 273.

why laws are DATSIs, it does so for reasons beyond their lawhood alone. But what Humeans can reject is the suggestion that science is concerned only with the accumulation of information regarding the world's nonmodal facts. Suggestions as to what else science might be aiming at have been made:

> All science is a search for unification … Finding hidden links between seemingly disparate phenomena is what makes the scientific method so powerful and compelling. The distinctive feature of science is that it is both broad and deep: broad in the way that it tackles all physical phenomena and deep in the way it weaves them, economically, into a common explanatory scheme requiring fewer and fewer assumptions.[51]
>
> [T]he important thing in science is not so much to obtain new facts as to discover new ways of thinking about them.[52]
>
> The aim of science is to seek the simplest explanations of complex facts.[53]

That we wish to discover more of our world's nonmodal facts seems difficult to deny. But the scientific project is not solely concerned with listing these facts. It is also concerned with issues of explanation and – more controversially – understanding. Stating some collection of facts is one thing, showing how they fit together and relate to one another is a step beyond that. Not for nothing are scientific laws expected to play a role in scientific explanations. If I am able to appreciate that a given fact fits into a general pattern, I have gained a different kind of knowledge to that which I would have if I was merely aware of the fact and not its wider significance. One way in which we get a grip on explanations is through thinking about counterfactuals and difference-making.[54] If I had performed some other action, how might things have developed differently? If the world's initial conditions were tweaked just so, would it still have been hospitable to lifeforms like ourselves? If Isildur had cast the One Ring into the fires whence it came, what kind of life would Frodo have lived? In

[51] Davies (2006) p. 103.
[52] This is attributed to William Bragg by Hydén (1969) p. 115.
[53] Whitehead (1920) p. 163.
[54] Although the association between explanation and understanding is not without controversy, this is closely related to discussions on the relationship between explanation and counterfactuals. The most well-known defence of the claim that (causal) explanations are a matter of seeing what would be different under a counterfactual is Woodward (2003).

considering such questions, we need to grasp how different facts relate to each other in order to formulate any kind of reasonable response.

It is with this sort of aim in mind that we can say why scientists might care about the laws generated by a BSA that incorporates an ICH/DH split. Informativeness of the Dynamic Hypothesis is clearly a virtue, as the more deterministic the laws are, the better they pick out our world. Accumulation of nonmodal facts may not be the only aim of science, but it is still an aim. The reason that uninformativeness of the initial conditions is a virtue is that it plays into our counterfactual reasoning. If a maximally specific set of initial conditions was taken to be physically necessary, it would be very difficult to understand how any phenomenon depended on any other. Any difference in set-up would require that we consider physically impossible worlds when evaluating counterfactuals. But considering differences is a large part of how we get an explanatory grip on (and perhaps an understanding of) the world. A theory’s ability to contribute towards answering counterfactuals regarding alternative initial conditions is a significant part of its ability to provide explanations. Aiming for a less specific ICH lets a wider range of worlds count as relevantly like ours when we start to ask what things would be like if certain facts were different. The purpose of having uninformative initial conditions is to enable us to see that certain features of our particular world are not arbitrary. That this planet is home to creatures like us, or that there is a planet Earth in the first place, might well count as arbitrary: small changes in the boundary conditions could have led to different later results. But we can see how these differ from the patterns captured by the laws: that the planets should have orbits like the ones they in fact do is not arbitrary in the same way. In making every one of the world’s nonmodal facts count as physically necessary, a maximally specific ICH obliterates this difference. This is why a best system amended to include a split between the DH and the ICH should count as a DATSI. It plays a role in efficiently summarising the nonmodal facts at our world. It also helps us to understand how those facts fit together by supporting counterfactuals.

In tackling various issues that arise when we set out the regularity view, I have not tried to argue specifically for a single account that solves all of these issues in a way that all of the participants in the debate would accept. If that's the criterion for success, then I suspect that no such account is available. There are too many differences between starting presuppositions of the philosophers in question and their assessments of the relative upsides and downsides of each solution for that to be a reasonable goal. Happily, I don't think that trying to provide that single account is necessary, given the goals of this thesis. What we need is a reasonably clear idea of the Humean approach to laws, and metaphysics more generally. That's compatible with there being various accounts sitting together under the Humean umbrella. I leave it up to the individual Humeans as to which specific variation of the regularity account they find most palatable. In responding to Hall regarding what would make a system a DATSI, I have appealed to explanation. However, one might worry that Humean accounts of laws are unsuited to generate genuine explanations due to their lack of metaphysical 'oomph'. The following chapter takes up this question in some detail.

Chapter 2 Humean explanations

While the previous chapter was concerned with setting out the core Humean position, we turn now to a common objection to it. Regularity-based accounts of laws are often thought to fail to be explanatory and so cannot give us genuine laws. For all that it is well-known, the statements of this objection differ from one another in their emphasis. As such, the first step is to set out the problem in what I take to be its most troublesome form. Discussion then turns to a prominent response given by Barry Loewer, along with a restatement of the circularity objection that takes his distinction into account. Two further responses in the literature are considered: one based on the irreducibility of macro-explanations, the other on multiple realisability. Unfortunately, the circularity objection overcomes both and so I conclude that Humeans must look elsewhere. One such route requires examining the reduction of laws in more detail. There are multiple options here, depending on whether one takes the relationship between the mosaic and the laws to involve grounding and, if so, how the grounding relations are arranged. A radical solution to the problem of explanation beckons, although this comes at the price of giving up on what many take to be a core feature of the Humean world-view. For those reluctant to make this contrarian move, I then offer two alternative ways to respond. The first is to claim that the problem of explanation has not been sufficiently well motivated and that Humeans are free to reject one of its central premises (unless, of course, Humeanism’s philosophical opponents can supply the missing motivation). The final response is to bring into question the nature of the explanation being appealed to in this debate. I suggest that Humeans are not required to appeal to the kind of explanation that anti-Humeans are presupposing and that a pluralistic approach might both better fit the view and allow for non-circular explanations involving Humean laws.

2.1 The circularity argument

One of the roles commonly ascribed to laws is that they must be able to explain their instances. But, goes the immediate objection, this is something that the regularity view of laws cannot give us. The core worry is that the laws are what they are in virtue of the way the world is, and hence they cannot explain why the world is that way. Attempts to do so are guilty of circular reasoning. If the fundamental state of the world explains the fundamental laws, then we cannot immediately turn around and explain the world's state by appeal to these very laws. Otherwise we would be claiming that the fundamental state of the world explains itself, only with the additional use of laws as a middle man. Regardless of what one thinks of the possibility of anything explaining itself, any sense of explanation here is clearly not what was being asked for in the first place. Here is Tim Maudlin making the point explicit:

> If one is a Humean, then the Humean Mosaic itself appears to admit of no further explanation. Since it is the ontological bedrock in terms of which all other existent things are to be explicated, none of these further things can really *account for* the structure of the Mosaic itself. This complaint has been long voiced, commonly as an objection to any Humean account of laws. If the laws are nothing but generic features of the Humean Mosaic, then there is a sense in which one cannot appeal to those very laws to *explain* the particular features of the Mosaic itself: the laws are what they are in virtue of the Mosaic rather than vice versa.[55]

The very same point is also expressed by Marc Lange:

> In short, if the Humean mosaic is responsible for making certain facts qualify as laws, then the facts about what the laws are cannot be responsible for features of the mosaic.[56]

This is an objection commonly voiced by anti-Humeans.[57] It has some intuitive force to it. Suppose it were a law that all ravens are black and, upon coming across a particular black raven, I were to ask why it was black. Our usual way of thinking about scientific laws suggests an immediate answer to the question: this raven is black

[55] Maudlin (2007a) p. 172, italics in original.
[56] Lange (2013) p. 256.
[57] For example, by Armstrong (1983) and Bird (2007).

because it is a law that all ravens are black. In other words, an instance of the law is explained by appeal to that law. There is nothing particularly special about this case; we could multiply examples easily. However, anyone who provides an account of laws that takes them to be regularities faces a problem here. If the law that all ravens are black is a law because it is a regularity – perhaps one that possesses other features, such as being a part of the best systematisation of the world's facts – then the reason it holds at all is ultimately because of the fact that there are ravens and the fact that they are black. But if the reason that this raven is black is because of the law and the reason that there is such a law is because of the black ravens, then we have an explanatory circle going on. For one of the law's instances is a partial explanation of the law and so at least a partial explanation of itself. And self-explanation is a high cost for Humeanism to bear.[58]

Anti-Humeans like Lange and Maudlin take the absurdity of self-explanation to give us reason to give up on the idea that the Humean Mosaic is explicable by appealing to Humean laws. After all, if the laws do not explain their instances, then there is no explanatory circle and Humeans are not guilty of countenancing self-explanation. But the ability of laws to explain their instances is a core part of what we take laws to be: if Humean laws are unable to do this, then perhaps they do not deserve to be called laws at all.[59]

In order to assess the options open to Humeans, it will be useful to have a clear statement of exactly what this argument for explanatory circularity is. Michael Townsen Hicks and Peter van Elswyk provide a particularly clear formalisation of the objection:

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[58] We might wonder whether there's any genuine explanation going on at all here if things end up explaining themselves. That is, the failure of objects to explain themselves might plausibly be taken to be a prerequisite for an adequate account of explanation.
[59] See Hempel and Oppenheim (1948), and Hempel (1965).

(P1) The natural laws are generalisations.

(P2) The truth of generalisations is (partially) explained by their positive instances.

(P3) The natural laws explain their instances.

(P4) If *A* (partially) explains *B* and *B* (partially) explains *C*, then *A* (partially) explains *C*.

(C1) The natural laws are (partially) explained by their positive instances.

(C2) The instances of laws explain themselves.[60]

The initial conclusion (C1) follows from (P1) and (P2). The second conclusion follows from (P3), (P4) and (C1). As mentioned above, it might be more accurate for the conclusion (C2) to say that the instances of laws *partially* explain themselves. In what follows, I will assume that even partial self-explanation is a bad enough result for Humeanism, leaving aside the question of whether it is any more plausible than full self-explanation.

This is not the only way the argument might be formalised. Elizabeth Miller prefers to give it in the form of an inconsistent tetrad, to which Lange and others accuse Humeans of being committed to.[61] But this is merely a change in emphasis as opposed to a different argument altogether: three of the principles that Miller gives have equivalents in the above argument (specifically (P3), (P4) and (C1)) and the fourth is simply the negation of conclusion (C2). As such, it makes no real difference which way we choose to write out the argument.

A somewhat more substantive change that could be made would be to not frame the problem as one of self-explanation.[62] Take the case of the black raven once more. The blackness of any particular raven is explained by the law that all ravens are black. According to the Humean view of laws, this law is not grounded in just this single instance of a black raven but in the black raven regularity taken as a whole. So it is partially grounded in each and every black raven that ever exists. If one accepts the

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[60] Hicks and van Elswyk (2015) p.434.
[61] Miller (2015) p. 1317.
[62] This is suggested by Miller (2015) pp. 1324-1325 and then echoed by both Marshall (2015) p. 3152 fn. 14 and Lange (2016).

transitivity of explanation (that is, premise (P4) in Hicks and van Elswyk's formulation), one must also accept that each of these ravens partially explains the blackness of the particular raven being considered. But that is not a conclusion that we should wish to be saddled with! The colouration of some raven born in Russia a hundred years from now does not look like a suitable explanans for the colour of *this* raven here and now. The fact that *a* is *G* does not in general explain the fact that *b* is *G*.

So the violation of the prohibition on self-explanation is not the only problem that faces Humeans who accept transitivity of explanation. The issue is a wider one: the tension between these two claims causes one to have to accept a range of rather implausible conclusions. Since biting the bullet on that particular point is not an attractive option, it is better to look for other routes of response open to Humeans. In what follows I will leave the above formulation of the objection from explanatory circularity unchanged, since I regard its conclusion that instances of laws explain themselves to be bad enough. But it is worth keeping in mind the point just made, if only as a reminder that looking for reasons to accept self-explanation is not helpful. Even if that particular ban can be called into question, it will not solve the larger problem facing Humeanism.

It is also worth drawing attention to exactly who should be concerned with this objection to Humeanism. For the argument to get going, it requires that the laws be regularities. Despite labelling premise (P1) HUMEANISM, Hicks and van Elswyk are quick to acknowledge that the name is misleading. Humeans are certainly committed to the premise, but they are not the only ones. Anti-Humean views that take the laws to be generalisations that possess some additional nomic feature are also vulnerable to the argument. So the success or failure of this argument should be of interest even to those who are no friends of Humeanism. If nothing else, recognition of this point should be motivation for certain kinds of anti-Humeans to seek alternative arguments against their opponents!

An alternative way to raise this argument is given by Dan Marshall.[63] Start with this: the law that all *F*s are *G*s, together with the fact that *a* is *F*, explains the fact that *a* is *G*. This is just the claim that the laws are supposed to explain their instances. On the Best System Account, whether some regularity is a law is determined by what the best system is. In turn, that is determined by the various particular matters of fact. So whether all *F*s are *G*s is a law is partially determined by the fact that *a* is *G* (along with the other matters of fact). As such, the lawhood of this regularity is partially explained by the fact that *a* is *G*. But given transitivity, the fact that *a* is *G* partially explains the 'All *F*s are *G*s' law means it also partially explains itself. We are back to self-explanation of a law's instances.

Marshall prefers this presentation of the issue with its talk of facts being determined by other facts as it avoids the claim that generalisations are explained by their instances. This, in fact, forms a core part of his positive proposal on behalf of Humeanism (and will be dealt with later in this chapter). He also raises an objection to the style of presentation favoured by Miller and Hicks and van Elswyk.[64]

This is the thought that accepting that generalisations are explained by their instances come at considerable cost. He is willing to accept that accidental regularities are explained by their instances. The fact that everyone in this room is a philosopher does intuitively seem to be explained by appealing to the philosopherhood of each person who happens to be in the room (notice that explaining why some person in the room is a philosopher by way of appealing to the regularity is a far less satisfying approach). But this feature of accidental regularities should not be extended to laws:

> If laws can partly explain their instances then a vast number of particular matters of fact can plausibly be explained in terms of a much smaller number of particular matters of fact, together with a small number of laws. If [the explanatory priority of instances] is true, on the other hand, and laws cannot partially explain their instances, then a vast number of particular matters of fact will instead have to be foundational.[65]

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[63] Marshall (2015) p. 3148.
[64] Ibid. pp. 3161-3163.
[65] Ibid. p. 3163.

Of course, whether or not laws can (partially) explain their instances when instances explain those laws is precisely the point that is under discussion! Marshall finds reason to reject the approach taken in the next section, but that is a conclusion to be reached, not the starting point of the debate. It is important at this juncture not to beg the question against any of the accounts that we will consider.

Further, I find it unclear that the first case of explanation Marshall mentions is simpler or more parsimonious than the second. Humeans certainly do not need to take every particular matter of fact in the world as unexplained by any further facts. For one example, Lewis' project of Humean Supervenience aims to recover the richness of the world from a particular kind of austere base. Particular matters of fact that are non-local or non-fundamental are ultimately explained by the distribution of certain other (local and fundamental) particular matters of fact. One need not be wedded to the details of Lewis' account in order to make a claim like this: any account that is compatible with Humeanism regarding laws and seeks to explain non-fundamental facts in terms of fundamental matters of fact will have the same sort of result. The vast number of foundational particular matters of fact that Marshall envisages are not a necessary commitment of this approach to generalisations. Even if this were not so, we would still be lacking an argument that the gain in quantitative parsimony outweighs the loss of qualitative parsimony. That is, Marshall does not provide reason to prefer explanations based on particular matters of fact and unexplained laws to those based on a greater number of particular matters of fact alone.

For these reasons, I see little advantage in rejecting the original formulation of the argument given earlier. This will provide us with a clear way in which to set out the various responses that Humeans could make to the charge of explanatory circularity. The next section will begin with a particularly influential distinction that has been recently been drawn between different kinds of explanation involved in the Humean account.

2.2 Metaphysical and scientific explanations

In order for Humeanism about laws to be guilty of an explanatory circle, the explanations appealed to must be of the same kind. If we use *A* to explain *B* and then use *B* to explain *A* in the same kind of way, then, intuitively, there is something wrong with the explanation being offered. But if *B* is used to explain *A* in a different kind of way from the first case, then intuition may not tell against this. To put it another way, we might be prepared to grant that the transitivity principle in (P4) of the formalised argument holds for one kind of explanation, but resist the claim that transitivity will hold across different kinds of explanation. That is exactly the approach that I will describe in this section.

Here is a quick and dirty way of motivating the idea that we do treat explanations in this way. I am currently drinking a cup of tea, which a rather common event. Someone – presumably a person unfamiliar with British customs – might wonder why it is that I am so often found drinking tea. One kind of explanation available would be to identify certain kinds of distributions of fundamental particles as being tea drinking events (in a way analogous to how particles distributed table-wise might well constitute a table or a catty distribution of particles might be identified with a cat). We might then point out that if you were able to know the distribution of fundamental particles across my lifetime, you would find many such events. Another kind of explanation makes no reference to particles and instead appeals to my desires, local customs and, perhaps, mild addiction. While these two kinds of explanation are very different to one another, both of them deserve to be called explanations of my tea drinking habits.

Moreover, the two explanations do not merely explain the tea drinking, but they also explain each other to a certain extent. The first kind of explanation provides a physical ground for the mental states and cultural tendencies that the second appeals to. The second kind of explanation tells us why we should expect there to be the physical patterns that the first is built upon. But this interdependency of the two explanations introduces no problematic circles. The distinctness of the explanations is not diminished by acknowledgement of this connection. It would be very odd to dismiss either of these explanations on the grounds that having both makes each one guilty of self-explanation.

So much for the everyday case. For this to work when applied to laws, of course, we will need to see some details as to exactly what the different kinds of explanation appealed to by Humeans are supposed to be. Barry Loewer has suggested that there is such a distinction between metaphysical and scientific explanations.[66] Metaphysical explanations are concerned with constitution in some way. To say that *A* metaphysically explains *B* is to say that *B* is grounded in *A*, or that *B* holds in virtue of *A* or that *B* is constituted by *B*.[67] An explanation of this sort does not need to cite laws. If explanans and explanandum are both temporal entities, then they must be co-temporal. This last captures the following idea: if we want to say that *A* is the underlying stuff out of which we get *B* then they clearly must both exist at the same time. It would make little sense to claim that *B* is constituted by something no longer around.

Scientific explanations do not need to mention grounds in that way. Rather, Loewer suggests that the core of scientific explanation of some event is showing why that event occurred through appeal to other events and certain laws. If *B* scientifically explains *A*, then events mentioned in *B* will typically be temporally prior to events in *A*. That said, this is not a strict requirement since there is no requirement that the explanans or the explanandum be temporally located entities. Another difference between the two kinds of explanation is probability: scientific explanations may be probabilistic while metaphysical ones cannot.

> For example, the quantum mechanical explanation of why a lump of radium emits alpha particles goes by way of showing how the laws and the quantum state of the radium atoms make the emission likely. But of course, this doesn't show what the emission of alpha particles consists in.[68]

[66] Loewer (2012) p. 131. See also his (2007).
[67] More details about the contemporary notion of grounding can be found in Fine (2012) and Rosen (2010). The link between grounding and explanation specifically is considered in Jansson (2016).
[68] Loewer (2012) p. 131.

This is far from a full account of scientific explanation. That said, it is not intended to be one. Loewer is simply trying to give some characterisation of what the kind of scientific explanation he has in mind is. The most important point is that it be clearly distinct from metaphysical explanation; this at least seems successful. Suppose that your window was shattered and you came to me for an explanation. Setting aside claims of ignorance, there are multiple different reasons that I might offer for why your window is in its current state. I might point out that the various component pieces of your window are disconnected from one another. It is in virtue of this disconnectedness that your window is shattered. In ordinary conversations it is implicit that this is unlikely to be the sort of explanation that you are looking for, but it is a reason nonetheless. This sort of explanation is a metaphysical one: I am claiming that your window's current state is grounded in how its parts are distributed (widely, we can assume). Another kind of explanation I might offer is by way of telling you a story about Suzy kicking a ball through the window. I could talk about the fragility of the window, the hardness of the ball and the momentum with which it travelled through the air. If pressed, I might make appeal to Newton's laws of motion. This is an explanation of a different sort to the first. It seems likely that Loewer would be inclined to count this causal story as the basis of a scientific explanation. A further alternative exists. I might instead point out that every other house on your street has had a broken window this week and yours is no different. Here I am identifying some sort of pattern in recent events and portraying the incident with your window as another instance of that pattern.

As this is something of a toy example, let's consider a more detailed case. The following example, of Arbuthnot's regularity, will hopefully bring out the distinction that Loewer has in mind.[69] The regularity which forms our explanandum is simple: over an 82 year span from 1623 onwards, more boys than girls were born in London. Calling this mere coincidence is hardly credible given how unlikely this is to have come about by chance. But there are two kinds of explanation on offer for this regularity. Call the first the fundamental physics explanation. We may not have such

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[69] A discussion of Arbuthnot's regularity and explanation can be found in Kitcher (2001) p. 71. See Frisch (2011) for further discussion. The relevance of this example to Loewer's distinction was first brought to my attention in a talk by John Roberts.

an explanation presently available, but we can sketch out the form it might take. Take the fundamental laws of physics and assume that they are deterministic. Now take the exact state of all of the fundamental physical particles at some point prior to 1623. (Given deterministic laws it doesn't matter when; the initial conditions of the universe would suffice. We might also appeal to the position of the particles at some future time, but the sense in which that would explain why this past regularity occurred is very slim indeed.) As the laws are deterministic, we can now in principle derive the positions of the particles for any later time, and for the relevant 82 year period in particular. Associate some distributions of particles with the birth of a boy and some other distributions with the birth of a girl. Count the number of boy-birth distributions and compare it to the number of girl-birth distributions. We can clearly see that more boys than girls were born in London each year!

There is a sense in which this constitutes a good explanation of the Arbuthnot regularity. We have given the details (or, at least, the form such details might take) of what the regularity might fundamentally consist in. The regularity obtains in virtue of such-and-such fundamental physical facts (despite my having just called it a fundamental physics explanation, this is a metaphysical explanation in the sense of giving information regarding what grounds what). But it is also easy to see what this explanation misses. Most obviously, it is an 'in principle' explanation: we do not have epistemic access to the world's fundamental state prior to 1623, we do not believe we have discovered what the fundamental laws actually are and we cannot even decide whether such laws will turn out to be deterministic. But even if we had all of this information available to us, the explanation would still be missing out on something. The fundamental physics explanation does nothing to help us understand why the regularity is not some big coincidence. It might not be a coincidence in the sense of having come about by a random process since deterministic laws rule out any of that kind of coincidence. But there is still the unanswered question: why this particular fundamental distribution that generates the Arbuthnot regularity when so many others are compatible with the laws? Furthermore, the explanation explains too much. For the fundamental physics explanation not only explains the Arbuthnot regularity but also every other event and regularity that occurs after the time associated with the given state of the particles. So the very same explanans will explain my tea drinking, the first moon landing, and the rise and fall of alien empires in other galaxies. The sense in which this is an explanation of the *Arbuthnot regularity* is perhaps a rather slim one.

So there is plenty of room for an alternative to the metaphysical explanation just given. Contrast this with Fisher's neo-Darwinian explanation of the regularity. In any population which differs from a 1:1 ratio at sexual maturity, there is a selection pressure in favouring of producing more offspring of the sex that is not as well represented. If one sex in a population is more vulnerable than the other, and so less likely to reach sexual maturity, natural selection will favour a birth ratio skewed towards the more vulnerable sex. In humans, males are more vulnerable than females: boys are more likely to die before puberty than girls. Any large human population which fails to birth more boys will experience a selection pressure towards having more boys. This is why Arbuthnot observed this regularity; because of natural selection, every year is likely to be a year in which more boys are born than girls.

This scientific explanation does not generate the same sense of mystery as the metaphysical one given previously. The regularity in the London births no longer appears to be a coincidence, but rather something we should expect to hold. This is a virtue of the second explanation in that it grants a greater understanding of the explanandum than the first does.[70] We might also note that this explanation is more specific than the first. It does explain more than just a regularity in seventeenth-century London – it explains similar birth trends across different places and times – but it explains far less than the first does. By appealing to the entire Humean mosaic, the metaphysical explanation explains every regularity that holds (assuming the fundamental state of the world was given at an early enough point; even if it were given at the beginning of 1623, it would still cover a wide range of regularities). So the explanations differ in how widely applicable they are.[71]

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[70] See de Regt and Dieks (2005) for an emphatic defence of the importance of understanding in explanation.

[71] A third difference between the two explanations is that the first appeals to fundamental laws of physics, while the second appeals to the non-fundamental laws of biology. It would

2.3 The revised circularity argument

The first challenge to Loewer's distinction comes from Marshall, who complains that even if there is such a distinction, it is not obviously relevant.[72] Take proper parthood as an example. We can distinguish two different types: the first where one of the relata has a smaller volume than the other, the second where either the relata do not have volumes or they are the same size. But noticing that proper parthood comes in these different forms does not convince us that it is not asymmetric. Similarly, the fact that there are different kinds of explanation does not mean that explanation fails to be asymmetric.

The analogy here is rather weak. The distinction between different kinds of proper parthood partitions the set of proper parthood instances into two equivalence classes. Loewer's distinction does not. While every case of proper parthood will fall into one of the categories that Marshall picks out, Loewer makes no claim that the kinds of explanation he identifies are the only ones available. Moreover, being in one proper parthood category rules out being in the other. But, as we have just seen, multiple different kinds of explanation of some phenomenon can coexist. Most importantly, this sort of objection misses the point of drawing a distinction in the way Loewer does. The point is this: explanation is not homogeneous, so it makes little sense to ask whether *explanation* is asymmetric. Different explanatory relations are doing different things, although insofar as they all aid in our understanding, we are justified in referring to each as an explanation. We might sensibly ask whether any given explanatory relation is asymmetric. Perhaps Hicks and van Elswyk are right to suggest that some are, perhaps not. But even if all explanatory relations are asymmetric, we cannot make any further claim about *explanation as a whole* from this. None of this should be terribly surprising: Marshall's distinction is an arbitrary

be interesting to see a similar case being made without use of the special sciences, if that is even possible.

[72] Marshall (2015) pp. 3149-3150.

one between two different ways that some relation can be instantiated whereas Loewer is making the claim that there are entirely different relations involved.

So much for the analogy. Marshall continues:

> Moreover, no further reason to reject (A) [the asymmetry of explanation] is provided by Loewer's particular way of drawing this distinction. This is illustrated by the fact that, if we assume that all explanation is either scientific or metaphysical, and we slightly simplify his characterisation so that it holds that *f* partly metaphysically explains *g* only if *f* and *g* are co-temporal or both non-temporal, and that *f* partly scientifically explains *g* only if *f* is temporally prior to *g*, then his characterisation will entail (A) rather than refute it!

Marshall is right that if we make two assumptions then all explanation comes out as asymmetric. (If the explanans is located at a different time to the explanandum then it will be a scientific explanation, otherwise it will be metaphysical. Since whether two things are co-temporal is symmetric, any putative explanation running the other way will be of the same kind as the first. But each particular kind of explanation is intuitively asymmetric.) But this is no better an argument than the one from proper parthood as the simplification suggested is misleading. Not only has Loewer not claimed that there are no other kinds of explanation (indeed, this seems highly doubtful), but the simple characterisation of scientific explanation is simply false. Non-temporal entities can be involved in the scientific explanation of temporally located entities. The most obvious examples of this are laws! They are not normally taken to be located in time yet are clearly a major part of the scientific explanation of phenomena in the world. The simplification here shows only that we ought to be careful in how we characterise each kind of explanation so as to avoid problems like this. It does not reveal anything more about the nature of the distinction.

I suggest that what Marshall is really trying to get at is the following. Explanation is taken to be asymmetric on the basis of – presumably – intuition.[73] But Loewer's

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[73] In fairness, that asymmetry is involved in explanation might well be a particularly well-entrenched intuition such that any view which fails to introduce a requirement that there be asymmetry is not a view that captures what we mean by explanation. This is rather plausible: if explanations could be symmetric there would be cases where we would be unable to distinguish explanans from explanandum. It is not clear to me, however, that this

distinction does nothing to change Marshall's intuitions on the matter. That explanation comes in different forms does not, by itself, lessen the circularity involved in the case of Humean laws. But intuitions are a notoriously shaky ground to build an argument upon. My own intuitions differ markedly from Marshall's. I am happy to agree that explanatory relations are asymmetric, but I see no reason to move from this to the claim that if one such relation holds between two relata in one order then another kind of explanatory relation cannot hold between the relata taken in a different order. In short, my own intuitions only object when one explanatory relation is taken to hold symmetrically. Stalemate. We might take the clash of intuitions to tell us that Loewer's distinction needs additional argument if it is to show that explanation can hold symmetrically if there are different relations involved. But we might equally take it to tell us that additional argument is needed to show that the instantiation of two different explanatory relations renders any explanation unacceptably circular.[74]

Marshall complains that Humeans have not yet provided any 'independently plausible' examples where metaphysical and scientific explanation both obtain in a non-circular fashion. But neither have anti-Humeans provided any independently plausible examples of such explanations obtaining in cases that we find circular. Further, if the demand for the examples to be independently plausible requires that they not involve the very laws that we are interested in, then the demand may not be possible to satisfy. For one of the kinds of explanation under consideration is scientific explanation which involves appealing to laws in order to explain some phenomenon. There is little point in trying to meet a challenge that has been set up in such a way that ensures it cannot be met.

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well-entrenched intuition has anything to say when different kinds of explanation are at play.

[74] Perhaps it is worth noting that Marshall's conclusion follows if one already has accepted something like Lange's transitivity principle below. But we should not start out by assuming that something like this holds, that must be argued for. There are arguments that transitivity fails in the cases of particular examples of explanation, so let's not assume that it will hold across different kinds. See Paul (2000) for an argument that causal explanations fail to be transitive and Schaffer (2012) for an argument that grounding explanations fail to be transitive.

A more challenging line of response is given by Lange.[75] Grant that there is a distinction between scientific and metaphysical explanations. Grant also that the holding of some explanatory relation between *A* and *B*, and another explanatory relation between *B* and *A* does not automatically entail that either explanation is viciously circular. Even so, Loewer's distinction will not save the Humean account. The problem is that these two kinds of explanation are connected to one another by a transitivity principle.[76] Here is the original statement of it:

> If E scientifically explains [or helps to scientifically explain] F and D grounds [or helps to ground] E, then D scientifically explains [or helps to scientifically explain] F.[77]

The motivation for this update to our first transitivity principle is straightforward. If *D* is the ground of *E*, then *E* obtains because of *D*. It is only in virtue of *D* obtaining that *E* does. If metaphysical explanation is concerned with constitution, then we can say that *E* is constituted by *D*. Any role that *E* plays, it plays because of the way it is and *D* is what makes *E* the way it is. So if *E* is involved in the scientific explanation of some other fact then *D* must also scientifically explain that other fact.

This allows us the run the argument against Humeanism again. The laws scientifically explain the mosaic, while the mosaic metaphysically explains the laws. Due to this latter fact, the mosaic must scientifically explain whatever the laws scientifically explain as it is only in virtue of the mosaic being a certain way that we have these laws to explain with. But then by the new transitivity principle, this means that the mosaic scientifically explains itself.

Rather abstract considerations about the relationship between scientific explanation and grounding are not the only reason to accept this principle. Lange offers an example of this in action from evolutionary biology.[78] Fitness differences in two types

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[75] In Lange (2013). This is developed further in Lange (2016).
[76] Loewer's distinction is a way to dispute the relevance of (P4) in the formalised explanatory circle argument by claiming it only applies when all of the explanation involved are of the same type (which is not the case when we consider Humean laws). Lange's transitivity principle given here updates (P4) so as to be able to accommodate the distinction.
[77] Lange (2013) p. 256.
[78] Lange (2016).

of moth are involved in the scientific explanation of why the fitter type of moth has more offspring than the less fit type. But we cannot use the average number of offspring to ground the fitness differences in these two types of moth. This is because:

> Where fitness is defined in terms of survival and reproduction success, to say that type A is fitter than type B is just to say that type A is leaving a higher average number of offspring than type B. Clearly, we cannot say that the difference in fitness of A and B *explains* the difference in actual average offspring contributions of A and B, when fitness is defined in terms of actual reproductive success.[79]

A transitivity principle of some kind is involved here as the authors quoted are evidently keen to avoid self-explanation. Lange takes it that the kind of transitivity principle just given fits this case nicely. Pointing out that fitness differences scientifically explain average offspring number while average offspring number metaphysically explains fitness differences does not seem to help. That said, I do not find it all that clear how analogous this case is. Notice that a key part of why explaining fitness differences and average offspring number in terms of one another is unsatisfactory is because the former is defined by reference to the latter. The example Lange gives is motivation to accept a transitivity principle when there are definitions involved. But it would be misleading to say that Humeans *define* laws in terms of the mosaic.

While I do not take this particular example that Lange gives to be sufficient motivation to think his principle holds, he does offer others. Further, Harjit Bhogal makes the case that even if the Humean finds examples where this sort of explanation transitivity fails, we still need an account of why this is sometimes taken to be possible:

> For example, we can chain together (i) a scientific explanation of the facts about the energy of the particles in this room from facts about those particles ten minutes ago, with (ii) a metaphysical explanation of the fact about the current temperature of the room in terms of facts about the energy of the particles in the room, to form (iii) a larger scientific

[79] Mills and Beatty (1979) p. 265.

> explanation of the fact about the temperature in this room from the facts about the particles ten minutes ago.[80]

Clearly this sort of chaining together of explanations so as to form a scientific explanation from the grounds of some other explanatory fact is sometimes done. This is *prima facie* motivation to think that Lange's transitivity principle – or something very much like it – does hold. But the importance of these sorts of examples is not merely to motivate that principle. Rather, they give us a better understanding of the challenge that the Humean faces: the project is not only to show that the transitivity principle does not hold generally, but to do so in a way that does not condemn all instances of such explanation transitivity. It must remain possible, after the Humean has offered a defence of the distinction between different types of explanations, for this chaining together to sometimes be a legitimate move. Taking a straightforward example, this rules out claiming that the scientific explanation of some explanandum has a unique explanans. An argument for this would rule out the transitivity principle, since *E* and the grounds of *E* would not be able to both scientifically explain some *F*, but would leave it completely mysterious as to why we think explanations like the one in Bhogal's example are legitimate.

With this discussion in mind, we can now consider the explanatory circle argument that Humeans adopting Loewer's distinction must face:

[80] Bhogal (2016) p. 3.

(P1′) The natural laws are generalisations.

(P2′) The truth of generalisations is (partially) metaphysically explained by their positive instances.

(P3′) The natural laws scientifically explain their instances.

(P4′) If *A* (partially) metaphysically explains *B* and *B* (partially) scientifically explains *C*, then *A* (partially) scientifically explains *C*.

(C1′) The natural laws are (partially) metaphysically explained by their positive instances.

(C2′) The instances of laws scientifically explain themselves.

Given the similarity of this updated argument to the previous one, there is little need to explain the premises. Let us take this to be the objection that Humeans are challenged to overcome. The question now is how best to do that. The next two sections consider attempts in the literature that fall short of the task before we turn to responses that will do the job, if we are willing to make the relevant claims.

2.4 Irreducibility of the special sciences

The first line of response available to Humeans is given by Hicks and van Elswyk.[81] This is the worry that Lange's transitivity principle amounts to a claim that macro-explanations (those given by the special sciences) are always reducible to micro-explanations (those given by lower-level sciences, ultimately fundamental physics). Suppose that some *E* scientifically explains *F* and that *D*, the phenomena grounding *E*, thereby scientifically explains *F*. Since *D* is the grounds of *E*, it is likely that (at least some of the time) *D* and *E* will be involved in the explanations offered by different sciences. This is a part of seeing the world as coming in levels which differ in their fundamentality: the grounds for some phenomenon will typically be more fundamental than the phenomenon itself, and it is the job of the more fundamental sciences like physics to investigate the more fundamental phenomena.

[81] Hicks and van Elswyk (2015) pp. 440-441.

Suppose that *D* is the sort of object studied by physics and *E* is part of a biological explanation of some event. Then by the transitivity principle, there is an explanation of that event available from physics. But, so the objection goes, this rides roughshod over the usual debates concerning the reducibility of the special sciences. If one sees the world as structured according to these levels, with physics describing the bottom one and various other sciences describing the higher-levels, the project of spelling out exactly how these levels interact and relate to one another is an interesting and substantive one. By taking the reducibility of facts to entail the reducibility of the explanatory relations concerning those facts, Lange is claimed to be committed to trivialising such a project. Of course, there might still be work to do be done when it comes to finding what the correct lower-level explanation is whenever we have a higher-level explanation. But we no longer need to ask any deep questions about the relationship between the various levels: higher-level ones are ultimately reducible to the level described by physics. But that is too quick a move, even if one agrees with the conclusion. The reducibility of the special sciences is not something that can be convincingly proven from a principle connecting metaphysical and scientific explanations.

Unfortunately, this response will not help Humeans to refute the transitivity principle. According to that principle, if *E* provides a macro-explanation of an event and is grounded in *D*, then *D* will provide a micro-explanation of that event. So we should not expect macro-explanations to be the unique explainers of events. But that is not the same as saying that in such a case there is no macro-explanation of the event. The transitivity principle is compatible with there being multiple scientific explanations of a single event. If the macro-explanation provides some sort of explanatory contribution that the micro-explanation does not provide, then it is entirely reasonable to think that the macro-explanation is not reducible to the micro-explanation.[82] Someone who accepts the transitivity principle may wish to go further and claim that the special sciences are reducible to physics. But while the principle is compatible with that move too, it does not require it.

[82] This point is made in Lange (2016).

Recall the example of the Arbuthnot regularity given earlier, where I suggested that the birth regularity can be explained through appeal to fundamental physics or natural selection. The fact that the biological explanation is a scientific one and is grounded in the goings-on studied by physics entails by the transitivity principle that there is a scientific explanation of the birth regularity that could be given by appropriately informed physicists. That would, of course, be the in-principle explanation suggested that appealed to the positions of the world's fundamental particles at some past time. But as the discussion of the example hopefully made clear, the existence of such an explanation does nothing to make us think that there is not still a neo-Darwinian explanation available. Nor is Lange committed to such a claim. The apparent irreducibility of the biological explanation arises from the explanatory contributions it makes that the physics explanation does not. The biological explanation is stable over a (small) range of different initial conditions of the universe, whereas the physical explanation, in appealing to the exact positions of the particles, is not. This is the sense in which the appeal to natural selection makes the regularity seem like less of a coincidence, since it would still hold if the world were different in appropriately small ways. Since it makes no appeal to the regularity occurring in seventeenth-century London, it also unifies relevantly similar explanations about birth rates at other times and places. As the explanation from fundamental physics does not make these sorts of explanatory contributions, we should not expect the neo-Darwinian explanation to be reducible to one of a lower-level science (even if the transitivity principle tells us that a lower-level one does exist).

2.5 Multiple realisability

Here is a different reason to reject the transitivity principle: it gives the wrong results in cases of multiple realisability.[83] Again we appeal to that picture of the world

[83] I will be largely following the presentation of Hicks and van Elswyk (2015) pp. 437-438. A similar response is offered by Miller (2015) pp. 1320-1328. Miller does also suggest that Humeans could repeal the ban on self-explanation. However, as she herself acknowledges

coming in levels, with different sciences studying different levels. Suppose that *E* is a fact that turns up in an explanation offered by a higher-level science of some phenomena *F*. Fact *E* forms the scientific explanation of *F*. But rather than worry about whether *E* is reducible to some lower-level fact as we did in the last section, let us instead suppose that *E* is multiply realisable. That is, there are multiple different lower-level facts that would be capable of grounding *E*. As such, fact *E*'s obtaining is consistent with the failure of each of these lower-level facts to occur (although presumably not with the failure of *all* of them). Finally, some of these *D* facts will be compatible with different *E* facts. So the occurrence of one of these lower-level facts does not automatically entail this particular *E* fact.

The charge is that in cases like this, Lange's transitivity principle will predict that *D* will be part of a scientific explanation of *F*. But this often seems to be the wrong kind of result. When examining such a case we often think that scientific explanations of the phenomena *F* will not mention *D*. Hicks and van Elswyk's Lion example makes this point clear:

> The position of electron *e* partially metaphysically explains the position of lion *L*. The position of *L* scientifically explains the number of prey animals in region *R*. But the position of electron *e* does not explain the number of prey animals in region *R*. For if the electron were elsewhere, *L* would still be warding prey animals out of *R*.

The example meets the criteria just mentioned. The lion's existence is multiply realisable, in that it is consistent with there being different underlying particle configurations. And many of the facts that serve as partial grounds of the lion (such as the presence of electron *e*) are compatible with there not being a lion there at all as opposed to, say, a tree. There's nothing particularly special about this example; it is easy to see how further examples with the same general form can be given.

Crucially, this example appeals to a counterfactual: had the electron not been present, the prey-warding explanation would be unaffected. In other words, had grounding fact *D* not obtained, fact *E* would still scientifically explain fact *F*. Since the

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and I noted in the first section of this chapter, such a Humean is still left saying very odd things about explanation. Better to not have to make such a desperate move.

holding of the scientific explanation is independent of whether the electron is present, it is concluded that the electron's presence cannot be part of a scientific explanation of the number of prey animals. One might worry, as Marshall does, whether we ought to have such faith in that sort of counterfactual test.[84] Cases of causal overdetermination also seem to fail the test. For example, Suzy and Billy throw a rock each at a window. The rocks hit the window at the same time, at which point it duly shatters. In explaining the breaking of the window, I should mention the rocks impacting it. The fact that the breaking of the window was overdetermined – had either rock missed it would still have broken – changes nothing. It would be incorrect to announce of either rock that since the window's breaking is independent of that rock hitting it, the rock should not feature in a scientific explanation of the breaking. So checking for counterfactual differences in such a simple way cannot be the right test for whether *D* can scientifically explain *F*.[85]

This should not be an especially surprising result. The Lion example is not too dissimilar in the relevant respects from the example of Arbuthnot's regularity. Ratios of males to females in a given population are multiply realisable, since a given population might be made up from any one of multiple different particle configurations. The failure of any one particle to have its actual position would not change the ratio in the population. But that alone does not mean that we have good reason to claim that it is impossible for physics to explain the regularity.

Further, Hicks and van Elswyk do not apply their counterfactual test to cases of metaphysical explanation.[86] After all, the existence of the lion is independent of electron *e*'s presence, but the electron is still taken to be a part of the metaphysical explanation of that lion. One might take this to be further evidence that the counterfactual test is a poor one. But that conclusion might be a step too far. After all, there is a sense in which the electron's presence is not part of why there is a

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[84] Marshall (2015) p. 3152.

[85] We might wish to rule out cases of causal overdetermination through appeal to something like Kim's (1989) principle of causal exclusion, which denies that there are ever two complete causal explanations of a single event. But such a principle is highly controversial and so of limited use in defending Humeanism generally. Moreover, it does nothing to respond to Lange's point regarding contrastive explanations, which I deal with shortly.

[86] This is pressed in Lange (2016) fn. 2, where Lange credits the point to Chris Dorst.

certain number of prey animals in the area, and that sense does seem connected to the fact that it doesn't matter whether the electron is there or not. That sense does strike me as distinct from the sense in which the lion does not require *e* in order to exist. So perhaps there is room for Hicks and van Elswyk to reply with a more sophisticated counterfactual test, one that is sensitive to cases of causal overdetermination and makes it clear why metaphysical explanation does not seem to require counterfactual dependence in the same way scientific explanation might. Unfortunately, at this point I have little idea what such a test would look like.

In any case, there is a further problem facing the defence by way of the Lion example. As Lange presses, explanations are often contrastive.[87] An explanation of some phenomenon might well appeal to reasons that make it clear why some other *X* did not occur, but not even attempt to show why it was not *Y* that occurred. Taking up an earlier example, my preference for tea over the other hot drinks in my house would feature in an explanation for why it is tea that I was drinking (as opposed to, say, coffee). But an appeal to this sort of preference would not help to explain why I was having a drink at all. Similarly, a preference for coffee would explain a coffee drinking incident rather than a tea drinking one, but would still not help explain why a drink was being had in the first place. In order to explain that fact, we could appeal to my thirst. My being thirsty – as opposed to my being satisfied – would explain why I was drinking a drink of some kind, but would not be sufficient to explain why it was tea.

Neither the original nor the revised transitivity principle appealed to in the explanatory circularity argument mentioned contrasts explicitly. But it is easy to modify the fourth premise to take into account the contrastive nature of explanations:

(P4$_C$) If the fact that E rather than *E*' (partially) scientifically explains the fact that *F* rather than *F*', and if the fact that *D* rather than *D*' (partially) metaphysically

[87] Ibid.

explains the fact that *E* rather than *E*', then the fact that *D* rather than *D*' (partially) scientifically explains the fact that *F* rather than *F*'.

It is easy to see why Lange originally went for the previous, more readable version. Nevertheless, this is simply the result of making each step in the non-contrastive version explicitly contrastive. Lange goes on to offer an example by way of motivation for thinking that this principle still holds. Imagine a seesaw whose centre of mass is over its base of support rather than slightly to the right. This helps to scientifically explain why the seesaw is balanced rather than tipping to the right. The fact that Jones sitting on the right weighs 90 pounds rather than 120 pounds helps to ground the fact that the centre of mass is over the base of support rather than to the right of it. By ($P4_C$) above, the fact concerning Jones' weight helps to scientifically explain why the seesaw is balanced rather than tipping to the right. The claim that Jones' weight is involved in a scientific explanation of the seesaw's balancing rather than tipping to the right seems to be correct.

This new transitivity principle is then deployed by Lange to show why transitivity does not give the wrong result in the Lion example. In order to get the result that Hicks and van Elswyk expect, this principle must be combined with two premises: (i) The lion's presence at rather than absence from the region helps to scientifically explain why there are few rather than many prey creatures there, and (ii) Electron *e*'s presence at rather than absence from a given location helps to metaphysically explain the lion's presence in rather than absence from the given region. While (i) is true, (ii) is false. Electron *e*'s presence does help to metaphysically explain the lion's presence. But the relevant contrastive claim does not hold: *e*'s presence rather than *e*'s absence does not help to explain why the lion is present rather than absent. Here Lange also appeals to a counterfactual test. Had electron *e* been absent, the lion would not also have been absent since it can very well exist without that electron. Therefore, *e*'s absence cannot help to metaphysically explain the lion's absence. The upshot of this is that accepting ($P4_C$) does not commit one to the conclusion that electron *e*'s presence rather than absence helps to scientifically explain the number of prey animals in the area.

Suppose that both *Fa* and *Ga* are included in the mosaic. Then, in conjunction with the rest of the mosaic, *Ga*'s presence rather than ¬*Ga*'s presence helps to metaphysically explain why 'All *F*s are *G*' is a law (if ¬*Ga* held, then not all *F*s would be *G* and so the universal generalisation could not be a law). The fact that 'All *F*s are *G*' is a law rather than not a law helps to scientifically explain why *Ga* rather than ¬*Ga*. By ($P4_C$), this means that *Ga*'s presence rather than ¬*Ga*'s presence helps to scientifically explain why *Ga* rather than ¬*Ga*. But this is blatantly a case of self-explanation. So Lange's revised transitivity principle generates a challenge for Humean accounts of law while not being vulnerable to problems arising from multiple realisability. If we are to resist the implausible conclusion, we must look elsewhere.

2.6 Direction of grounding

Disputing the transitivity principle is not the only way in which Humeans might resist the explanatory circularity argument. After all, that principle only causes trouble if we accept that there is two-way explanation – regardless of whether those explanations are of the same kind of not. Suppose that we wish to hang on to the argument's third premise: we want to be able to say that the laws explain their instances. We could then re-examine the second premise, which claims that generalisations are (metaphysically) explained by their instances. If this does not always hold, and in particular if it failed for laws, then there would be no explanatory circle. For then we would only have one-way explanation, which is no problem at all. That line of thinking is the focus of this section.

There are two main ways in which laws might not be explained by their instances. The first is what Miller refers to as 'groundless Humeanism'.[88] This is a Humean who takes there to be no metaphysically explanatory relations holding between laws and their instances. The second, again following Miller, is 'contrarian Humeanism'.[89] This

[88] Miller (2015) pp. 1318-1320.
[89] Ibid. pp. 1328-1331.

is a Humean who thinks that there are metaphysically explanatory relations holding between laws and their instances, it is just that they do not run in the usual direction. Instead, laws are involved in metaphysically explaining their instances.

Let us deal with the groundless position first. It is important to be clear that such a Humean is not advocating for a failure of supervenience. A core part of standard Humeanism regarding laws is the idea that the laws are not 'free-floating' or independent of how the world is. Rather they are dependent on the world in the sense that variation in what the laws are must be accompanied by variation in how the underlying worldly facts are. In other words, the laws supervene on the world. This supervenience is asymmetrical, as not every variation in the worldly facts will be accompanied by a change in the laws. Consider how little difference the presence of the electron made to the lion: a world with one fewer particle in it might well exhibit the same pattern of zebra hunting as ours. (This is just as well, or else no two possible worlds containing different events will have the same laws. Consequently, our world would end up being the only physically possible world, since no other world would have the same physical laws.)

No part of this commitment to asymmetrical supervenience requires a further commitment to grounding claims. One fact supervening on another does not require one fact to have some form of metaphysical priority over the other. This suggests a straightforward way out of the debate: accept that laws scientifically explain their instances but deny that they are grounded in those instances. Since there is no metaphysical explanation at work here, there cannot be an explanatory circle.

Such a Humean need not deny that there are any grounding relations in play at all. So far I have been loosely talking of whether instances metaphysically explain laws. For a fan of the Best System Account, however, the laws are not determined by individual events but by the mosaic as a whole. Call *C* the fact conjoining all the individual events that make up the mosaic. Then as Miller rightly points out, a 'groundless' Humean might still wish to say (i) that *C* grounds the laws, even though *C* is not grounded in the individual mosaic facts, or (ii) that those latter facts ground *C*, even though *C* does not ground the laws. Thus a groundless Humean need not be without grounding at all, as long as there is not a clear chain of grounding from the local facts up to the laws. This is something of a minor point however. If one were worried by the thought of Humean laws not being grounded in local facts, I very much doubt that either of the above options will assuage those concerns. The first option involves a commitment to conjunctions not being grounded in their conjuncts. The second is really just a restatement of the groundless Humeanism position taking into account the point that it is the entire mosaic that determines the laws.

Given that both Lange and Loewer do not stop to mention this distinction between grounding and asymmetric supervenience, we might worry that the position just suggested does not capture the kind of Humeanism that they are interested in. Put another way: is groundless Humeanism sufficiently Humean? If not, then while one might be able to call oneself a Humean while using the groundless defence to avoid explanatory circularity, actual Humeans might still be vulnerable to the argument.[90] To support the idea that this is a genuinely Humean position, Miller quotes David Lewis at length:

> Imagine a grid of a million tiny spots – pixels – each of which can be made light or dark. When some are light and some are dark, they form a picture, replete with interesting intrinsic gestalt properties. The case evokes reductionist comments. Yes, the picture really does exist. Yes, it really does have those gestalt properties. However, the picture and the properties reduce to the arrangement of light and dark pixels. They are nothing over and above the pixels. The make nothing true that is not made true already by the pixels. They could go unmentioned in an inventory of what there is without thereby rendering that inventory incomplete. And so on.
>
> Such comments seem to me obviously right. The picture reduces to the pixels. And that is because the picture supervenes on the pixels: there could be no difference in the picture and its properties without some difference in the arrangement of light and dark pixels. Further, the supervenience is asymmetric: not just any difference in the pixels would matter to the gestalt properties of the picture. And it is supervenience of the large upon the small and many. In such a case, say I, supervenience is reduction.[91]

[90] I should note in passing that anti-Humeans who take the laws to be regularities plus some additional factors might find this sort of defence to be an attractive one. This will obviously depend on their overall commitments, but there's no in-principle reason why such anti-Humeans could not utilise Miller's suggestion for their own ends.
[91] Lewis (1994b) pp. 413-414.

This is taken to support the claim that Lewis had nothing stronger in mind when setting out his position. And if Humeanism's most famous defender was not making grounding claims, it would be unreasonable to claim that a Humeanism without grounding is not Humeanism at all.

Of course, this does nothing to protect actual Humeans. Many of them do seem to have something stronger in mind than Miller's reading of Lewis. Bhogal and Perry explicitly introduce a *fundamentality* claim into their overview of Humeanism.[92] Loewer takes Humean Supervenience to be claiming that contingent properties are instantiated *in virtue of* what is instantiated in the mosaic.[93] Maudlin, no Humean himself, takes the mosaic claim to be one of *determination* by a separable physical base.[94] Nor is the passage quoted above especially clear on the matter: the 'nothing over and above' claim and the statement that the picture is not a separate entry into the world's inventory both suggest that something stronger is at play here.[95]

Advocates of a stronger form of Humeanism appear to be taking the mosaic metaphor more seriously: 'all there is to the world is a vast mosaic of local matters of particular fact, just one little thing and then another.'[96] Contemporary Humeans are not always in agreement with Lewis regarding what the mosaic is made of.[97] But the crucial point here is that the mosaic – however it may be – is all there is to the world. Extending the metaphor, if God were to set out the world's mosaic, then there is nothing else that He would need to do. Tables and chairs, relations of causation and even laws do not need to be added in by hand afterwards. This is stronger than mere covariance; there is no talk of how things are at duplicate worlds here. Setting

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[92] Bhogal and Perry (2015) note 2.
[93] Loewer (1996) p. 102.
[94] Maudlin (2007a) pp. 51-2.
[95] This is not an exegetical work, so I do not intend to argue for a particular stance on what Lewis' views really were. I will mention a cautionary point though: we ought to be careful when interpreting work that both informs and precedes contemporary debates. The current literature on grounding and metaphysical explanation was not available when Lewis was first developing his form of Humeanism. We ought not, therefore, take a lack of references to grounding to mean that something like grounding was not intended (or would have been intended).
[96] Lewis (1986a) ix.
[97] Recall the first chapter, where we noted some of the modifications that philosophers have suggested need to be made to the mosaic in the wake of quantum physics.

the mosaic down determines everything else. Even if this is not an explicit appeal to the metaphysical priority of the mosaic, it is something very close to it. For such a Humean, the account rescued by Miller's groundless Humeanism will not capture what they are reaching for.

That is not to say that groundless Humeanism is incoherent or indefensible. It is neither, and it makes a substantial claim about the way the world is. But adopting it does require that many contemporary Humeans abandon their current views in favour of something weaker. As such, I take this to be a possible response to the explanatory circle argument, but not an ideal one. The next view I shall examine perhaps fares even worse in this regard, but is worth discussing nevertheless.

Take the global facts about the mosaic to include both conjunctions of the usual local facts and generalisations about the mosaic. This means that laws will be counted as global facts since they are generalisations for Humeans. For Humeanism to hold, the global facts must supervene on the local facts. Further, this supervenience is asymmetric as the local facts do not supervene on the laws (two worlds might have the same laws while having different events in their histories). But this does not mean that the local facts do not supervene on these global facts more generally. In fact, we should expect them to because the global facts include conjunctions of local facts. There cannot be a change in the local facts without there also being a change in what the conjunctions of local facts are. So while the supervenience between local facts and laws is asymmetric, the supervenience between local facts and global facts is symmetric.

At this point a groundless Humean will stop and say that this is enough to characterise the view; there is no need to introduce grounding claims. But a contrarian Humean will go further: the symmetric supervenience between local and global facts does not have to be taken to mean that there is no grounding at all. Rather, the grounding might run in the opposite way to how it is usually taken to go. That is, the local facts could be taken to be grounded in the global facts. This includes the laws, which would partially ground the local facts. It is still the case that the local facts do not supervene on the laws, which might be why their grounding in the laws is only partial: the laws will help to ground local facts when taken with other global facts, but cannot do it by themselves. This is supposed to mirror how grounding is usually taken to work in the standard Humean account. There the laws are grounded in the entire mosaic – in all the local facts taken together. The laws are not grounded in just a few local facts and neither will they in general supervene on such a restricted base. Just as the laws are not expected to be grounded in a restricted portion of the local facts on the standard Humean account, the local facts will not be grounded in a restricted portion of the global facts (that is, the laws) on the contrarian Humean account.

The view held by Miller's contrarian includes all of the generalisations about the mosaic in the global facts. This includes the laws and the accidental regularities, both of which might be involved in the grounding of some local fact. But this is not the only possible contrarian position. Bhogal suggests a similar view, where the grounding relations can be used to distinguish laws from accidents.[98] Take the accidental regularities to be grounded in the local facts as normal. The classic generalisation 'everyone in this room is a philosopher' is an example of this. It holds because of the philosophical status of each person in the room, where 'because' is understood to indicate a grounding fact. (We might note that taking grounding to run in the opposite direction seems mistaken: it is not the case that everyone in the room is a philosopher *because of* the regularity. Not with the same sense of 'because' at any rate.)

The laws are then distinguishable from the accidents by virtue of not being grounded in their instances. Bhogal's example here is of the law that $F = ma$, which he does not see as grounded in local facts in the same way that the accidental regularities are. While accidents are grounded in their instances, instances of laws are grounded in the laws. In support of this disunified treatment of generalisations, we could point to the fact that laws and accidents are commonly taken to play different roles when we are dealing with, say, explanations or counterfactuals.

That said, I take Bhogal's claim that this is motivated by how we ordinarily conceive of laws and grounding to be rather shaky. It is true that $F = ma$ does not

[98] Bhogal (2016).

immediately seem to be grounded in its instances in the obvious way that everyone in the room being a philosopher is. But much of that might be in the presentation. Consider instead the generalisation that all objects accelerate proportionally to the net force they are experiencing and inversely proportionally to their own mass. It is far less clear to me that there is a relevant distinction in grounding between that statement of the regularity and the statement of the accidental one. More forcefully, recall Lange's example of the law that all sodium salts burn with a yellow flame. It is not immediately apparent to me that this ought to be grounded differently to the one concerning the room of philosophers.

Such concerns suggest that the motivation for the view is not as clear-cut as we might like. But that alone is not enough to rule it out. Theoretical virtue might tell in its favour. Solving the explanatory circle problem for Humeanism would be a mark in its favour. In common with Maudlin's primitivism, it also does not seek to understand fundamental laws in terms of anything else.[99] This brings it closer to scientific practice, where physicists do not typically look for reductions of laws that they take to be fundamental. Having a view on some part of science be closely aligned to scientific practice is also plausibly a virtue.[100]

This is a more straightforward conception of the grounding relationship between laws and local facts than Miller's. Her contrarian does not ground the local facts in laws alone, but in the global facts all together. It is less clear whether this is an advantage or not. Both are consistent with the Humean desire to have the laws supervene on the local facts. Both seek to avoid the explanatory circle argument by undermining the second premise: since laws are not metaphysically explained by their instances, having them scientifically explain their instances does not create any form of explanatory circle. Whether a contrarian favours Miller's or Bhogal's version of the view will depend on whether that contrarian finds it plausible for the local facts to be grounded in the laws alone. For my part, I have no view on the matter.

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[99] Maudlin (2007a).
[100] Of course, the willingness of Humeans to reject what scientists say when they talk – perhaps unreflectively – of governance or constraint suggests that Humeans do not see being closely aligned to all of scientific practice as a great virtue. A tendency towards this sort of revisionism is part and parcel of the view.

Fortunately, this difference plays no role in the usefulness of contrarianism in the present debate and can be set to one side.

Marshall also offers a suggestion in a similar spirit.[101] As already indicated, Marshall prefers to avoid casting the debate in terms of grounding and has little use for Loewer's disambiguation of explanation strategy. However, his position has a natural equivalent in those terms. Given that I am describing the issue in that way, I will give the analogous version. The crucial point here is that we can draw a distinction between some law and the fact that it is a law. Suppose that it is a law that all *F*s are *G*s. Then there is a difference between the law that all *F*s are *G*s and the fact that all *F*s are *G*s is a law. Most obviously, Humeans will take the law to be a regularity whereas the latter fact is not itself a regularity. Rather, the fact that all *F*s are *G*s is a law is a higher-level fact ascribing lawhood to some regularity. Hence this fact concerning lawhood cannot itself be a law. We might also note that it is possible for the regularity to hold while the lawhood fact fails to obtain: this is exactly what happens in the case of accidental regularities. Therefore, they cannot be the same thing.

This distinction matters because of how these are both related to explanation. The law that all *F*s are *G*s can be expected to turn up in scientific explanations of worldly local facts, like all laws. However, Marshall claims, instances of this law – such as *Ga* – do not (metaphysically) explain the law itself. This latter claim unfortunately is not supported by an argument, other than to note it is not *required* for instances to explain laws. I take this to be where Marshall's view has much in common with Miller's and Bhogal's, for it too is committed to the claim that not all regularities are explained by their instances (or, indeed, grounded in them). By contrast, the fact that all *F*s are *G*s is a law is explained by its instances. This is because the lawhood facts are determined by what the best system is, and this in turn is determined by the local matters of fact in the mosaic plus the particular standards used for judging which systematisation is the best. The explanation, then, is metaphysical. But the lawhood fact does not scientifically explain local matters of fact. This is because it is not

[101] Marshall (2015) pp. 3157-3161.

concerned with them. Facts ascribing lawhood to regularities are concerned with which universal generalisations appear in the best system.

We might wonder why actual scientific explanations do appeal to facts about which regularities are laws. After all, a sensible response to the question "Why did this powder burn with a yellow flame?" might well mention the fact that it is a law that all sodium salts burn with a yellow flame. But there is a straightforward way to interpret this. The lawhood fact is not itself an explainer here, but it does provide information regarding what the real explainer is: this powder has something in common with other sodium salts that causes all of them to burn yellow. The response is a partial answer to the question that points to where the rest of the answer lies (the burning of the sodium salts causes a change in the energy levels of electrons, which is associated with the release of photons with a wavelength that we see as yellow).[102]

So on this view the laws scientifically explain their instances while not being metaphysically explained by them in turn. There are also facts ascribing lawhood to regularities: these are metaphysically explained by the instances of the relevant law, but are not scientific explainers of those instances. This is not an explanatory circle, even granting the transitivity principle.

On Marshall's account, this raven's blackness helps to explain why it is a law that all ravens are black. The fact that it is a law that all ravens are black requires that the all ravens are black regularity holds (a generalisation must be true in order to be a law). The truth of this regularity helps to explain why this raven is black. But while, strictly speaking, the circle fails to obtain, we might worry that lawhood facts are too tightly associated to the scientific explanations that they cannot be allowed to feature in. Lange gives voice to this concern:

> [W]e do not have to figure out whether the lawhood of All F's are G helps to explain why it is that Ga or merely helps to explain why it is that Fa helps to explain why it is that Ga. The prohibition on self-explanation should be interpreted not only as prohibiting a fact q from helping to explain itself,

[102] This example aligns with the treatment in both Lange (2016) and Skow (2016) pp. 139-40.

> but also as prohibiting q from helping to explain why (if q obtains) some other fact helps to explain q. Both of these are too circular to qualify as explanations.[103]

Does the blackness of this raven help to explain why some other fact helps to explain the blackness of this raven? Well, it is supposed to help to explain the lawhood of all ravens are black. And it is explained by the relevant regularity. So the connection between the law and the fact of its lawhood is important. If the lawhood fact helped to explain the regularity then we would have a straightforward chain of explanations from this black raven around in a circle back to this black raven. In order to avoid self-explanation, Marshall is then committed to the claim that the fact that *A* is a law does not help to explain why *A* obtains (even though it entails that *A* must obtain). This has the advantage of getting around the updated prohibition, since it means that there is no single other fact that both explains the raven's blackness and is explained by the raven's blackness.

But even if there is an explanatory gap between laws and the facts of their lawhood, this sort of case is clearly of the kind that Lange intends to rule out with the revised prohibition on self-explanation. There is some intuitive force to this: getting out on a technicality does not remove the suspicion that something explanatorily untoward is going on. But suspicions are not arguments. Lange does offer some precedent for widening the scope of what we count as circularity. Salmon argues that there are two ways for an argument to be circular.[104] The first is for the conclusion to turn up in the premises. The second is to use a rule of inference in an argument to conclude that that very rule of inference is a reliable one. Similarly, Lange takes there to be two ways in which an explanation might be broadly circular. The first is for a fact to turn up in the explanation of itself. The second is for a fact to explain which some other fact turns up in an explanation of the first fact.

But while there might be a structural analogy in this kind of scope widening, it is not clear that this suffices to justify such a move. Salmon was taking a broader view of what counts as unhealthy circularity in the context of inferences, and it is clear to

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[103] Lange (2016).
[104] Salmon (1967).

see the motivation for doing so. Both ways that he identifies are ways of assuming the very point at stake, albeit in different forms. That's not what is going on in Marshall's account.

Here is another way of raising the circularity worry: how is it that the blackness of this raven explains the fact that it is a law that all ravens are black? Supposing that it does, we might wonder what exactly is being explained (taking explanation to be contrastive, this is to look for the relevant contrast). Perhaps it is why a certain regularity is a law, as opposed to a mere accidental regularity. If so, particular instances of black ravens look irrelevant to the explanation since both the explanandum and the contrast state of affairs assume that the regularity holds. The information relevant to this explanation concerns the Best System Account and the standards being used for judging if a system containing the regularity is the best. So the blackness of this raven does not explain the lawhood fact in that way.

Perhaps, then, the relevant contrast is not between the regularity being a law or an accident but rather between the regularity obtaining or not. For a regularity to be a law it must first be true. So if the blackness of this raven helps to explain why the regularity obtains, it could plausibly be said to be involved in the explanation of the lawhood of that regularity. But here we have a problem. Marshall's view is committed to lawful regularities (scientifically) explaining their instances. If those instances also (metaphysically) explain the regularities, transitivity brings us right back at the circle we started with. So the blackness of this raven does not explain the lawhood fact in that way either.

I do not see another way in which instances of laws can explain the associated lawhood facts. The obvious conclusion of this is that they simply do not. We are left with a picture where the laws scientifically explain their instances but are not in turn metaphysically explained by those instances. This leaves it open as to where we should take there to be grounding relations. One option is that there are not any, in which case Marshall's view is that of a groundless Humean. The other option is that the instances are grounded in the laws, making this another way of spelling out the contrarian position.

Roughly speaking, for there to be an explanatory circle there must be both explanation of the laws by the mosaic and explanation of the mosaic by the laws. Contrarian positions neatly sidestep the debate regarding the transitivity principle because they do not take explanations to run in both directions. Rather, the laws are taken to both scientifically and metaphysically explain the mosaic. Since explanation therefore runs in only one direction, there cannot be a circle. But such views are not without costs and I shall now mention the two main ones.

The first downside to contrarianism is that it requires us to give up on otherwise attractive ideas regarding metaphysical explanation. It is standard (for those who accept the notion of grounding) to take conjunctions to be grounded in their conjuncts. Fine is a particularly clear example of this. To introduce the notion of grounding he uses the following example: the fact that the ball is red and round obtains in virtue of the fact that the ball is red and the fact that the ball is round.[105] The 'in virtue of' locution here is intended to bring grounding to mind. But on Miller's presentation of the view, this does not hold. Global facts – including conjunctions of local facts – are not grounded in those local facts, but instead serve as grounds for them. The ball is red and the ball is round in virtue of the fact that the ball is both red and round. That is an odd consequence, as we normally take more complex facts to be grounded in the less complex ones rather than vice versa. However, it is not an unprecedented move. Schaffer's priority monism is an excellent example of a view that takes the more complex to serve as an ontological foundation for the less complex.[106] Alternatively, one might hold the view that conjunctive states of affairs are more fundamental than their derivative conjuncts. For example, the ball in front of me is both red and round. To appreciate each of these qualities individually, I need to 'abstract' away from the actual situation. Ultimately, it is the ball (which is both red and round) that grounds the truth of various ball-concerned propositions. Someone with such a view might well claim that it is only the influence of logical atomism that inclines us to think that the ball's being red is more fundamental than the ball's being red and round.

[105] Fine (2012) p. 37.
[106] Schaffer (2010a) and (2010b).

Similar things regarding grounding need to be said about the relationship between universal generalisations and their instances. A standard approach to this is to take the generalisations to be grounded in their instances. As Bhogal notes, this might not be the whole story.[107] Take two worlds, both of which contain ten entities that are both *F*s and *G*s. There is nothing else in the first world and so the generalisation that all *F*s are *G*s is true. It cannot be grounded solely in those ten entities, however, as the second world contains a further entity which is *F* but not *G*. If the generalisation were grounded in the ten entities alone, then it would also be true in the second world since those ten entities are present there too. But, obviously, it is not true in the second world. So generalisations cannot be grounded in only their instances.

Now a Humean willing to accept modal realism might well object at this point that it is not true that the same ten entities are present at both worlds. Worldly entities exist at one world and one world only. The second world does not have the same ten entities as the first, although it might well have counterparts to the original ten. But then this is no counterexample to generalisations being grounded in their instances since we do not have a situation where both worlds have the same grounds but different grounded facts.

That said, there is something to the point Bhogal makes that is not answered by that response. Even if we accept modal realism, something remains unexplained: why is it that the counterpart entities do not ground the generalisation? Counterparts often end up having rather different properties and grounding very different facts: after a period of bloody warfare, one of my counterparts becomes ruler of a new British Empire and in doing so grounds many different facts to those that I actually ground. But in this case the counterparts seem sufficiently similar to the originals (by stipulation they have the same relevant properties!) that pointing this out does not help. The most obvious way to explain why the generalisation is true in the first world but not the second is to appeal to totality facts. Crucially for this discussion, totality facts are not instances of generalisations like all *F*s are *G*s.

[107] Bhogal (2016) p. 9.

However, if the laws are to ground the local mosaic facts, then merely showing they are not grounded in their instances alone is not enough. Nothing in the above story indicates that generalisations are not partially grounded in their instances. Concluding that either there are no relevant grounding relations here or that grounding runs in the opposite direction to what we normally assume is too much. As we have already seen previously, Bhogal takes there to be independent reason to think that laws like Newton's second law of motion are not grounded in their instances. Such a position is not obviously false and there is no contradiction involved in accepting it. But – again, as already noted – I do not find it clear that grounding runs in the direction that contrarians favour in such cases. Humeans who share my uncertainty will see this commitment as being insufficiently motivated.

Lurking behind all this discussion is the most significant issue with this response to the explanatory circle. The clue, really, is in the name. So far I have used Miller's terminology and called the view contrarian Humeanism. But Bhogal's view is very similar in the crucial respects, and he favours calling it minimal anti-Humeanism.[108] A rose may smell sweet no matter its name, but this particular disagreement over names is a bit more worrying: it points to disagreement over exactly what Humeanism is about. Let us follow Bhogal in distinguishing between three different ways to characterise the Humean claim.[109] These are supervenience, necessary connections and dependence.

First the supervenience interpretation. Here the core idea is that modal facts, including laws, must supervene on a base of occurrent facts. As we have already seen, Miller favours this approach and identifies it with certain passages in Lewis' work. Since this supervenience thesis is a claim about covariation alone, it is compatible with different claims regarding the more metaphysically-laden notion of grounding. That is, the supervenience of modal facts on occurrent ones does not mean that the former must be grounded in the latter. It is also compatible with the local facts being grounded in the laws, or there being no relevant grounding relations

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[108] Marshall, by way of contrast, does not offer a name. This is because he takes his suggestion to be the natural response for Humeans generally.
[109] Bhogal (2016) pp. 11-13.

at all (as in the case of groundless Humeanism mentioned earlier). If one accepts this characterisation of Humeanism, then the contrarian position genuinely counts as Humean despite its unorthodox commitments.

The problem with this first interpretation is not so much that it cannot be true, but rather that it misses what many take to be the motivation to be Humean at all. This point was raised as an issue for the groundless defence as well: those attracted to Humeanism through a love of sparse desert landscapes and an uneasiness towards metaphysically powerful entities (such as irreducible dispositions) have something stronger than this claim in mind. For nothing in the supervenience claim says that there are no fundamental modal facts, only that there cannot be a change in them without also being a change in the occurrent facts.

The second way to capture Humeanism is to deny that there are any necessary connections in the world. Or, at least, there are not any between distinct existents. This does a better job of expressing the negative claim that Humeans make. There is nothing preventing this part of the mosaic from being independent of that part. Dispositions, for example, get ruled out on account of the link between a disposition and its manifestation.[110] While Bhogal favours the third characterisation, he does note that the view will count as Humean in this second sense. Necessary connections are only banned if they hold between distinct existents. If the local facts are grounded in the laws, then they are not existents distinct from those laws (this is presumably the sense in which grounded facts are nothing over and above grounding facts).

One might worry that something of a trick has been pulled here. Humeanism regarding laws is often contrasted with anti-Humean pictures where the laws are taken to be metaphysically productive in some sense. Metaphorically speaking, God could set down the laws for the world, then arrange some physical stuff for the laws

[110] Of course, this is not to say that a substance disposed to dissolve in water must do so in all circumstances. We would not expect it to dissolve in an already saturated sample of water, for example. So perhaps it is more that it would dissolve in ideal circumstances, with no blockers present. But even that hedged amount of necessitation is uncomfortable for Humeans.

to govern and finally sit back at watch the events unfold.[111] But the metaphor for the current account looks rather similar: God sets down the laws for the world, the physical stuff comes along as an ontological free lunch and God is once more able to sit back and watch (this metaphor is suspiciously silent on the fact that the laws underdetermine the physical events). What happened to the idea that physical events occur and the laws are mere descriptions of them?

One response to this might be to question whether grounded facts are really nothing over and above the grounding ones. Another, more radical response, is to deny that there are any necessary connections between existents at all, distinct or not. But there is a simpler solution: this is just an impoverished characterisation of Humeanism. It lacks serious commitment to something like the underlying mosaic: in writing an inventory of the world's entities, we need only write down those in the mosaic since everything else 'comes for free'. This is the dependence approach, although it might equally be called the grounding approach, and it more closely captures the orthodox Humean's position. The idea is that there is a base of local occurrent facts and all the modal ones are grounded in this (as opposed to merely supervening upon it). We might go further, of course, and distinguish between fundamental and non-fundamental occurrent facts. Then we could require that the non-fundamental ones be grounded in the base as well. But the distinction is not important for present purposes. On this characterisation, contrarians will not be counted as genuine Humeans. Neither will groundless Humeans, as both positions fail to ground the laws in the local occurrent facts.

Whether one takes the contrarian route to be a viable response to the explanatory circle argument depends on how strong a version of Humeanism one is committed to. On a minimal characterisation concerned only with supervenience, contrarianism is a viable option. More would need to be said by the defender of such view regarding why the groundless variant is not sufficient for their purposes. For my part, I see little advantage to introducing non-standard grounding relations if all one cares about is supervenience. Those Humeans who favour a stronger characterisation, where there

[111] Beebee (2000) nicely covers the God metaphor when discussing the nature of laws.

is a real sense in which everything is based on the underlying mosaic, must reject the contrarian route. This is unsurprising: if one's view is committed to grounding relations running in a certain direction, then one cannot defend this view by claiming that the grounding relations run in the opposite direction!

2.7 Is transitivity motivated?

If we set aside the contrarian route, are we then committed to the circularity argument's undesirable conclusion? Not immediately, for we might still have concerns about the transitivity principle. Earlier we saw that drawing a distinction between metaphysical and scientific explanations does little to avoid the principle, since it is possible to give a revised principle that takes that distinction into account: the worry now is that the mosaic will scientifically explain itself. We also looked at two unwanted consequences that it might be thought to entail. The first was implying the reduction of all higher-level explanations to those of physics, the second was getting the wrong results in cases of multiple realisability. The former worry is not a problem since the principle does not claim that there are no higher-level explanations or that they cannot make an explanatory contribution beyond that provided by lower-level explanations. The latter worry was responded to by noting the contrastive nature of explanation: once this has been acknowledged the principle does get the right results for multiply realisable cases.

So much for the uncontroversial cases: the transitivity principle gets the expected results in those. But the reduction of scientific laws to patterns in the mosaic is not an easy uncontroversial case. If anti-Humeans wish to apply the principle to this case, they must ensure that it has been sufficiently well-motivated. Whether it has been depends very much on how close the motivating examples are to the case we are particularly interested in. If the examples are analogous to the Humean reduction of laws then that would be good reason to think that transitivity holds. If there is a significant enough disanalogy, it becomes far less clear that Humeans ought to accept transitivity. The concern examined in this section is that there is an important difference between the cases looked at so far and the case that the argument applies the principle to.[112]

In order to assess whether there is a close analogy between the motivating examples and the Humean reduction of laws, we first need to know what those examples are. So what are the examples taken to motivate the principle? Here is a brief statement of a sample of them. An object falls over because its centre of mass is not over its base of support; its centre of mass is grounded in the masses and locations of its parts. A balloon expands because the pressure inside the balloon is greater than the external atmospheric pressure; the internal pressure is grounded in the forces with which the gas molecules collide with the balloon's walls. The temperature of the room has some value because of the state it was in ten minutes ago; the (relevant) state it was in ten minutes ago was grounded in the energy the particles in the room had. In each of these cases a scientific explanation is given (an explanation from physics, although that does not seem to be a requirement), grounds for the scientific explanans found and then a new scientific explanation using those grounds as the new explanans is offered. And in each of those cases the new scientific explanation is a plausible one: transitivity is getting the right results!

There are also a couple of examples taken from the philosophy of science literature that do not follow that pattern but where an assumption of transitivity is still clearly in play. Random genetic drift explains why the frequency of a trait in a population does not match its expectation value. But the drift cannot just be that departure from the expectation value, for it would then explain itself. Fitness differences between two groups explains why one group has a greater average number of offspring. But then those fitness differences cannot be defined in terms of the average number of offspring, for then the fitness differences would explain themselves. The chance of this coin landing heads explains the frequency with which it lands heads. But the chance cannot just be the actual frequency of heads, for then the frequency would explain itself.

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112 This line of objection was first raised to me by Steven French in discussion.

We have already encountered the example drawn from fitness differences and noticed the disanalogy to the case at hand: the Humean account of laws is not one concerned with definitions. Let us therefore set this example aside. Of the remaining examples, the one concerned with frequency and chance is of a different kind to the others. It has more in common with Humean claims about laws. Because of how close it is to the case we ultimately care about, I will delay discussion of it until the more numerous examples have been dealt with.

Let us now consider those examples. All of them have something important in common: the grounds appealed to in the new scientific explanations are what we might call 'scientifically respectable' entities. They are, in short, the sorts of things that scientists might take themselves to be studying. Take the example of the expanding balloon as representative of this. The expansion of the balloon is partially explained by the balloon's internal pressure. This internal pressure is grounded in the forces generated by the molecules of the gas inside the balloon when they collide with the walls of the balloon. Both the interior pressure and the forces exerted by the gas molecules are studied by scientists. There is a branch of physics – kinetic theory – that is concerned with the relationship between the two. What the balloon example provides is support for the claim that transitivity holds when both of the relata in the grounding relation are scientifically respectable. The other examples function in exactly the same way. When both the initial explanans and the entities it is grounded in are studied by scientists, it is reasonable to think that the grounding entities can function as the explanantia in a scientific explanation of the explananda. The grounded entities exist in virtue of the grounding ones, so one might take it as completely unsurprising that the grounding entities are able to play the same roles as that which they constitute.

However, the Humean account of laws is not like this. The laws are just the regularities recognised by the best systematisation of facts regarding the mosaic. They are grounded in the local matters of fact across spacetime. But while the laws of nature might be scientifically respectable, their grounds are not. Or, at least, they aren't in any genuinely revealing sense. To see the difference, consider the difference between discovering that pressure is not grounded in the motion of gas molecules to discovering that laws are not grounded in the way Humeanism envisages. The former discovery would call kinetic theory into doubt and require an explanation of why certain experiments give the results that they do. We would expect to see a new branch of physics emerge that incorporates the successful elements of kinetic theory while explaining gas pressures in some other way. The second discovery would spark no such scientific revolution (although it would very likely generate a good deal of philosophical interest into how exactly such a metaphysical claim has been discovered to be false). Philosophers of science would have to adopt other views of laws – perhaps taking them as primitives – but science and the practices of scientists would remain unchanged.

While (most of) the examples taken to motivate the transitivity principle may not have appealed to metaphysical accounts, a defender of the principle might still expect it to hold when they are involved. Absent any opposing argument, it is not unreasonable to think that if scientific explanatoriness is transitive over some grounding relations then it might also be transitive over others. That said, there is reason to think that the kinds of metaphysical claims that we are interested in are different enough from the scientific claims of the examples that we should not make that assumption.

Let us take a more neutral example of a distinctly metaphysical claim to illustrate the point. Bundle theorists take objects to be nothing more than bundles of properties (perhaps including relations, or alternatively tropes). Substance theorists take the directly opposing view that there is something more to objects than the properties they have. This ‘something more’ is some sort of bare substance. The entities appealed to by these accounts, be they collections of properties or substances in which those properties inhere, are not scientifically respectable entities. No scientific theory appeals to the fact that there is an underlying substance to the objects it is concerned with. It would be a mistake to try to scientifically explain some phenomenon by appealing to one of those accounts rather than the other. The forces exerted by the gas molecules in the balloon example are partially grounded in what those gas molecules are. If one is a substance theorist, then one will take each of the gas molecules to ultimately be grounded in a bare substance. But the fact that there are bare substances with various trajectories is no *scientific* explanation of the balloon's expansion.

It is a distinctive feature of these sorts of metaphysical claims that they are underdetermined by the scientific evidence.[113] Of course, it is not only purely metaphysical claims that are underdetermined by the available empirical data. Underdetermination of scientific theories is hardly a new issue in the philosophical literature. But there is a significant difference between a scientific theory being underdetermined by the current evidence that we have managed to gather and the underdetermination at play when it comes to metaphysical claims. Underdetermination in the latter case is of an in-principle kind. While we might have no way of deciding between two current scientific theories, we can remain optimistic that we will eventually have evidence in favour of one rather than the other. No such hope exists for metaphysical claims like bundle theory since both it and substance theory are empirically equivalent (trivially so, since neither makes any empirically testable claims).

This underdetermination of metaphysical accounts makes them unsuitable to play roles in scientific explanations. Since they are compatible with all physically possible states of affairs, they cannot be appealed to in order to explain why this state of affairs came about as opposed to another. It is difficult to see how they play any role in scientific explanations given that they are not difference-makers to those explanations. Whether a putative explanation is genuinely scientifically explanatory does not seem to depend on what the underlying metaphysical account of the matter is. So those metaphysical claims cannot be doing any scientifically explanatory work on account of their irrelevance to the explanation.

In making this claim, we respond to a challenge raised by Bhogal.[114] We have reason to think that there are cases where transitivity holds, such as in the balloon example as given. So if Humeans wish to argue that the transitivity principle fails, they need

[113] One might take the tension between Humean Supervenience and quantum physics as a counterexample to that claim. But, as the first chapter indicated, it is no straightforward matter to show that the former has been refuted.
[114] Bhogal (2016) p. 3.

to explain why there are cases where chaining a scientific explanation to a grounding claim generates a new scientific explanation. What has just been said goes some way towards meeting that challenge. The difference between legitimate and illegitimate applications of this chaining is in the relata of the grounding relation. Where a scientifically respectable entity is grounded in another scientifically respectable entity, we should expect for scientific explanations to be transitive over that grounding relation. If it is instead grounded in a distinctly metaphysical entity, then we have no good reason to expect transitivity to hold. This is not a full response to Bhogal's challenge, as meeting that would require spelling out how we can tell whether an entity is scientifically respectable or not. But it does go some way towards indicating why there are legitimate and illegitimate cases of chaining.

So far so good, but so far we have only dealt with the first group of motivating examples – those in which grounding relates scientifically studied entities to other scientifically studied entities. There is also the example of the relationship between chance and frequency. This example is taken from Hájek; the relevant section is given below:

> We posit chances in order to explain the stability of these relative frequencies. But there is no explaining to be done if chance just *is* relative frequency: you can't explain something by reference to itself. Here I am echoing a well-known argument due to Armstrong (1979) against the 'naïve regularity theory' of lawhood (that laws are simply true universal generalizations). Compare: we posit laws of nature in order to explain regularities, so they had better not simply *be* those regularities, as a naïve regularity theory of lawhood would have it.[115]

In this passage, Hájek could be read as arguing against the view that chance is identical to frequency, rather than arguing against the view that chance is grounded in frequency. Doing so would not provide any motivation to accept the transitivity principle as the principle links at least two distinct entities to one another.[116] But if chances and frequencies are identical to one another, then we do not have two

[115] Hájek (1996) pp. 79-80, italics in original.
[116] Setting aside trivial cases. The principle is compatible with self-grounding, self-explaining entities as it merely spits out the rather redundant result that such entities will explain themselves. Whether there are any such entities is unimportant, for they would be insufficient to motivate the transitivity principle.

distinct entities. Nor would it make any sense to attempt to explain one in terms of the other if they were identical.

Let us set that interpretation aside. The example is specifically intended to mirror the reasoning used in the explanatory circle argument although, as we have seen, the argument can be raised against more than just naïve regularity theories. The chances are grounded in the frequencies while explaining them in the much the same way that Humean laws are grounded in the mosaic while explaining it (granting that it cannot be exactly the same way as the frequentism Hájek is arguing against makes no appeal to anything like the best system). But this makes the motivation from this example far too closely intertwined with the fate of the Humean account of laws. Hájek's argument here is explicitly intended to piggyback on the argument against regularity theories of lawhood. The form of the argument is the same in both cases: both need to appeal to the transitivity principle. If one is in a position from which Hájek's argument appears sound, then one has already accepted the principle under discussion. In that case, there is no need to look for motivation for it! In short, the argument against frequentism will look like good reason to accept the transitivity principle to those who already do. On the other hand, to those harbouring doubts about the principle, this will not provide any independent reason to accept it. (Frequentism, of course, may run afoul of one of the other fourteen arguments that Hájek raises against it.)

2.8 Explanatory pluralism

Let us begin by noting that someone could object to my treatment of Hájek's example at the end of the previous section. Such an objection might well appeal to a feeling of unsatisfactoriness experienced when considering the explanation being offered by the frequentist. It just does not seem to be a good explanation, and this intuition may well support taking the example in the spirit Lange intended. That is, in an example explicitly designed to mirror the argument against regularity theories, the proposed explanation strikes us as being a very bad one. The most natural conclusion here is that this is because it is a case of self-explanation. So the transitivity principle holds for these kinds of examples as well as for those that involve scientific entities in a more obvious way. Hence we have good reason to appeal to the transitivity principle when arguing against the Humean account.

One response to that is to follow Lange and other anti-Humeans in rejecting regularity accounts of laws. But another is to suggest that the problem here lies in the explanation, not the view of laws. Putting the point more directly, none of the responses so far have questioned the circularity argument's third premise: the natural laws scientifically explain their instances. Without this, there cannot be an explanatory circle as the relevant explanations would run in one direction only. That this premise has so far received a free pass is likely because it seems to be a commonsensical claim. If I were to explain why my pen falls to the ground when I drop it, I might well appeal to Newtonian mechanics and, in particular, the laws of motion. There is nothing unusual about this sort of example; we can multiply cases easily.

This is not the place to develop a full account of what scientific explanation involves: the sheer variety of different views in the literature suggests that adding one more will not resolve the problem. Those who have responded to the explanatory circle argument have also avoided filling in the details of what they take scientific explanation to involve, likely in an effort to remain neutral. But there is something that we can say about the strength of the explanation involved: it is stronger than Humeans should be comfortable with. This can be most clearly seen in the quotation from Lange at the beginning of the chapter:

> In short, if the Humean mosaic is *responsible* for making certain facts qualify as laws, then the facts about what the laws are cannot be *responsible* for features of the mosaic.[117]

The first sort of responsibility is understandable: this is the sort of responsibility that accompanies the grounding relation. The laws are what they are in virtue of the mosaic. But the second mention of responsibility is rather odd. On the Humean view, laws are mere descriptions. They do not govern or constrain or make anything

[117] Lange (2013) p. 256, italics added for emphasis.

happen. Of course they are not responsible for the mosaic; Humean laws are not responsible for anything! Nor is anything responsible for the mosaic. There is nothing that forces it to be a certain way, as adding some such thing to the account would reintroduce the necessary connections that were dropped to begin with. In slogan form: things just happen, it turns out they can be described systematically.

Following on from that, the second part of Lange's quote might come as something of a surprise to Humeans in a certain frame of mind. It is picking out a core commitment of the view, that the relationship between the mosaic and the laws is bottom-up. However, here it is being taken to express an objection: that the laws are not responsible for the mosaic is supposed to be a problem for the view. Perhaps the lack of responsibility that laws show is an unattractive feature of the account for its detractors. But that in itself is not a strong objection, especially when Humeans find the notion of responsible laws mysterious.

A belligerent Humean might wish to stop there. The problem with the explanatory circle argument is that it assumes that laws can scientifically explain their instances. Since explanation here is being meant in a strong sense, such a Humean does not take laws to be capable of doing that. Once the third premise has been rejected the argument fails since there is no self-explanation going on. I describe such a Humean as belligerent because of the bullet that needs to be bitten here. That laws can help to explain the world's phenomena is a common assumption. It is involved in my appealing to the laws of Newtonian mechanics to explain the falling of my pen. It is involved in rather more sophisticated descriptions of how various systems evolve offered by practicing scientists. Denying the third premise both blocks the circularity argument and fits in nicely with the Humean conception of the role that laws play. But it comes at a great price, for this denial stands in conflict with both a commonsense belief about laws and how scientists are wont to use them. Perhaps the conflict with the folk usage can be explained away. After all, governance is commonly associated with laws but Humeans are committed to denying that they play that role. A revisionary approach towards a conceptually unclear area like the everyday conception of laws is no great sin. But the ascription of error to working scientists is not so easily dealt with. One of the main motivations for adopting a

Humean view is a respect for science and a suspicion of claims that science cannot grasp the nature of the world without some form of metaphysically weighty supplementation.[118] To claim that scientists are misusing laws and should revise this usage on metaphysical grounds is, at the very least, in tension with this motivation.

A Humean unwilling to take such a belligerent stance will not want to say that laws cannot explain their instances. But that is not to say that the explanatory circle argument goes through. Why think that the kind of explanation appealed to in the argument is the kind of explanation that Humean laws can provide? In other words, if anti-Humeans like Lange are appealing to a strong or 'thick' form of explanation in their objection, then it is open to Humeans to rescue the explanatory role of laws by appealing to a 'thinner' form of explanation. Taking this route requires rejecting a kind of dogmatism concerning scientific explanation, one that claims all scientific explanations are of the same kind. Putative explanations are intended to increase our understanding of the phenomena or help us to make sense of some aspect of the world.[119] If this is the aim of explanations then there are different ways in which explanations might be employed to achieve this aim.

In explaining some phenomenon, there are various kinds of explanation that I might offer. I could tell you what the phenomenon consists in. This rock is both heavy and hard because it is made of granite. Your window is shattered because the components of the window are disconnected from one another. I might instead offer some kind of causal story to inform you how the phenomenon was brought about. The rock has these properties because it was formed through the crystallisation of magma several thousand years ago. Your window is in its current state because Suzy threw said rock through it. I might alternatively give you reason to expect this phenomenon by fitting it into a more general pattern. It is no surprise that this granite rock is hard and heavy since all granite rocks have such properties. Your window is broken because every house on your street had a window broken last night. These ways in which explanations can differ is intended to be illustrative rather

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[118] This is particularly clear in Lewis (1994a) p. 474.

[119] This point is made by de Regt and Dieks (2005) and de Regt (2009). See also Hempel (1965) for a well-known rejection of the role of understanding in explanations.

than exhaustive. There are other kinds of explanation too. For example, the breaking of your window might be explained by referring to intentions or mental states: Suzy bears you a grudge.

Which kind of explanation is relevant to the question being asked is context-sensitive. Upon discovering that your window is broken it is unlikely that you will respond by querying what it is for a window to be in a broken state. You already understand that, so my offering such an explanation would miss the point of the question. What you are probably hoping for is the causal story. But some questions are more ambiguous. If you ask why the rock has the weightiness that it has, it might not be immediately clear whether you are inquiring into its constitution or its causal history. But just because one kind of explanation is a more relevant answer, it does not mean that the others fail to be explanatory.

One might worry that not all the indicated kinds of explanation are on a par. Lange quotes with approval an example from Carroll where the students in a classroom explain their presence by pointing out that they are always present at that time.[120] For both Carroll and Lange, this is not a case of a regularity or pattern explaining its instances. On their view, that there is such a pattern indicates that there are other reasons for the students' presence (such as their intentions). And it is these other reasons that are genuinely explaining why the students are in the classroom. The initial explanation mentions the pattern of attendance not because that pattern explains their presence but because it points to other reasons that do the explanatory work. It just happens that the explanation offered does not cite those other reasons directly. In a similar vein, Skow treats 'all ravens are black' as a reasonable response to being asked why this raven is black despite not taking it itself to be a reason why this raven is black. Rather, that regularity is taken to provide information that all ravens share in some common feature that makes each of them black.

There is undoubtedly something to be said for this response. There does seem to be a difference between fitting this phenomenon into a more general pattern and

[120] Lange (2016). The example is drawn from Carroll (1999) p. 79.

providing the mechanisms by which it was brought about. But that alone does not indicate that the pattern-based explanation fails to be explanatory. One can gain understanding in different ways. Much of the intuitive force behind the claims made by Carroll, Lange and Skow could well be because we are aware that alternative kinds of explanation are available. As the case of the broken window indicates, we are inclined to prefer causal histories to pattern recognition when it comes to explanations. Perhaps the causal story is better at providing the understanding that we seek. But the existence of a stronger form of explanation does not mean that weaker kinds of explanation are either not explanatory or only derive their explanatoriness from the stronger kinds. We are not required to say that Carroll's students failed to provide an explanation for their presence because there were alternative stronger explanations that they could have offered. Going further, there are cases where the recognition of a pattern provides a different kind of understanding to one, say, based on causal mechanisms. The smashing of your window is one such example. By drawing your smashed window into the wider case of your street's smashed windows, I can provide you with a different understanding of the situation than if I had told you about Suzy throwing that rock. Explanation via patterns may often be thought of as a weaker form of explanation, but it is not a strictly inferior one.

Returning to the Humean account of laws, this provides a way to avoid ascribing widespread error to working scientists. The explanation Humeans can provide of the laws is a constitutive one: the laws are grounded in the patterns that occur in the mosaic. The explanation of the mosaic via the laws cannot be one in which the laws are taken to have any responsibility of the goings-on in the world. Rather, the explanations that invoke the laws are explanations in the form of pattern recognition. If I say that my pen falls because of gravity, I am pointing out that massive objects show a general pattern: they move towards one another. As my pen has mass, it is just another instance of this pattern. I have not attempted to identify something that acts on my pen to ensure that it moves downwards. Nor have I tried to why there is such a general pattern (that would presumably be an attempt to find a deeper reason that Lange takes appeals to regularities to be pointing towards). In fact, I had better not try to do this if I am a Humean! Ultimately, there is no deep reason why the world is one with this pattern of events as opposed to another one. For Humeans, there simply is no explanation of that kind of the mosaic.

The explanatory circle argument as raised in the literature has too strong a sense of explanation in mind. Humeans who adopt a wider meaning of ‘explanation’ can say that laws are genuinely involved in explanations without running afoul of it. But there is an obvious objection to dismissing it on those grounds. Notions like responsibility were used to introduce the argument, but they are not central to it. If the argument’s third premise also includes forms of explanation like pattern recognition, Humeans still have a problem on their hands.

That move might be obvious, but it causes another issue: transitivity once more. Setting aside the concerns raised in the previous section, the transitivity principle has some plausibility when we think of explanations as offering causal information. If *E* brings about *F*, and *E* is really just made up from *D*, then it is not unreasonable to think that *D* brings about *F*. That kind of neat story is not available when it is patterns that we are interested in. Suppose that *E* explains *F* by treating it as part of some wider pattern and that *E* is grounded in *D*. There is nothing to guarantee that *D* will be able to explain *F* in the same way that *E* does. If *D* is some lower-level fact, it might be so different in kind to the higher-level facts involved in the pattern that it will fail to unify *F* with them.[121]

We began this chapter by formalising a common objection to regularity accounts of law based on their claimed failure to provide explanations. While Loewer’s distinction between metaphysical and scientific explanations failed to resolve the issue, it did help to clarify exactly what was being appealed to in the debate. From here, three options are open to Humeans wishing to escape the circle. First, they might take an unconventional stance on the grounding relations involved so that explanation is one-way only. Second, they might complain that we have not yet been given sufficient motivation to accept the transitivity principle at the heart of the

[121] This stops short of advocating that Humeans adopt a unificationist account of scientific explanation. The pluralist account suggested does not need to treat every scientific explanation as an attempt at unification. See Kitcher (1989) for an overview of the unificationist position. See Sober (1999) for some issues.

argument. Third, they might take themselves to be offering a different kind of explanation to the one looked for by anti-Humeans. I favour the third option, as I think that it best illustrates the fact that explanations of the mosaic cannot be identifying things responsible for it while still being Humean explanations. That said, this is a defeasible reason and any Humean unwilling to adopt the pluralistic stance indicated might instead focus on one of the other two lines of response.

Chapter 3 Symmetries as metalaws

Now that we have the Humean account of laws in place and have seen how it can be connected to explanations, we turn to a different line of modification. Instead of seeking to tweak the Best System Account further, we now look to build upon it. Happily, there is a clear source of motivation for this. Lewis' regularity account is primarily concerned with physics, and capturing the laws that physicists refer to. However, modern physics deals not only in laws but also in symmetries or invariances. As will be explained, these are taken to have a connection to the laws, yet the Best System Account says nothing about them. This chapter is influenced by Marc Lange's understanding of symmetry principles as second-order laws that hold of first-order laws. The natural way to capture this in the Humean framework is to extend the best system account to include systems that systematise important regularities in the laws. To show that this proposal does in fact fit nicely with the approach to laws, I go on to consider whether the roles played by the second-order best system are analogous to those played by the first-order best system. After concluding that they are, I introduce two objections to this treatment, courtesy of Lange. While the first rests on anti-Humean presuppositions, I conclude that the second is a problem even on Humean terms.

3.1 Introducing invariance

If I pick up a rock, hold it in my hand and then release it, it will fall to the ground. If instead of performing that sequence of actions today, I do them tomorrow, then the rock will still fall to the ground in the same way. If, 500 years from now, one of our robot overlords does those actions, the rock will still fall to the ground. It does not matter where I stand before releasing the rock. The rock will move in the same way regardless of whether I stand here or at the end of the street. Even moving somewhere more exotic will not change that; a rock released on the surface of Mars will still fall towards the (Martian) ground. This may not be a motion towards the surface of Earth as in the previous cases, but it still fits the general pattern. Objects with mass experience forces attracting them to one another in a predictable way formalised by Newtonian mechanics (of course there are certain limits to this, I am ignoring General Relativity for expositional convenience).

None of that will come as a surprise. It is a familiar thought that objects move in the same sort of way no matter where or when they happen to be. As Wigner noted, the spatiotemporal invariance of laws looks to be a requirement on our being able to discover them.[122] After all, if there was no reason to think that experiments performed at different times would get the same results, then experiments would no longer be repeatable. There would be no way to disconfirm someone's claims, since the conditions under which I perform an experiment might fit into a different regularity to the conditions under which you perform your experiment. Further, if there was enough spatiotemporal variation of the laws then there would be little point in attempting any form of ordered investigation into the nature of the world. If we did not believe that there were universal patterns to be discovered, there would be little point in developing any kind of scientific method.

The modern notion of a symmetry is a development of a concept that has been in use for a very long time. Informally, we might say that something possesses a symmetry if it remains unchanged by a certain action. A classic example would be of a square. Rotating a square through an angle that is a multiple of 90° leaves the square the same as it was pre-rotation. Similarly, reflecting the square either through one of its centre lines or one of its diagonals will not affect it. These eight transformations that do not affect the square are referred to as its symmetries. That they form a mathematical group leads to the modern definition: something possesses a symmetry when it is invariant under a certain group of transformations. The generality of this definition means that it is not just physical objects that can have symmetries. Laws too can exhibit symmetries when they remain unchanged under some transformation. The laws of Newtonian mechanics are invariant under

[122] Wigner (1949) makes this point. Brading and Castellani's introduction to their (2003) also mentions that this is sometimes seen as a prerequisite to describe the world by modern science.

spatial and temporal translations as we have just rehearsed. These two are not the only spacetime symmetries. For example, it does not matter how my rock is oriented in space when I release it, which is just to say that space exhibits rotational invariance. Nor are the laws associated with spacetime the only ones which possess symmetries. Consider Coulomb's law:

$$F = k\frac{q_1 q_2}{r^2}$$

Coulomb's law claims that the force between two charged particles is proportional to the charges on the particles and inversely proportional to the square of the distance between them. None of this is dependent on when the two particles exist, the lack of this dependence implies that Coulomb's law is temporally invariant. Similarly, it is the distance between the particles that matters for the magnitude of the force exerted, not their absolute positions. As a spatial translation of such a system will not change anything about it, Coulomb's law is also spatially invariant. Giving the mathematical form of Coulomb's law makes it clear that the law is invariant under inversion of charges. If q_1 and q_2 both had their signs inverted, the force between the two particles would remain the same. This corresponds to the truth of an accompanying counterfactual: had all the charges in the world been inverted, there would have been no empirically discernible difference.

Some of these symmetries are tied closely into how we normally think about laws. One common expectation regarding fundamental laws is that they be universal such that they do not hold only in one restricted area of spacetime. But while we expect laws to accord with this sort of symmetry, they are not conceptually required to do so.[123] The requirement mentioned above, that their doing so is necessary for modern science, is an epistemic requirement. It is not an ontic one. That we need the laws to be spatiotemporally invariant to make an ordered investigation into the world does not mean that there would be no laws if this sort of invariance failed. Rather, it would simply be more difficult – impossible perhaps – for us to find out what they would be. We might be able to cope with a lack of invariance if it made a difference only in

[123] This expectation that the laws take a certain form is a demonstration of the heuristic role played by symmetries. More will be said on this later.

extreme situations or if it manifested in a particularly ordered way, but this is an epistemic requirement brought about by our epistemic limitations.

Lange gives a more concrete example of a world where not every law is invariant under spatial translations.[124] Suppose that some world has a privileged centre and that every object at this world experiences a force towards this centre inversely proportional to the square of the separation of object and centre-point. The world also has a law that each object experiences this force. The law at such a world will not demonstrate spatial invariance. A translation of everything in that world's space that leaves the forces and centre-point unchanged will result in a world where the associated 'law' is false. Since we can imagine a world in which spatial displacement makes a difference to the laws, invariance under spatial translation cannot be a conceptual requirement on laws.

Investigation into the relationship between the world and symmetries is often not done from the armchair as in the case of Lange's world with the privileged centre. Experimental results are expected to accord with those symmetries that we think hold at our world, but this does not always happen. A recent case of this was in 2011, when a team led by Webb published their (second) findings on fine structure constant variations.[125] The fine structure constant, typically denoted by α, is a measure of the strength of electromagnetism. By analysing the light coming in from distant quasars, the team found indications that this 'constant' has a lower value in one direction and a higher one in the other, suggesting a spatial dipole. If the immediate interpretation of the results is correct, then we have evidence that the laws are not the same everywhere. Even if an error is found in either the experiments or the interpretation of the data, this is a clear indication that working scientists are willing to take seriously the suggestion that one of the classic spacetime symmetries fails. A note of caution here on just what is varying. In discussions of possible worlds with different laws to our own, it is common to see examples like an inverse cube law of gravity as opposed to an inverse square. In those cases, it is the form of the

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[124] Lange (2007) p. 461.
[125] Webb et al. (2011).

law that is varying (albeit to a minor degree).[126] This sort of variation is not supported by the recent experiments. Rather, the variation they suggest is in the constants that occur in the laws. This is still variation, of course, but of a much more restricted kind to that usually seen in philosophical imagination.

A more famous case is concerned with mirror reflection. Up until the mid-1950s, physicists thought that nature had no way of distinguishing between 'right-handedness' or 'left-handedness' (more commonly referred to as 'parity').[127] This was the belief that the laws of nature were symmetric under mirror reflection. We can define right or left relative to ourselves and our surroundings but there is no intrinsic difference between right and left hands.[128] Hence a mirror reflection of the world would make no difference. A series of experiments showed that this is not the case for weak decay and that there is a difference between mirror reflections. At small enough scales, parity breaks down. It turns out that even commonsensical symmetries are vulnerable to empirical disconfirmation.

The situation is somewhat more complicated than just indicated, as these things often are. Parity is one of the symmetries that goes into the so-called CPT Theorem (corresponding, naturally, to the P). The letter C stands for charge-conjugation, the operation of swapping particles out for their corresponding antiparticles (in the quantum context, this is more impactful than the mere swapping of charge signs indicated earlier). Here, T is the time-reversal operation, the temporal analogue of P's spatial inversion. The product of these three symmetries, the CPT-symmetry, is taken to imply that a mirror image of our world would evolve in the same way. While the C, P and T symmetries might be individually violated, the CPT-symmetry is currently taken by the Standard Model of particle physics to be one that holds at our world.[129]

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[126] The conservative nature of these changes is noted by McKenzie (2014).
[127] Dardo (2004) pp. 260-264.
[128] For a defence of the lack of intrinsic differences, see Pooley (2003). For commentary, see Huggett (2003).
[129] Coughlan and Dodd (1991) pp. 44-48. See Greaves (2010) for discussion.

The symmetries of interest to physics are commonly classified in some of the following ways.[130] First there are the spacetime symmetries, examples of which we have already encountered. These are contrasted with the symmetries which do not involve spacetime (such as permutation invariance, discussed below). There are global symmetries such as rotation, those whose transformations can be specified in a time-independent way. These should be contrasted with local symmetries, those whose transformations are time-dependent.[131] Some symmetries are continuous – their transformations can come in arbitrary amounts (think of the possible rotations of a circle; any degree of rotation leaves the circle unaffected). Other symmetries are discrete – the associated transformations are not continuous in this way (think of the possible rotations of a square; only rotations that are a multiple of 90° leave the square unaffected). More recently, Caulton has argued that it is useful to distinguish between 'analytic' symmetries and 'synthetic' ones. Only the holding of the latter corresponds to physical differences; the former are to be explained as constraints on theory interpretations.[132]

Symmetries are often claimed to play particularly important roles. One of these is philosophical: that certain ones obtain is supposed to tell us something about the nature of the world. The classic example here is permutation invariance, which is often taken to mean that quantum particles are not individuals.[133] This is typically introduced by way of a rough analogy. Suppose that we have two balls (corresponding to particles) and two boxes (the microstates that the particles can be in). Classically, there are four different ways to distribute the balls across the boxes: both in the left box, both in the right, one in each and finally the other way of putting one in each. Assuming that each of these distributions is equally likely, the chance of any one of them is 1/4. This leads to an empirical consequence: if there were some

[130] Accessible overviews are provided by Bangu (2013) and Morrison (2008).
[131] For a discussion of why it is that time-independence has priority in this definition, see Wallace (2003). Earman (2003) complains that much discussion of this distinction is misleadingly presented.
[132] Caulton (2015).
[133] Here I draw upon French and Rickles (2003), and French and Krause (2006). The following discussion sets aside the issue of parastatistics.

process that randomly assigned balls to boxes, we would expect the frequency of each distribution to tend towards 1/4 as time passed.

The classical case is straightforward enough because permuting the balls gives rise to new states of affairs. The quantum case is more interesting because permutation invariance is taken to imply that permuting balls (or, more accurately, particles) does not lead to a new possibility. Interchanging them produces no observable difference. Combinations of quantum mechanical particles have two forms of statistical behaviour. Bosons act according to Bose-Einstein statistics while fermions act according to Fermi-Dirac statistics. Relating this to the example set-up, since bosons can be in the same quantum state, bosonic balls can occupy the same box. On the other hand, Pauli's Exclusion Principle rules out fermions from being in the same state. Fermionic balls, therefore, cannot reside in the same box. The difference in statistical behaviour is associated with a difference in the relevant possibilities. When distributing balls that obey Bose-Einstein statistics, there are only three possible distributions: both balls to the left, both to the right and one in each box. We 'lose' a possibility by moving away from the classical case since, from the theory's perspective, the distributions where each ball has its own box are identical to one another (this is the invariance: permuting the balls makes no difference). The situation is even more stark for the fermionic balls. Once we have made the same identification as in the case of the bosons and ruled out distributions where both balls are in the same box, we have only a single distribution left: a ball in the left box and a ball in the right. Consequently, the probabilities in the two quantum cases differ from that in the classical case. The probability of finding any one configuration of bosons (assuming all the distributions are equally likely) is 1/3. Given that there is only one possible configuration of fermions, it will receive a probability of 1.

The philosophical import of this is found in the identification of permuted configurations. One might take the fact that permutations of classical particles are treated as new arrangements to indicate that the particles have some form of identity that goes beyond their intrinsic properties (basing their individuality on their having different intrinsic properties fails as all instances of each kind of particle has these properties in common). This might be some form of haecceity or primitive substance. But it is not required that we invoke metaphysically loaded notions like these. If we assume that some principle of impenetrability holds such that no two objects occupy the same spatiotemporal point, then classical particles will differ from one another in their spatiotemporal relations. Empiricists sceptical of primitive individuality might therefore wish to ground the individuality of particles in these different relations. This is an appeal to a Principle of the Identity of Indiscernibles, a claim that any two distinct objects must be distinguishable (that is, they must differ in their properties or relations).

However, since permutation does not lead to any empirical difference when we are concerned with quantum statistics, the status of such a principle is more controversial when applied to quantum particles. Two electrons, for example, entangled in the singlet state will possess the same intrinsic properties as each other (these discussions usually make implicit appeal to the so-called Eigenstate-Eigenvalue link, discussion of which I set to the side as tangential). Since quantum particles do not generally have unique spatiotemporal trajectories, at least under the standard interpretation of quantum theory, we cannot appeal to different spatiotemporal relations to distinguish them as we can for classical particles. This is commonly taken to indicate that the particles under discussion lack individuality in some substantive way. The evocative metaphor here is that particles are akin to money in the bank. If I have £100 in my account and withdraw £10, it makes little sense to ask which 10 of the original 100 that I took. The pounds in a bank account lack the individuality required to ask those sorts of questions about them.

There are moves that can be made in response to this.[134] Saunders has suggested a particularly prominent response, reviving Quine's three grades of discriminability and arguing that even the individuality of entangled particles can be grounded in the fact that there exists a non-reflexive relation between them: an electron in the singlet state bears the relation of having opposite spin to another electron, but not to itself. This strategy has the advantage of covering other philosophically well-known cases, such as Black's two spheres.[135] It is not maximally applicable, however,

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[134] See French and Krause (2006) pp. 149-173 for discussion of these.
[135] Black (1952).

since it will not suffice to recover a Principle of the Identity of Indiscernibles in every domain. Graph theory, despite sometimes being thought to provide cases appropriately analogous to quantum particles, has edgeless graphs which contain vertices that do not satisfy such a principle.[136] On a more physical note, whether bosons are weakly discernible is still a contentious matter of debate.

Of course, it is not just philosophers who are interested in the holding of symmetries; they are enormously important to the practice of physics. The acceptance of Einstein's work on relativity marked, to use Wigner's phrase, 'the reversal of a trend'.[137] Whereas laws had taken centre place in our efforts to understand the world, now principles of invariance were of prime importance. Years later, this led Nobel laureate Philip Anderson to comment that 'It is only slightly overstating the case to say that physics is the study of symmetry.'[138]

An indication of this importance is found in the use of symmetry groups to predict phenomena. Briefly, this is where a certain kind of invariance principle is posited and then particle behaviour is predicted on the assumption that the world behaves according to such a principle. The famous historical example of this is the Eightfold Way and the completion of the spin-3/2 baryon decouplet. When classifying particles, it is common to fit them into particular families called multiplets. The various quantum numbers that a particle possesses (corresponding to properties such as mass, charge and spin) determine its position in a multiplet. These multiplets do not have arbitrary structure, but rather are determined as bases of irreducible representations of various symmetry groups. The dimensionalities of these irreducible representations correspond with the cardinalities of the associated multiplets. If one assumes that an appropriate symmetry holds and has empirical data concerning particles that fit into a certain multiplet scheme, one can then predict the existence of the 'missing' particles that would complete that scheme.[139]

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[136] Ladyman (2007). De Clercq (2012) argues for a defence of the PII based on inter-graph relations, but see Duguid (2016) for criticisms of such approaches.
[137] Wigner (1967) p. 5.
[138] Anderson (1972) p. 394.
[139] A more thorough account of this prediction, and the assumptions that were involved, is given by Bangu (2008).

This is what happened through appeal to the classificatory scheme called the Eightfold Way. At the beginning of 1961, the physics community was aware of four baryons with spin 3/2 (and had been since their discovery in 1952).[140] The relevant symmetry group, SU(3), allows for multiplets consisting of 1, 8, 10 or 27 particles. Later that year, the discovery of three more particles was announced. The properties of the known particles indicated that they could fit into either the 10 or 27 member multiplets. At a conference the following year, two more particle 'resonances' that fit into both schemes were announced, along with an experimental failure: experimentalists were unable to find results pointing towards the existence of particles that were expected if the spin-3/2 baryons were to fit into the 27 member multiplet. Based on this information, Gell-Mann and Ne'eman predicted that the particles fit into the decuplet. Of course, a decuplet requires there to be 10 particles and only 9 had so far been discovered. So they then predicted that the tenth place in the decuplet had a physical interpretation. Not only was there a particle that had not been found, but its properties could be predicted in advance based on position it was expected to occupy in the classificatory scheme. In 1964 the existence of this particle, named the omega minus, was experimentally verified.

As the above discussion indicates, symmetries occupy an important role in modern science, and a particularly prominent place in physics. The question, then, is how best to understand them. This chapter will follow a suggestion that has been voiced by various scientists.

> At present, we regard invariance transformations as superlaws which we expect to hold not only for those laws of nature which we have come to understand but also for all others.[141]
>
> When learning about the laws of physics you find that there are a large number of complicated and detailed laws, laws of gravitation, of electricity and magnetism, nuclear interactions, and so on, but across the variety of these detailed laws there sweep great general principles which all the laws seem to follow. Examples of these are the principles of conservation … All the various physical laws obey the same conservation principles.[142]

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[140] This historical account follows Ne'eman and Kirsh (1996) pp. 202-203.
[141] Houtappel, Van Dam and Wigner (1965) p. 600.
[142] Feynman (1965) pp. 59, 83.

> The content of the restricted relativity theory can accordingly be summarized in one sentence: all natural laws must be so conditioned that they are covariant with respect to Lorentz transformations.[143]

The first quotation, from Houtappel, Van Damn and Wigner, is the most suggestive. It suggests that symmetry principles are to be understood as 'superlaws', laws which hold of other laws. The second two are less explicit, although they do claim that there are principles which the laws obey and that there are transformations which the laws must stand in a certain relationship to. It is important to sound a note of caution here. The third quotation invokes necessity directly, choosing to talk about how the laws *must* be. This is, obviously enough, not a Humean claim. Taking Einstein at face value here would prevent us from being able to give a Humean interpretation of symmetry principles since the interpretation would have the laws necessarily connected to them. The second quotation could be understood as making an indirect appeal to anti-Humean intuitions. The use of 'obey' might well suggest something akin to governance. That is appropriate for non-Humeans, as they are not restricted from having the laws govern their instances in some substantive sense. An immediate anti-Humean interpretation of symmetry principles would be to have them governing laws in a manner directly analogous to how the laws govern the world.

Two of the above quotations have an anti-Humean flavour, although this is not uncommon when reading the writings of various scientists. It is important to emphasise, however, that these were not remarks made in the context of the modern debate over how laws of nature ought to be understood. It would be unreasonable to expect even very successful and intelligent scientists to anticipate the structure of a philosophical debate that would take shape later that century, and to carefully formulate all of their writings so as not to beg any questions in said debate! A far more reasonable response to the anti-Humean flavour of some of these remarks is to assign little weight to their consequences for arguments that the remarks were not intending to address. This parallels how Humeans normally treat the writings of scientists on laws of nature. It is not uncommon to find scientists

[143] Einstein (1954) p. 329.

unreflectively talking of governance without indicating how they understand this notion. Humeans then assign little importance to this, commonly treating it as a holdover from older theological accounts of how the world works.[144] But this re-interpretation of comments is not intended to be dismissive of how well scientists understand their own subjects. Humeanism may be revisionary to some degree, but it is not *that* revisionary! Rather, it is simply an acknowledgement of the fact that many working scientists are either unaware of or uninterested in the philosophical debates that accompany their work. As with governance, so with necessity. This too can be given a Humean interpretation, via the possible worlds of modal realism, that aims to preserve the central points being made by scientists. In short, the anti-Humean implications in the above quotations should not be assigned too much weight, since they can be understood in the same ways that Humeans normally understand such suggestive comments. How effectively this treatment can be carried out is the question that this chapter is concerned with.

The interpretation of symmetry principles as metalaws has appeared in the philosophical literature, albeit not yet extensively. Margaret Morrison has talked about the view that laws are the manifestations of certain symmetries, although she also makes the anti-Humean suggestion that we take local symmetries to impose constraints on how physical systems can be.[145] Marc Lange has pressed the issue more recently both by offering motivation for the metalaw interpretation and by showing how his account of laws can be extended to incorporate metalaws.[146] This can be viewed as a challenge for rival accounts of laws: can they be extended in order to incorporate metalaws? Given the importance of symmetries to modern physics and their connection to laws (this connection will be examined further in the next section), it is highly desirable for an account of scientific law to say something about them. At the very least, it is a point in an account's favour if it is able to do so and a mark against if it cannot. While Lange's own account does not have a problem with explaining the nature and role of metalaws, it is not an account that will be of any help to Humeans. Lange is not squeamish about necessity and is happy to appeal to

[144] Beebee (2000).
[145] Morrison (1995).
[146] Lange (2000), Lange (2007) and Lange (2010).

it in order to spell out the difference between laws and accidents: the laws have a higher grade of necessity than accidental facts. Metalaws, in turn, are more necessary than laws and so occupy an analogous place in his system – only one step up. Given the unapologetically anti-Humean nature of Lange's account, we must look elsewhere. The next section does just this, by first extending the usual Humean account of laws to incorporate metalaws, and then by discussing how comfortably this extension sits with pre-existing Humean commitments.

3.2 Extending the BSA

The focus of this chapter is to see how symmetries, understood as metalaws, might be incorporated into the Humean account of laws. To do this, I will pursue Lange's suggestion on how this might look.[147] What I will be calling the Extended Best System Account (or EBSA) is a very straightforward extension to the standard BSA. In the BSA, the regularities that turn out to be laws are first-order: they are patterns in the instantiation of perfectly natural properties across spacetime (more generally, they are patterns in whatever is fundamental). If we are to understand symmetries as laws of laws, they must be second-order laws. If all laws are just regularities, then second-order laws must be regularities. Not regularities in the distribution of natural properties, or they would simply be further first-order laws, but in the laws themselves.

The EBSA, then, adds a second-order analogue of the BSA to the original account. As laws summarise patterns in the world, so metalaws summarise patterns in the laws. The manner in which a system is judged to be best need not be different to the usual BSA. Some systems of metalaws are stronger than others. Maximal strength can be achieved by a system that lists all and only those laws that hold at the actual world. This system would be consistent with only those worlds which have exactly the same laws as our own. Unlike for first-order systems, a maximally strong second-order

[147] Lange (2007) and Lange (2011). This is quoted with approval and explored in Yudell (2013). It also makes a brief appearance in French and Saatsi (forthcoming).

system will not suffice to pick out our world uniquely since different possible worlds can have the same laws while having different histories. If the physically possible worlds are those which have the same laws as ours, it is these which will be picked out by a maximally strong second-order system. The case of simplicity mirrors that of simplicity in the BSA. However it is that simplicity is judged (say by sentence length), there is no reason to think that it need be different for the EBSA. These virtues compete against one another in the usual way. For instance, adding sentences to a system will increase its strength while decreasing its simplicity.[148]

A point of clarification: the extension to the BSA does not replace the original account. It had better not, since a second-order account will only give us metalaws and we still need laws! Rather, the competition for which system is best is run separately for each 'level', although the results of the competition for the metalaws will depend on the results of the competition for the laws; ontologically speaking, laws are prior to patterns in the laws.

In the same way that we generated a second-order BSA, might there also be a third-order BSA (and so forth)? There is nothing special about the move to a system concerned with patterns in the patterns, so if that is acceptable then the move to the other higher-order BSAs is open. Two salient comments. First, the mere fact that a Best System-like account is available does not guarantee that there will be any associated laws. Should the world be sufficiently disordered it will lack the sort of striking regularities that the BSA will describe via laws. Similar comments apply to the second-order BSA: should the first-order laws fail to exhibit regularities, there will be no metalaws. This will obviously continue to hold for the higher-order best systems, so there is no guarantee that by moving to a third-order BSA we will find metametalaws. The point would be a stronger one if we had good reason to think that there will be decreasing amounts of order as the level increased. So, once we are sufficiently far removed from the fundamental distribution, we run out of regularities and no longer have to worry about saying what these higher-order laws

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[148] I have not mentioned fit here as it is not clear that there is going to be a second-order analogue, not unless we think that there will be probabilistic metalaws. Perhaps this is one difference between the BSA and the EBSA.

are and what role they play. I think that this is plausible, but I lack an argument in its favour.

Second, this is not a full explanation of how higher-order BSA laws link up with scientific practice (it is not even close to an attempt). The link between first-order BSA laws and scientific laws is well-studied in the literature; the former are supposed to capture the latter. While understanding symmetries as second-order BSA laws is a relatively recent move, the intention is clear here too. But I know of no account which suggests what in scientific practice would match up with third-order BSA laws, or what role these would play in assessing counterfactuals and the like. The lack of such a link might not be an objection to the higher-order BSAs, but would be a striking dissimilarity between the first two orders of BSA, and all the others.

A stronger case for this extension of the BSA can be made if the resulting second-order BSA plays philosophical roles analogous to those played by the standard first-order account. That the roles are analogous is important. Given that the EBSA works in such a close manner to the usual BSA, unjustified differences in the usage of them would be a mark against the extension. But this requirement also serves to keep the challenge fair: if we have reason to think that the Humean account does not seek to capture some aspect of our folk conception of laws then we should not require that a Humean account of metalaws attempt to do so either. To judge how analogous the roles of these accounts are, we need to have a list of the roles that are played by the Humean account of laws. Then, for each role, we can assess whether this account of metalaws satisfies an analogous version of that role. Roles commonly thought to be played by laws include: holding of necessity, being confirmed by their instances, supporting a principle of inference, being an aim of scientific inquiry, explaining their instances and supporting counterfactuals.[149]

Let us begin with necessity. One reason to move from a naïve regularity account of laws to the BSA is that the former fails to distinguish between accidental regularities and lawful ones. It is mere happenstance that everyone in the room is a philosopher, but laws like, say, Coulomb's law are not just accidentally true. For anti-Humeans

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[149] Carroll (2016), Hall (2015) and Yudell (2013) pp. 7-13.

this distinction can be spelled out through appeal to necessity. The laws hold necessarily, whereas the accidental regularities are only contingently true. While this route is obviously not open to those who deny necessary connections, the BSA provides an alternative route to distinguish between them. Laws appear as generalisations in the world's best system, while accidental regularities do not. The link between laws and necessity is then reversed: rather than appealing to physical necessity to identify the laws, Humeans can appeal to the laws to set out what is physically possible. Those worlds with the same laws of nature as our own are the physically possible worlds. (If we assume the truth of modal realism, then there is nothing terribly special about our world, and so it is more accurate to say those worlds are physically possible *relative to our world.*) This allows for a reductive explanation of the link between laws and necessity. Since the physically possible worlds have the same laws as ours, it follows immediately that the laws are physically necessary.

The EBSA therefore must allow for a way in which we can distinguish between the higher-order analogues of lawful and accidental regularities. Further, it should do so in a way that allows us to reduce modality rather than needing to appeal to some previously understood concept of necessity or possibility. But this is straightforward. The relevant distinction to be drawn is between symmetry principles and byproducts. Those generalisations that occur in the second-order best system are the symmetry principles. Byproducts do not contribute enough strength to the system to counteract the loss of simplicity created by adding them. This lack of importance captures the sense in which they merely happen to be true due to the specific laws our world has; there is nothing deeper to them.

This is not an exact mirror of the relationship between first-order laws and their instances. Laws are physically necessary while worldly phenomena are contingent. The EBSA then cannot treat laws as contingent while metalaws are necessary. But this difference is due to the subject matter, it is not a defect of the Humean treatment of metalaws. What is more, there is a roughly analogous necessity-related difference between laws and metalaws to be found in the EBSA. Lange treats metalaws as more necessary than laws on account of their stability: since metalaws are invariant under a wider range of counterfactuals than laws, his account treats them as more necessary.[150] This then mirrors the relationship between laws and their instances. But there is nothing to prevent a Humean from giving an appropriate translation of that. We have already noted that Humeans define physical necessity in terms of laws. The account of stability is very similar: notice that we hold laws invariant under a greater range of counterfactuals than we do worldly facts, this is what it is for laws to be more 'stable' than their instances. It is just that the resulting sense of necessity is derivative. In the case of metalaws, Humeans are free to note that they too are held fixed under a greater range of counterfactuals than laws.[151] But, again, the necessity here is derivative. We can define a type of necessity in terms of metalaws just as we can define physical possibility in terms of the laws. A fact can be said to possess this kind of necessity if it is true at every world that shares in our metalaws. Similarly, a fact will possess this kind of possibility if it is true at least one of the worlds that share in our metalaws. Naming this kind of modality is a more difficult matter. It cannot be physical possibility, since a world might have the same metalaws as our own while having different laws. Metaphysical possibility too is unsuitable, for this is typically taken to refer to a kind of modality unrelated to the findings of any science. Perhaps metanomological possibility is a better fit, despite being a bit of a mouthful. Fortunately, selecting the right name might be difficult, but it is also largely unimportant. What does matter is that it demarcates an area of possibility space distinct from and wider ranging than physical possibility. Since Humeans can offer this, there is no great problem arising from how the EBSA treats necessity.

The next role played by laws is confirmation, in that laws are typically taken to be confirmed by their instances. The main difficulty in assessing how similarly this role is played by the metalaws of the EBSA is that a canonical account of how confirmation is supposed to function in the BSA has not been explicitly offered by Lewis. Yudell however, has offered a speculative suggestion regarding Lewis' intentions based around natural properties. One of the characteristics normally

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[150] Lange (2007).
[151] The relationship between metalaws and counterfactuals will be dealt with at greater length shortly.

assigned to natural properties is eligibility: predicates referring to them are projectable in a way that predicates referring to unnatural properties are not. We typically take ‘All emeralds are green’ to be projectable while taking ‘All emeralds are grue’ to not be. Assuming that we have accepted natural properties, we can put them to work here: the difference in projectability is due to the fact that green is a natural property and grue is not. Since the laws of the BSA are expressed in terms of perfectly natural properties, they are confirmable in a way that regularities expressed in terms of unnatural properties are not.

If that is how confirmation works for first-order laws, then whether a similar story can be told for metalaws depends on whether metalaws will be expressed in terms of perfectly natural properties. As he notes himself, this is where Yudell’s suggestion runs into an issue.[152] It is not altogether clear that metalaws will be expressed in those terms. The main reason for this is explained in more detail in the next section, where I discuss Lange’s second objection to the EBSA and how it motivates an extension to the account. For now, let us simply note the outline of the problem: the regularities of the second-order best system summarise the regularities in the first-order best system, which in turn summarise the mosaic. The EBSA, then, provides information about the mosaic at a higher-level to the BSA. In a sense, then, it is ‘nothing new’ as it is a second way in which to describe the very same mosaic. The natural properties are supposed to be sparse and never redundant, so the metalaws cannot contain natural properties. But then they cannot be confirmed in the way that Yudell has indicated.

Loewer has suggested a straightforward alternative response to the issue of confirmation.[153] For a law to be confirmed by an instance, examination of that instance must increase our confidence in both the generalisation expressed by the law and in our belief that unexamined cases will accord with this generalisation. Suppose that we accept a Bayesian account of confirmation. Then upon encountering some object falling in the way that one would expect from Newton’s law of gravity, I ought to increase my degree of belief in the truth of the

[152] Yudell (2013) p. 358.
[153] Loewer (1996).

generalisation captured by that law. Since my degree of belief in Newton's gravitational law has increased, my degree of belief that the next object I encounter will move in the same sort of way should also increase. Humean laws are required to be exception-less regularities, so if I am more sure that the regularity holds then I ought to be more sure that the next instance will not prove to be a counterexample. This does not guarantee that it is the Humean laws that will be confirmed, as opposed to mere accidental regularities. But the issue of a scientist having a probability distribution that results in non-laws getting confirmed is one that faces Bayesian anti-Humeans as well. Neither is the possibility of non-laws getting confirmed a disaster for the account: confirmability of accidental regularities has been suggested multiple times in the literature.[154] A nice aspect to making the Bayesian commitment here explicit is that it highlights the way in which the confirmation of some generalisation depends on what background assumptions are in place.[155] Change the background beliefs sufficiently and one changes the confirmation.

We might note in passing that there is a long-standing objection to Humean laws playing any role in inductive reasoning. Armstrong, following Dretske, takes confirmation to be a sort of converse of explanation.[156] Induction for him involves the – somewhat controversial – claim that it is based on inference to the best explanation: from examining instances of some generalisation, one can apply inference to the best explanation to infer that the corresponding law holds. From the fact that the law holds, one can then make inferences regarding the unexamined instances. This sort of story will only work if the relevant law can explain its instances; if it cannot then one cannot make an inference to it as the best explanation. Humean laws, so the story goes, cannot explain their instances and so cannot play this role in inductive reasoning. As we have seen in the previous chapter, things are not so straightforward. Even granting that inference to the best explanation plays any role in induction and confirmation, there are still moves that Humeans can make

[154] Examples include Carnap (1950) pp. 574-5, Hempel (1965) p. 176, and Sober (1988) pp. 95-8.
[155] van Fraassen (1987) p. 255.
[156] Armstrong (1983) and Dretske (1977) p. 261.

regarding explanation. One need not accept that Humean laws cannot explain their instances.

There seems to me to be no in-principle reason why the same Bayesian story would not work if we go up a level. Encountering instances of a first-order law needs to be replaced with encountering or, rather, discovering laws that exhibit a certain symmetry, of course. Once this is done, there is no reason why the discovery of a law that is, say, invariant over spatial displacements would not increase our degree of belief in the generalisation that all the laws are so invariant. And if we are more confident that all the laws are invariant in some way, then we should be correspondingly more confident that newly discovered laws will be invariant under that transformation. In the case of confirmation, the EBSA is directly analogous to the BSA.

It has been suggested by van Fraassen that another thing we may wish of laws is for them to accord with a principle of inference.[157] According to this principle, it is valid to make the move from 'It is a law that *A*' to '*A*'. For Humean laws, this is satisfied easily. Since the laws are the generalisations in the best systematisation of truths about the world, it follows immediately that they are all true descriptions of regularities. Hence whenever it is a law that some regularity holds, that regularity really does hold.

Parallel things can be said about metalaws. The Humean account of laws satisfies van Fraassen's principle because of the relationship it claims holds between laws and their instances. Exactly the same kind of relationship holds between metalaws and laws. As the laws summarise the local facts, the metalaws summarise the laws. So they too satisfy the principle. If it is a metalaw that the laws are invariant under some transformation, then the laws are invariant under that transformation.

While setting down the form of the BSA in chapter one, we discussed the suggestion that the standard account be modified in order to include a more distinguished role for initial conditions. Hall's proposal that candidate systems can be split into an Initial

[157] van Fraassen (1989) p. 64. This is not uncontroversial. See Cartwright (1983) and Lange (2000) for rejections of such a principle.

Conditions Hypothesis and a Dynamic Hypothesis was introduced, and we saw how doing so influenced the way in which the theoretical virtues that determine which system is best interact with candidate systems. In short, strength is a property of entire candidate systems and must be balanced against both the entire system's simplicity and the uninformativeness of its ICH. We also dealt with Hall's concern that the laws of such a system would not be Distinctively Appropriate Targets of Scientific Inquiry – the kind of thing that scientists would be interested in – by pointing out the scientific project is about both the gathering of the world's nonmodal information and about organising this information into explanations. The question to consider at this stage is how much of this discussion carries over to the EBSA.

We can certainly say which part of the first-order BSA it is that the EBSA is concerned with summarising. It is the component that deals with the laws – the DH – as opposed to the initial conditions. When we talk of the laws remaining invariant under some transformation, this transformation is a change in the conditions under consideration. The laws must describe the same kind of behaviour even under changes described by, say, spatial rotations, but no such invariance is expected to hold for the initial conditions. It is not entirely clear what an invariance requirement on the initial conditions would even mean. But does the EBSA incorporate something like the ICH/DH split? And does this have any consequences for whether the metalaws are distinctively appropriate targets of scientific inquiry?

On the one hand, we might want to deny that an account of metalaws has any business dealing with initial conditions. The purpose of a second-order account is not to summarise the mosaic directly, but to summarise patterns in the first-order summary of the mosaic. Initial conditions, dealing with how things actually stand in the mosaic, are from this view not obviously applicable to the second-order account. The question that underlies this thinking is the issue of how including these sorts of facts helps a system to better describe the laws. There is a potential problem too: given that a distinction between initial conditions and dynamic laws is arguably necessary in order to understand what invariance principles even are, allowing for what looks something like initial conditions in the laws themselves threatens to collapse this important distinction.

With that sort of worry in mind, we might not want to allow for there to be initial conditions in the EBSA, strictly speaking. This need not be so bad. Woodward, drawing on Wigner and Sklar, reminds us that there are contexts in which the distinction between initial conditions and dynamic laws is less useful.[158] Certain cosmological models might provide examples of this. However, even if there are cases where the distinction breaks down, that fact alone does not undermine the distinction more widely. It is simply a sign that talking of the ICH or the DH in that context is unhelpful. Humeans might well want to extend this thought to the present case: once we have moved to the level of metalaws there is little to be gained by talking about initial conditions. That is no problem, since we didn't have good reason to think that the distinction was going to be useful everywhere. Perhaps similar things would need to be said about the standard BSA if it were able to adequately deal with special science laws. Humeans taking this approach to the metalaw account would not even need to abandon the ICH/DH split entirely: it would remain possible to claim that the theoretical apparatus remains in place, even if the ICH happened to be empty due to nothing playing the required role to enter it.

But recall that the aim of extending the BSA is to have an account that is analogous to the usual first-order one, not one that needs to match exactly. Not having an exact match here is perhaps inevitable, since there is something misleading about dividing talk of symmetry principles into a part concerned with boundary facts and a part concerned with dynamics. One might wonder exactly what is dynamic about a principle of invariance! So if we want to include something like the ICH/DH split in the EBSA, we need to remember that it cannot be precisely the same split that the BSA can incorporate. A more general way of talking about a split, one that dispenses with the misleading terminology, would be to distinguish between the component of the second-order account made up of propositions expressing regularities and the component whose propositions do not express regularities.

[158] Woodward (forthcoming).

In the first-order case, a law allows for there to be various histories of events that all fit into a wider pattern. Yet, the fact that a law holds is not sufficient for us to make any predictions regarding what events actually occur: we need to supplement the law with some form of boundary condition in order to get that. Similar thoughts apply once we have moved up a level. A symmetry principle might allow for there to be different kinds of dynamics at the world. But that by itself doesn't make any predictions regarding what dynamics the world has. This indicates that there is well-motivated room in the Humean account for something (roughly like initial or boundary conditions) to supplement the metalaws. Including these non-regularity facts in candidate systems of metalaws strengthens those candidates in the usual way: it makes the system compatible with a narrower range of possible worlds by ruling out some of those that have different dynamical laws, even if those laws are invariant under the same transformations as our own.

As an example of the sort of fact that might enter our world's second-order best system, recall that Permutation Invariance allows for there to be paraparticle behaviour. As far as we can tell, there are no paraparticles. Perhaps there is an explanation for this that we have not yet discovered, but it might also be a brute fact about the world: there is just a region of fermionic behaviour over here, a region of bosonic behaviour over there, and so on. Another way of describing this is to say that the relevant symmetry principle (PI) allows for there to be various kinds of statistical behaviour captured by laws, but it is also a non-regularity fact in our best system of metalaws that some of this behaviour does not occur. Are metalaws like this appropriate targets of science? Well the reason that first-order laws end up as DATSIs is that scientists use them in explanations of the phenomena, not merely as efficient summaries of the data. This explanatory aim of science is served by metalaws as well as laws. If Humeans can offer reason to think that their metalaws can feature in explanations, then Humean metalaws are DATSIs.

Let us now deal with explanation. As was noted more extensively in the previous chapter, this is a controversial area for Humean accounts. It is not uncommon to find claims that Humean laws are unsuited to play any role in scientific explanations on account of really being nothing over and above that which they are supposed to explain. This is normally taken to be a rather damning objection by its proponents, and those who make it tend to reject Humean views of laws altogether. We can therefore set aside concerns taken to have that sort of impact, for if one is not on the market for a Humean treatment of laws, then one is unlikely to be interested in a Humean treatment of metalaws. As an alternative, one might agree that Humean laws cannot scientifically explain their instances, but be willing to bite the bullet here. Perhaps the view has advantages enough to overcome this cost. If this looks attractive, then there is very little that needs to be said about metalaws and explanation. If the laws cannot explain due to their relationship to their instances, then metalaws will be unable to explain due to their analogous relationship to laws. If it is nevertheless acceptable to have a first-order account that lacks this explanatory role, then there is no principled reason why it would be unacceptable to have a second-order account that also lacks that role. So if one rejects the link between Humean laws and explanation, then there is no new problem here. Either the Humean account is fundamentally flawed or the lack of explanation is not a decisive factor. Regardless, anyone going in for this stance on Humean explanations will find a discussion of this account of metalaws and explanation uninteresting.

Let us therefore restrict our attention to views which allow for Humeans to give scientific explanations. The two questions that then arise are whether there is good reason to think that symmetry principles are involved in scientific explanations, and whether the Humean account has the resources to say something about that involvement. We will take these in turn.

The most common place to look for symmetries and explanation is Noether's (first) theorem.[159] It is well known that there is a connection between symmetries and conservation laws. For example, conservation of angular momentum is associated with invariance under rotations, and conservation of energy is associated with invariance under temporal translations. Noether's theorem states that for every continuous global symmetry there exists a corresponding conservation law. That is, for every such symmetry that a system exhibits, there will be a physical property that

[159] Noether's less famous second theorem is concerned with local, as opposed to global, symmetries.

is conserved. This connection might suggest that to understand the role that symmetries play in explanation we need only appeal to the theorem. If we can derive conservation laws from symmetry principles, then it looks plausible that those principles have explanatory priority. This sort of reasoning is not difficult to find:

> So, momentum conservation really follows from Newton's third law of motion. But where does Newton's third law come from? Noether's theorem is the deeper statement, implying that the total momentum is conserved, *because* the interactions are determined by laws that don't depend upon where the system is located in space![160]

> Conservation of energy and momentum had been known for centuries … In light of Emmy Noether's insight, it is instructive to ask what symmetries are responsible.[161]

While these authors suggest that Noether's theorem plays some sort of privileged explanatory role, Zee hints at a problem here. The relationship between symmetries and conservation laws that Noether's theorem applies to was old news by the time that she published her paper. The theorem was praised for its generality, not for its surprise factor.[162] As Lange has convincingly argued, Hamilton and Lagrange independently explained the conservation laws through appealing to symmetries well before Noether's theorem was published.[163] Given that conservation laws could be derived prior to the theorem's appearance, it is odd to suggest that the explanatory nature of these derivations is dependent upon that theorem. This does not rule such a suggestion out, since there is room to claim that Noether's theorem has an important role to play in improving such explanations by drawing them into a common framework (despite the derivations being possible without appeals to the theorem being made). But historical contingencies are not the deeper issue. More pressingly, Noether's theorem can be run in reverse, allowing for symmetries to be derived from conservation laws. It is therefore difficult to see how Noether's theorem assigns explanatory priority to symmetries over conservation laws given its two-way nature:

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[160] Lederman and Hill (2004) p. 105, emphasis in original.
[161] Zee (1999) p. 120.
[162] Brading and Brown (2003) p. 89.
[163] Lange (2007) p. 465.

> The very notion of explanation involved is misguided. Noether was not attempting to explain conservation principles in terms of variational symmetries; indeed she stressed that her first 1918 theorem can be proved in reverse … We have now established a *correlation* between certain dynamical symmetries and certain conservation principles. Neither of these two kinds of thing is conceptually more fundamental than, or used to explain the existence of, the other.[164]

When it comes to explanation, Noether's theorem is something of a red herring. However, as the above quote indicates, Brown and Holland go further than just dismissing the theorem as explanatorily irrelevant. Rather, they make the stronger claim that there is no explanatory priority to be given to symmetries or their associated laws. But that does not follow; the failure of Noether's theorem to supply symmetries with explanatory weight does not entail that symmetries are thereby unable to explain conservation laws. The fact that this sort of explanation was attempted by Hamilton and Lagrange suggests that the theorem is neither required for the relevant sort of explanations nor a block on there being any. That derivations are two way does not imply that there are no explanations. There are various examples of just this in the literature on scientific explanations. For one classic example, take the flagpole counterexample to the DN model of explanation.[165] Given the relevant laws, it is possible to derive the length of the shadow from the height of the flagpole and the angle of the sun above the horizon. The same sort of derivation can be run in reverse, as the sun's angle and the shadow's length together yield the flagpole's height. Yet it is commonly thought that there is an explanatory asymmetry here, since it is the first case that is taken to involve a (causal) explanation.

From the perspective of the present project, the explanatory irrelevance of Noether's theorem is something of a relief. After all, it covers only those symmetries that meet certain criteria. Not all dynamical systems can be given a Lagrangian formulation, in which case the theorem is simply not applicable.[166] Not all symmetries are continuous; as was noted earlier, there are important discrete symmetries. If Noether's theorem was an important part of the explanatory role

[164] Brown and Holland (2004) pp. 1137-1138.
[165] Salmon (1989).
[166] Wigner (1954).

played by symmetries, we would have unanswered questions to deal with. Why is there no equivalent principle that allows for discrete symmetries to explain? If discrete symmetries can explain without such a principle, why do those explanations differ in kind from the explanations that involve continuous symmetries? And if not, why not? Thankfully, such questions can be set aside. Let us now consider an example of an explanation that appeals to a symmetry principle.

A classic example here is of star collapse.[167] When a red giant star no longer has sufficient energy to fuse helium into carbon, it will begin to collapse. Pre-collapse, the outward thermal pressure generated by this process of fusion is balanced against the inward gravitational forces; so long as these are balanced, the star will remain in a state of equilibrium. The eventual exhaustion of its fuel source means that it will no longer be able to generate this level of thermal pressure and so the gravitational attraction will win out, causing the star to collapse. This collapse, however, comes to a stop and results in the formation of a very dense white dwarf. It is the halting of the collapse that has received the most attention in the literature on explanation, where the explanation is sought from the physicists. At some point in the collapse, the force of the gravitational attraction is balanced once more, this time by the 'pressure' of electron degeneracy. (Pressure here is meant in a more general sense than that normally encountered when dealing with gases. The scare quotes are used as a reminder that it is a contentious issue whether one can understand this sense of pressure in a similarly causal manner. As this is a tangential issue to our purposes here, I remain agnostic on this question.) The electron degeneracy is an application of the Pauli Exclusion Principle (PEP), which states that no two of the star's electrons can be in the same quantum state. This limits the number of electrons that can be at any one energy level. As the star collapses, the lower levels get filled up, leaving only higher energy levels available for the electrons to move into, thus creating the 'pressure' just mentioned. Given the gravitation attraction and the PEP, there are no more possible states for the star to move into and its collapse comes to a halt.

[167] The relationship between this example and PI is discussed in French and Saatsi (forthcoming). The philosophical implications for explanation were introduced in Lewis (1986b) pp. 222-3. See Skow (2014) for criticisms of Lewis' presentation.

The relevance of symmetry principles to this explanation can be found in the appeal to the PEP. This principle is really an application of the Permutation Invariance introduced earlier in this chapter. As electrons are one of the fermionic particles, their behaviour is described by Fermi-Dirac statistics. The reason why only certain arrangements of electrons in energy levels are physically possible is ultimately the same reason why we cannot have two fermionic balls in the same box. The above story does not apply to every kind of star. The Chandrasekhar limit, roughly equivalent to 1.44 solar masses, gives the upper limit on the mass of a white dwarf. In those with greater masses, the electron degeneracy will not balance out the gravitational attraction and the star will continue to collapse. This collapse will only be stopped by neutron degeneracy, at which point we can give an analogous explanation to the one above: Pauli's Exclusion Principle implies that the star's neutrons cannot exist in the same quantum state as one another, creating an effective pressure to balance the gravitational attraction. This too has an upper limit, about 2 to 3 solar masses, after which the collapse cannot be stopped and a black hole will be formed. It is a discrete symmetry that underlies all of this explanation of the behaviour of collapsing stars: Permutation Invariance.

It might be tempting to adopt an interpretation of these kinds of explanations where the notion of constraint is doing much of the work. This would be the idea that, say, two electrons cannot be in the same quantum state because the PEP (and so ultimately Permutation Invariance) prevents them from doing so; their behaviour is constrained by such principles. However, this is entirely optional. Humeans might well want to emphasise the heuristic role that this talk plays when employed by physicists. When modelling the evolution of some system, we need to set 'rules' for that model – if we make no assumptions as to how some system behaves it is impossible to model its evolution. In doing so we impose a form of constraint on the model. That said, there is nothing metaphysically weighty to this, since it amounts to little more than wanting to find out what will happen if we assume that it behaves in certain regular ways. A similar story can be told of the way in which symmetry principles 'constrain'. Certainly, if we assume that a certain regularity holds then some patterns of behaviour will be ruled out as physically impossible. But physical possibility gets a reductive treatment: there are worlds where such prohibited behaviour occurs, they just happen to not share in our laws (and metalaws) of physics. By claiming that some symmetry principle holds, we are restricting our attention to those worlds where the patterns of events do not violate that principle. So while it is true that some behaviour is not possible in a world where Permutation Invariance obtains, the necessity here is derivative, brought about by our implicit decision to restrict our attention to the physically possible worlds. This is simply to treat the relationship between metalaws and necessity in a manner analogous to that between laws and necessity. When physicists talk about symmetries constraining the space of possibilities, Humeans can interpret this as a way of focusing the audience's attention on a certain subset of possible worlds.

This narrowing of focus plays an important role in answering 'What If' questions (a fuller treatment of counterfactuals in this account follows shortly). All such questions require some initial assumptions, for without any assumptions of what rules should guide our answers there would be no way of giving a sensible response. Consider: if there are no assumptions made on the regularities at play then there is an infinite number of possible worlds whose possible events could be appealed to in answering each question. Indeed, at least one of those assumptions will always be indicated in the question itself, as it spells out the way in which we are supposed to imagining things being different. By narrowing the worlds under consideration, we can answer questions like 'What would the fate of our sun be if it had twice its actual mass?'. Such questions often presuppose that the laws and metalaws still hold, which allows us to select the right world to answer the counterfactual: electron degeneracy would not be sufficient to stop its collapse, but neutron degeneracy might. This allows Humeans to interpret the usual talk of restriction and constraint without thereby being committed to any metaphysically objectionable claims.

In the previous chapter, I suggested that one way Humean laws might be able to serve in explanations is by helping us to understand how events fit into a general pattern. This fits nicely with the approach to 'What If' questions just mentioned. When assessing these questions, we must make various contextual background assumptions. These will often be law-related, as we are often interested in cases of physical possibility. In order to answer these questions accurately, we need to appreciate what events would have to be like if some regularity were to hold. Scientific explanations can help to provide information by highlighting when events are connected to one another in a pattern. The approach to explanation involving metalaws just described is an extension of this treatment. A scientific explanation that involves appeal to symmetry principles helps us to understand the relationship between relevant events by pointing out ways in which they do not differ. It is only once we have this understanding that we are able to pick out the right possible worlds to assess the relevant counterfactuals and give the correct answer to What If questions.[168]

We have already seen mention of counterfactuals when evaluating symmetries and explanation. It is time to give them a more thorough look. According to Lewis' treatment, to judge whether a counterfactual conditional is true or not, we must consider what occurs in possible worlds where the antecedent obtains.[169] But not just any world will do, for that would let us turn to worlds quite unlike ours and obtain odd results about our counterfactual. Contradictory results, in fact, for not restricting the worlds under consideration includes situations where any logically possible consequent will be combined with the antecedent. This obviously would make a mockery out of attempts to analyse counterfactuals in this way. Rather, we must consider what happens at the closest possible world (or worlds, in case of a tie). If, at the closest possible world at which the counterfactual antecedent occurs, the consequent also occurs, the counterfactual conditional is true. Central to this account is closeness of possible worlds, a context-dependent notion. Consider the counterfactual 'Had I dropped this pen and gravity been an inverse cube law, the pen would have fallen'. In one context, this counterfactual is clearly true: the pen would experience a different force and fall at a different rate to the one it would fall at in our world, but the force experienced would be analogous to the one experienced in the actual world and would be in the same direction. Yet in another context, the

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[168] For a metaphysically neutral account of the link between counterfactuals and symmetry explanations, see French and Saatsi (forthcoming). The original causal account which they work to generalise is Woodward (2003).

[169] Lewis (1973) and Lewis (1979). This is not to say that Humeans must accept Lewis' treatment of counterfactuals, but rather that there is an appropriate treatment of them available.

counterfactual is much harder to assess. Had gravity been an inverse cube law, planetary orbits would have been unstable and our solar system would not have formed. In such a scenario there arguably would not have been a pen to drop, nor an Earth for it to fall towards.[170]

So some context dependence is inevitable on this kind of account. Setting that aside, there is a further question of how to assess which world is the closest to our own. Lewis suggests that the metric for similarity of worlds is weighted according to the following four priorities:

1. It is of the first importance to avoid big, widespread, diverse violations of law.
2. It is of the second importance to maximise the spatiotemporal region throughout which perfect match of particular fact prevails.
3. It is of the third importance to avoid even small, localized, simple violations of law.
4. It is of little or no importance to secure approximate similarity of particular fact, even in matters that concern us greatly.[171]

These evidently make no explicit mention of metalaws. Yet an appeal to metalaws is implicit even in rather mundane counterfactuals like 'Had I switched on the kettle, the water would have boiled'. A world in which water no longer boils at 100 degrees Celsius from the moment I flick the switch onwards is, of course, not very close to our own. For this to be the case, there would be a violation of the first priority. If only the water in the kettle did not boil, this might be a violation of the third priority, for the law violation would be local. But all water having a different boiling point is not a localised occurrence. Further, the boiling point of water is not some fact independent of the properties of other substances. Should we wish to minimise the law violations, a whole swath of properties might well need to change too, and not in a very clear way; we might wonder whether the freezing point of water or the boiling point of ethanol would also change. This makes conflict with the second priority all but inevitable.

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[170] Ehrenfest (1917).
[171] Lewis (1979) p. 472.

Notice the assumption behind all of that. In order for water no longer boiling at the same point it does in the actual world to count as a law violation, we must assume that it does in fact violate our world's laws. That is, we expect the laws of our world to be constant across time and space. If they could vary, then water failing to boil might not be a law violation after all! This silent expectation of spatial and temporal invariance is a belief that a particular metalaw holds of our world's laws.

However, we should not try to modify the above four priorities so as to accommodate this sort of appeal to metalaws in the standard account of counterfactuals. To see why, notice that none of the priorities makes any reference to the laws that hold at the possible world in question. The laws being violated are the laws of the actual world. From the perspective of an inhabitant of one of these worlds, their own world does not contain any law violations. This is Lewis' belief that laws are exceptionless regularities at work – even the laws of possible worlds admit of no exceptions.[172] The laws of those worlds are irrelevant to the analysis of counterfactuals. All that matters is the similarity of matters of fact, and whether those matters of fact are in accordance with our laws. It similarly makes no difference how similar the metalaws of some possible world are to our own. For if we do not care what some world's laws are, there is no reason to care whether they are invariant in the way we believe our laws are.

Nor should we attempt to make some independent requirement in the account that when assessing similarity of worlds, we should take certain metalaws to hold of our laws. It would, of course, only be certain ones that we might wish to afford this status to: permutation invariance and the like do not immediately appear to be related to counterfactuals in the same way temporal invariance is. The existence of evidence that would make us reject these metalaws may seem unlikely, but not impossible. The discovery in the 1950s that the weak interaction violates conservation of parity is a case in point. It is possible, after all, to distinguish a mirrored version of our world from the actual world. But this discovery did not cause widespread dismay amongst philosophers interested in counterfactual conditionals. An account of

[172] Ibid. pp. 468-469.

counterfactuals ought to not be tied to any particular claim regarding what the world's symmetry principles are. Let that remain an empirical matter.[173]

When checking the truth of counterfactuals, we try to hold the laws as fixed as we can while still allowing for minor 'miracles'. These are the localised law violations that are required for the antecedent to be true. As an example, recall the example of the kettle. Assuming the laws are deterministic and that I did not turn it on, there would have to be some kind of law violation to allow my counterpart to flick the switch. This is a general point: counterfactuals describe situations that do not occur, so they require some divergence from the laws and matters of fact that obtain at the actual world. Can we tell a similar story about metalaws? When assessing counterlegals – situations where the actual laws do not hold – do we hold metalaws as fixed as we can?

Sometimes. There are cases where this does occur. For instance, take 'Had gravity been an inverse cube force, planetary orbits would have been unstable'. In order to assess worlds for similarity relative to this counterlegal claim, we certainly do not require avoidance of widespread law violations. Doing so would rule out the very worlds at which gravity differs from our world! But we do require the higher-order analogue of this: avoidance of widespread metalaw violations. A world at which the laws varied across time would be sufficiently different from our own that it does not matter whether the consequent is true at them or not. Obedience to the metalaws is highly important when checking for closeness of worlds in this case.

Note however, a difference regarding the third priority. As just mentioned, assessment of world closeness for counterfactuals requires that we look at worlds at which minor miracles occur. But analogous minor miracles are not required in the metalaws when we are trying to decide whether the planetary orbits would be unstable. In that context, the most similar worlds will be ones at which the metalaws

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[173] It might be possible to claim that temporal invariance and perhaps spatial invariance are different and are assumptions that we need in place for our concept of a law to make any sense. I am not convinced that this would motivate a change in the account of counterfactuals in particular though. And there is evidence that at least some physicists are willing to suggest we could find results that imply they do not hold at our world. See also the discussion of universality and the laws of biology in chapter four.

are not violated at all.[174] This might be because, as Lange has suggested, the initial conditions and metalaws together do not entail the first-order laws.[175] Matters of particular fact, on the other hand, are entailed by the initial conditions and laws taken together. So it is only in the latter case – when we are considering a divergence of matters of fact from our world – that we require a miracle. Different initial conditions would also do the trick, but at the cost of having the world vary from ours in its history, something we wish to avoid when considering most counterfactuals.

However, there are also cases where we do not try to hold the metalaws fixed. Recall the counterfactual 'Had I dropped this pen and gravity been an inverse cube law, the pen would have fallen'. In certain contexts, it is not the instability of the planetary orbits that is most relevant when assessing this. As such, a possible world at which there is no pen to be dropped would not be counted as the closest. That requires there to either be a metalaw violation or a multitude of miracles: gravity cannot have been an inverse cube law throughout the world's history and there still be a pen without one of these. The latter is a poor option: it requires there to be continual miracles so that a world has both an inverse cube law of gravity and the same history as our own. The extreme nature of the miracles required brings this into conflict with Lewis' first priority. So we must require there to be a metalaw violation at this world. Prior to my dropping of the pen, the world's history unfolds according to an inverse square law of gravity. At the point of releasing the pen, there is not only a minor miracle to allow the counterfactual situation to occur, but a change in which exponent is associated with gravity. There are two situations compatible with this counterfactual: one in which the pen moves as if gravity dropped off by the inverse cube of distance but no other gravitational forces change, the other in which all of gravity's effects from that moment on are inverse cubes. Which is most similar will be a vague matter, one which context will greatly impact.

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[174] Which is not to say that those worlds must have the same metalaws as the actual world. Just as in the first-order case, where it is matters of fact not violating our laws rather than sharing in the same laws that is important, we require only no violations of our metalaws in the second-order case. This distinction becomes relevant at simple toy worlds, which may either lack sufficient regularities to have laws like our own or lack the regularities in their laws to generate the same metalaws.

[175] Lange (2011) p. 218

Regardless of which of those two situations is judged to be closest, they have something in common. Both of them require a violation of the metalaw of temporal invariance. So there are counterlegal conditionals which require metalaw violations in order to be accurately assessed. But it is not a free-for-all. Permitting a single violation of one metalaw is not the same as being indifferent to whether the metalaws hold at all. So something like the analogues of Lewis' first and third priorities do in fact hold when assessing counterlegals. Miraculous violations of metalaws may be required to judge which worlds are closest, but they are to be minimised.[176]

The roles identified in this section were not intended to form a comprehensive overview of every use that laws are put so. As such, there may still be a role for laws that has a higher-order analogue which the Humean account of metalaws fails to fill. But while this possibility has not been ruled out, the above considerations suggest that this extension to metalaws fits well with the already existing BSA. When it comes to necessity, inference and being an appropriate scientific aim, metalaws generate no new trouble for the Humean. Their support of counterfactuals is somewhat more interesting, but as we have just seen metalaws are a presupposition of our ordinary counterfactual thinking and have their own distinctive 'What If' questions in the form of counterlegals. Confirmation and explanation are the most difficult issues for the account, although it is worth pointing out that much of this is due to it not being entirely clear what 'the' standard BSA has to say about these (it is always going to be difficult to extend an account if it is not clear what the precise details are to begin with). That said, we have seen that there are reasonable moves Humeans can make regarding these areas in the first-order case and that these can be plausibly extended to the second-order account.[177]

[176] I incline to the view that a system of weighted priorities similar to Lewis' also exists for counterlegals, although I have little concrete to offer at this juncture.

[177] This is a slightly more optimistic conclusion than Yudell (2013) reaches. The difference in opinion arises because Yudell takes the Humean account to be saying different things about confirmation and explanation than I have suggested that it say.

3.3 Miracles and exception clauses

In addition to suggesting the natural way in which to extend Humeanism to cover metalaws, Lange also offers two obstacles for Humeans who accept this route to overcome.[178] The first is concerned with the relationship between metalaws and counterfactuals. In the standard Lewisian treatment of counterfactuals, laws play an important role. They are particularly resilient under counterfactuals in a way in which matters of particular fact are not. This is reflected in Lewis' list of priorities: avoiding widespread law violations is of the highest importance. When assessing counterfactuals and determining closeness of possible worlds, we hold the laws as fixed as we can. Similarly, when assessing counterlegals we attempt to hold the metalaws fixed. If we want to know what planetary orbits would be like in a world where the gravitational force was an inverse cube law, we need to assume that spatial invariance holds at that world. This only holds true in ordinary contexts, of course. There is nothing preventing us from asking what would happen in a world with both law and metalaw violations. But in those ordinary contexts metalaws show a resilience similar to that shown by laws.

So laws are resilient under antecedents concerned with differences of particular fact and metalaws are resilient under antecedents concerned with differences of laws. The problem Lange sees is that metalaws may not be resilient under the former kind of antecedents. In other words, ordinary counterfactuals like 'Had I struck the match at spatiotemporal location L, it would have caught fire' cause problems for this Humean treatment of metalaws. To assess this counterfactual we consider worlds at which I do strike the match at location L. Those closest to ours will have similar laws to ours. Not quite the same, since, from the perspective of our laws, a small miracle is required for the match to be struck. What then are the laws of the closest world (setting aside ties in closeness for simplicity)? At minimum, they must be exceptionless regularities. One suggestion is that they are the laws of our world, but with the addition of a clause like '… except at spatiotemporal location L, where the

[178] Lange (2011).

following occurs …'.[179] This allows for the 'miraculous' striking of the match to occur without any of that world's laws being violated. But while that world's laws have not been violated, that world cannot have the same metalaws as our own. If the laws pick out some spatiotemporal locations as special, then they cannot be invariant across spacetime. This conclusion is a surprising one, for it means that when assessing closeness of possible worlds for ordinary counterfactuals, we do not need to be concerned with whether the metalaws are held fixed. That is at odds with the conception of metalaws as holding true across a wider range of possibilities than laws.

While Lange takes this to be the issue with the Humean account, he is not insensitive to the point that referring to spatiotemporal locations is not the only way in which to distinguish events. This gives rise to the alternative route that he offers. Let C refer to some combination of natural properties that is present at the striking of the match. Now replace the reference to spatiotemporal location L in the clause with a reference to combination C. If this were added to our laws, we get a candidate system of laws for this close world. The laws of that world would not be violated for the same reason as before. Better still, the metalaws of that world would not need to differ from our own as no spatiotemporal location is treated differently to any other, and we have no reason to think that our metalaws make reference to invariance across different natural properties. (This last point is actually stronger than that. It is not just that we do not currently think the laws treat all natural properties equally, but that the opposite seems obvious. Different dynamical laws will be concerned with the evolution of different features of systems. But this is just to say that different laws are concerned with different properties.)

Let us take a moment to deal with the concern that the second kind of clause is something of a cheat. That is, a way in which to preserve the letter of spatiotemporal invariance while really aiming at introducing violations. However, for all that this way of setting up the second kind of clause – adding mention of a combination of natural properties – looks like a disguised way of getting at a clause that is concerned with

[179] This suggestion, offered by Lange in his (2011), echoes comments made in postscripts to Lewis (1979) printed in Lewis (1986a) pp. 54-55.

spatiotemporal locations, it may actually be the more natural way. On the standard Lewisian account, different worlds are different spacetimes, each completely isolated from the others. While properties can exist at different worlds, being partially instantiated wherever and whenever one of the members of that property's set of instances is present, individuals cannot. This is the well-known problem of accidental intrinsics: if an individual were at multiple worlds, it would have to possess incompatible properties.[180] As it seems utterly implausible that I could have the property of *being a philosopher* and the property of *not being a philosopher*, I must be a world-bound entity. At this stage a worry might creep in about the counterfactuals that a world with the sorts of clause-laws mentioned might be able to support. Suppose that someone at miraculous world W was evaluating claims that presupposed the match was struck at location L with combination C of natural properties. These might well be counterfactual claims, although since the striking of the match itself would not be counterfactual from that person's perspective, the counterfactual aspect of the claim must reside elsewhere. Spacetime point L exists within the (isolated) spacetime of that world and so does not exist at any other world's spacetimes. The laws of the miraculous world W do not make exceptions for the goings on at spacetime points in other spacetimes. So, from the perspective of someone at that world, the striking of the match at other worlds would look like a miracle as it would violate the laws under consideration. That is a rather odd conclusion, since the entire point of modifying the world W's laws was to ensure that the match-striking was no longer counted as a miracle at that world. There is no inconsistency in having the similar striking of the match at other worlds still get counted as a miracle from that world, but it is not an attractive feature either.

One might hope that counterfactual theory comes to the rescue here. After all, we can assess counterfactual claims that concern particular individuals despite those individuals never appearing at the possible worlds so critical for the evaluation of those counterfactuals. Applied to the present worry, this would be the suggestion that spatiotemporal location L has counterparts in other spacetimes, and it is because of these counterparts that the striking of matches at other worlds does not

[180] Lewis (1986c) p. 201.

appear miraculous from world W. Counterparts of spacetime points have been appealed to in order to respond to points in the literature on the hole argument.[181] But this is not entirely straightforward. Whether some individual at a possible world is a counterpart of an individual at this world is determined by a relation of similarity: the more properties and relations that the two individuals share in, the more similar they are. This is deliberately a somewhat vague relation, where the vagueness is intended to capture the context-dependence of counterfactuals; we might also count a matching of histories as making for similarity.[182] The natural properties that are instantiated at spacetime points will therefore get counted when determining similarity of points. This is awkward: do we wish to say that only those worlds which have a match being struck at that point in their histories get to have spacetime points that are similar enough to L to count as L's counterparts? Perhaps so, this may not be too large a bullet to be bitten here. Or perhaps not, in which case it may be that the context-dependence of similarity could be leant on to suggest that we can ignore the majority of natural properties that an individual possesses when checking for counterparts. Either way, if one wishes to appeal to spacetime points in laws, one needs to be prepared to say more about how their counterparts interact with counterfactuals. Given this, it is not unreasonable to think that clauses appealing to combinations of natural properties are the more immediate option.[183]

These clause-laws might strike some as rather odd. Typically, we do not expect the fundamental laws of physics to come with exception clauses. In fact, the fact that many special science laws look to be ceteris paribus laws is sometimes appealed to as a feature marking them as different from proposed fundamental laws.[184] Even setting aside the concern Lange has with this solution to the problem, one might worry that the peculiar character of these laws is a mark against the Humean

[181] For example, Brighouse (1994) and Butterfield (1988).
[182] Lewis (1973) pp. 39-41.
[183] This may be a relief to spacetime relationalists who, while perhaps willing to accept talk of spacetime points as a world-building manner of speech, will wish to avoid anything that looks like a substantial commitment to there being such entities. For an account that leans this way, see Huggett (2006).
[184] Earman and Roberts (1999) goes further, arguing that ceteris paribus laws fail to be genuine laws at all.

account. But it is worth pointing out that the difference between these laws with exception clauses and our own laws of physics is due entirely to the difference between this possible world and our own. The world we are considering is 'discontinuous' in a way that ours does not seem to be: it contains what we would consider a miracle. Given that the world evolves in an odd way (again, odd from our perspective), it is not altogether surprising that the nature of the laws of that world differ from those of our own.

After offering this route to Humeans, Lange goes on to reject it due to an objection based on the lack of uniqueness of combination C. Should a possible world have laws that include an exception clause mentioning C, then what we would regard as a miracle can occur whenever that particular combination of properties is instantiated. This will happen at the striking of the match, of course. But properties can be instantiated at multiple different times and places. Given the Humean rejection of necessary connections, there is nothing to prevent combination C from being instantiated more than once at the world. Preventing this requires either some form of necessary connection that blocks that combination of properties from being repeated, or for the multiple occurrences to be irrelevant due to the exception clause making reference to particular times and places. Neither is an option for Humeans here. Should C be instantiated more than once at the world, there would be multiple miracles at the world. Yet Lewis' own priorities for closeness of worlds requires that miracles be minimised. In short, if the clause mentions only natural properties then there is nothing to guarantee that a world whose laws have that clause will not have multiple miracles and so be unsuitable for evaluating counterfactuals.

For Humeans who accept Lewis' treatment of counterfactuals and possible worlds, however, the objection misses its target. Lange is right to point out that nothing prevents C from being instantiated repeatedly at a world. From a Humean perspective, there had better be nothing preventing natural properties from being instantiated! It is also true that a world with multiple C-instantiations could have the same laws as a world with only a single C-instantiation (there are limits though: a system of laws that admits of too many exceptions is unlikely to be counted as the best candidate system). Yet neither of those facts create problems for evaluating closeness of worlds. It is not as if evaluation of counterfactuals requires that we first decide on the laws that must occur at the closest world and then take a world with those laws and treat it as the closest regardless of the events that occur at the world. If it were, then Lange's objection would have some force since merely deciding on the laws is not sufficient to pick out worlds with only a single miracle. But this is a mistaken picture of Lewis' account.

The better way to think of things is to accept that there already are many possible worlds – we do not create them by considering counterfactuals.[185] These worlds differ from one another in that they have different patterns of instantiation of natural properties. Combination C will be instantiated once at some of these worlds, multiple times at others, and not at all at still more. Some of the worlds where C is instantiated a single time will have laws like our own, only with the addition of exception clauses as suggested. Some worlds where C is instantiated multiple times will have exactly the same laws as single-C worlds. Some worlds where C is instantiated will have wildly different laws or perhaps none at all. None of this is dependent on counterfactuals in any way. These worlds are not treated equally when it comes to counterfactuals. When assessing how close these pre-existing worlds are to our own, we turn to the priority list given previously. One of the priorities given is to minimise the number of miracles that occur. This minimisation is not meant in some creative sense where we influence other worlds, but simply that those worlds which happen to have more miracles are less similar to our own than worlds with fewer miracles. This is why multiple instantiations of C is not an issue. Nothing prevents this from occurring, so there are worlds with multiple instantiations. But they are less similar to our world than those with only a single instantiation of C. So when assessing counterfactuals like 'Had I struck the match it

[185] Lange may not have considered this as it appears to involve a commitment to modal realism. For my part, I am unconcerned by such a commitment; I take the unintuitiveness of the view to be largely overstated. But those squeamish about a plurality of concrete worlds can offer their own, less committal, interpretations of this claim (perhaps taking the worlds in question to be ersatz ones). See Divers (2002) for an overview of the options.

would have lit' we will only look to worlds where the miraculous lighting of the match happens a single time.

3.4 The problem of language

In order to understand Lange's second objection, we must first note that the canonical BSA requires every system to be stated in a language whose predicates correspond to perfectly natural properties. The usual reason given for this is that without this restriction, the account will misidentify the laws. Consider the predicate F which holds of all and only those things in our world. Then the system consisting of the single sentence $\forall x\ Fx$ will get counted as the best. It is simple, almost certainly simpler than its competitors. It is also maximally strong, for such a system will hold at only those worlds indiscernible from our own (should there be any). Yet despite being the best system, it does not give us the laws. For this sentence will imply all the truths of our world and hence all of the regularities. Consequently, every one of the world's regularities will get counted as a law: an absurd conclusion! Not only would this obliterate the distinction between laws and accidental regularities that the BSA was supposed to maintain, but it would make a mockery out of scientific practice. We would not discover the best system through empirical investigation and experiment but through simple armchair reasoning.

Clearly something must be done to prevent this degenerate system from being counted as best. The specific language requirement does exactly that. The predicate F does not correspond to a perfectly natural property (to give one reason: it is not sparse in the slightest). To state the system consisting of $\forall x\ Fx$ as its only axiom in terms of perfectly natural properties would require a very long complicated chain of definitions from those natural properties to the property referred to by F. Such a chain would cause the system to score very poorly on simplicity and hence no longer be counted as the best.

But while this reason for stating all systems in Lewis' favoured language is well-known, there is another reason.[186] Simplicity is a language-dependent notion. A single system can differ in how simple it is counted as depending on which language we are considering. In the terminology of Cohen and Callander – who credit Quine – simplicity is an immanent notion rather than a transcendent one (that is, defined relative to a system of basic predicates rather than independently of such a system). This is brought out well by an example from Loewer.[187] A system containing the sentence 'All emeralds are green' will get counted as more simple than 'All emeralds are grue' in a language whose basic predicates include 'green' but not 'grue'. Exactly the opposite result occurs if we consider a language whose basic predicates include 'grue'. Since simplicity is relative to language, simplicity comparisons of systems stated in different languages is impossible. One option when faced with this problem is to follow Lewis' example and demand that all systems be stated in the same language. The difficulty then is in specifying what language this is and giving motivation for singling it out as special.

Following Lewis' choice of language, it is easy to see how these questions are intended to be answered. The language is one whose predicates refer to perfectly natural properties. That is not to say that we currently have access to this language, in fact we most likely do not. It is the business of physics to, amongst other things, discover what the perfectly natural properties are. As no-one thinks that our current physics has achieved a final theory, it is reasonable to assume that a complete inventory of the perfectly natural properties has not yet been completed. But if we are optimistic then we might believe that we have come to understand some of what the fundamental kinds are.[188] As to why we should care about this particular way of describing the world, we should note that perfectly natural properties are supposed to correspond to objective distinctions in the world. To reuse a well-worn metaphor, if we were to carve the beast of reality at the joints, we would be carving it along

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[186] Cohen and Callander (2009) deserve credit for forcefully reminding metaphysicians of this, but we should also credit Lewis whose original discussion of these issues brought up exactly this point.
[187] Loewer (1996).
[188] We might compare this to Loewer (2007) where he discusses a property/law package deal. It is really not so different to what Lewis seems to have in mind at times.

lines demarcated by such properties. This is a suitable language because it respects the distinctions that are present in the world's fundamental character.

Suppose – for now – that this is the best solution to the issue of selecting a language. The best system is the system that has the best balance of theoretical virtues when stated in the language whose predicates refer to perfectly natural properties. It is natural, then, to extend this to the EBSA. The second-order best system must also be stated in a particular language in order to avoid a similarly degenerate choice of predicates: if G applies to all and only those sentences in the first-order best system, then $\forall x\ Gx$ will be the second-order best system. This would be a disaster, for such a system would entail all of the first-order laws and so all those laws would also get counted as metalaws. This parallels the problem in the first-order case exactly: it gets the wrong results and arrives at them through a priori reasoning instead of empirical investigation.[189] We might hope that by adopting the same language that we did in the first case, we might avoid the problem in the same way.

Unfortunately, this move is not as promising here. It would certainly block predicates like G, given that it is no more natural than F was. But, as Lange points out, it blocks rather too much. Metalaws are not stated in terms of perfectly natural properties, but rather ones like *covariance under temporal displacement*. There are several issues here. First, perfectly natural properties are intrinsic properties of spacetime points, whereas the properties of metalaws hold of members of the (first-order) best system. Second, properties of metalaws are mathematico-logical properties. Third, they are redundant. Suppose that we have a description of the fundamental nature of the world in terms of perfectly natural properties. Then a further description of the best systemisation of that distribution would not add anything new.

For these three reasons, the second-order best system will not be stated in terms of perfectly natural properties. This is a problem, as we require all systems to be stated in a language which takes these to be its basic kinds. A translation of the best system into the appropriate language might well be available, but this alone will not resolve the issue. Since metalaw properties are not perfectly natural, a translation of them

[189] Lange (2011).

into perfectly natural ones will, at best, be a blow to the system's simplicity.[190] Less optimistically, the required gruesomeness of the resulting sentences might well be enough to destroy the chances any system had of being the best.

I take there to be three distinct ways in which one might respond to Lange's objection. We might bite the bullet, and insist that the EBSA, formulated in Lewis' canonical language, really does get the right metalaws despite this issue. Second, we might change the account of perfectly natural properties so that properties of metalaws could be natural too. Third, we could take this to be motivation to drop this particular language requirement and adopt an alternative proposal. Let us take these in order.

To motivate the first response, recall that we are dealing with the *Best* System Account; we are not dealing with the *Great* System Account or even the *Fairly Good* System Account. To put the point in a less pithy way, for a system to count as best it need only be better than its competitors. It does not need to be considerably better than its competitors.[191] Nor does it need to score particularly highly on whatever the metric for assessing which system best balances strength and simplicity is. A system that does not balance these virtues particularly well, but whose competitors are even worse, will still count as the best system. With respect to Lange's objection, a Humean inclined towards this line of response might point out that exactly the same considerations apply to the EBSA. All that has been shown is that a system consisting of our current candidate metalaws will not be particularly simple when stated in terms of natural properties. It has not been shown that rival systems will be better balanced than this particular system. Perhaps all prospective systems fare poorly with regard to simplicity when expressed in Lewis' language, on account of their

[190] This is the more optimistic result as it would still allow for there to be metalaws in the envisioned way; they just would not be those that we take actual science to be discussing.
[191] Although we might hope for this. If a system is far and away best, then the regularities will match particularly striking patterns in the world and so we would be more inclined to accept these ones as laws as opposed to some other regularities. Lewis hopes for the best system to be *robustly* best while considering how objective his metric is, although I do not see this as an essential feature of the account.

subject matter: patterns found in the first-order laws. Or perhaps those that do not lack in simplicity are woefully lacking in strength.

Indeed, if all competing systems score badly then it may not even make much sense to talk of their being bad at all. It would be more accurate to realise that a system will only balance strength and simplicity well or poorly relative to other systems. To achieve good or bad balance in a non-relative way would require there to be some independent metric which all systems are scored according to. But – at least in the original BSA – there is no such independent metric.[192] On this view, it is entirely irrelevant that second-order systems of regularities will not be as simple as we might have initially thought. For being simple in a way that is not defined relative to the other systems on offer is not merely unimportant, but not something that it makes sense to speak of.

There is a cost to making this response. While Lange has not demonstrated that rivals to the system we think gives us the metalaws will achieve a better balance, I have also not supplied a reason to think that they will not. It is a weakness of the response that it simply leaves the matter open. Worse, there might be some reason to think that at least some of these rivals will do better. Suppose that we have a system which is simple compared to its competitors when stated in terms of natural properties. This will not have regularities that we would recognise as metalaws, given that it is using different basic properties. But it might also manage to be reasonably strong in the sense that there is only a small range of possible worlds at which it holds. Nothing in the way that strength is spelled out (insofar as it is at all) on the BSA prevents such a system from ruling out many possible worlds. The existence of such a system would prevent the system we want to come out as best from doing so.

[192] If there was a metric of this sort, we might expect systems to not only have to be better than their competitors but also to achieve some minimum score. This would ensure that the regularities it identifies are strikingly strong and simple; that they are not merely the best regularities available in a world lacking much order, but also that they are what we would be happy to consider laws (supposing that we might not want to admit there are any laws in a somewhat – but not excessively – disordered world). Much more would obviously need to be said to properly motivate this sort of BSA extension.

This argument is rather weak, of course. But the point remains that we have no strong evidence one way or the other as to whether a system, stated in terms of perfectly natural properties, could capture the second-order regularities better than the system, not stated in terms of those properties, that recognises the metalaws that science does. There are echoes here of a worry expressed by van Fraassen (among others) that the regularities picked out by the BSA as laws will not match up with those that our best science takes to be the laws.[193] Further argument might push us to regard it as more or less likely to think that the EBSA will misidentify the metalaws. Currently, however, I take this uncertainty to count against this response.

The second line of response is to accept that Lange has identified an issue with the EBSA, but rather than change the details of this approach to (meta)lawhood, change some of the background resources appealed to: specifically, the account of naturalness. To see the difficulty in following this approach, consider the following list of features the perfectly natural properties are supposed to have.[194]

They are supposed to serve as a supervenience base for the world's less fundamental existents. They are independent in the sense that they can be freely recombined to form new possible worlds (so there are no necessary connections between any of them). Relatedly, they are without redundancy: no perfectly natural property can be defined in terms of any other. They underwrite duplication, which can be analysed in terms of them, and hence intrinsicality too, which Lewis analyses in terms of duplication. They are discovered empirically: this need not be via physics, although properties discovered by any other branch of science are likely to be redundant. They allow for comparisons of naturalness to be made for all less natural properties (this is the chain of definitions idea that Lewis had; the longer the chain of definition required to connect a property to the perfectly natural ones, the less natural it is). They feature in laws – presumably fundamental ones. They pick out objective similarities and differences between objects which possess them. They serve as reference magnets in order to avoid Putnam's Paradox-style worries. The degree of this naturalness is not a contingent matter: properties are natural or not in all worlds,

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[193] van Fraassen (1989) p. 53.
[194] This is taken from Dorr and Hawthorne (2013). Compare with the list in Loewer (2007).

rather than their naturalness varying according to the world at which they are instantiated.

This list is not exhaustive, as there are other roles that Lewis appears to be appealing to in other articles. For example, spacetime relations, the only kind of perfectly natural relation that Lewis acknowledges, function as worldmate relations. That is, an object is related by these relations to all and only those objects which it shares a world with. But there is no need to identify every last bit of use naturalness is put to. The point is presumably clear by now: natural properties play a great many different roles. Attempting to modify this account of naturalness so that perfectly natural properties can feature in metalaws as well is more likely to cause problems than resolve them. Although, as Dorr and Hawthorne argue, there is considerable tension within this list as certain combinations of roles are not obviously compatible with one another. But perhaps this just adds motivation to respond to Lange by modifying naturalness: the account is already broken so we risk nothing by trying to fix it!

The point can be made in a bit more detail. Recall that Lange identified three problems with taking the properties featured in metalaws to be perfectly natural. They are properties of regularities rather than spacetime points, they are mathematical or logical in nature, and they are redundant. At first glance, the second issue is the most promising one. Allowing mathematical properties to be perfectly natural does not appear to directly conflict with most of the roles identified above. We might wonder what exactly they might serve as a supervenience base for, although that is no great issue. The requirement is not that every fundamental property be a base for others, but rather that all non-fundamental properties supervene on the distribution of perfectly natural ones. That is compatible with nothing supervening on certain perfectly natural properties. Similarly, the requirement that they be discovered by empirical means is less of an issue than one might think. So long as we do not confuse that with the requirement that all perfectly natural properties be physically significant in some strong sense, there is no reason to think that physics cannot discover that a certain mathematical property is natural.

That said, the independence requirement is going to be a sticking point. It is one that we are going to run into in two ways. First, as Lange has said, once we have one description of the world in terms of perfectly natural properties, any additional descriptions of the world are going to be redundant. They cannot be giving any new information. If they were, then the distribution of perfectly natural properties that was first described cannot be serving as a supervenience base for all of the non-fundamental properties. Saying that a certain regularity does not vary over time is a short, accessible way in which to convey information about it. But it is not the only way to convey that information, we could also give the precise distribution of physical magnitudes over time (assuming that we had access to this). Such a distribution would ground both the fact that a regularity holds and the fact that this regularity does not vary over time. The convenience of the first form the information took does not impact on the fact that it is, in principle, necessary to state separately.

The second issue with independence in allowing mathematical properties to be natural is necessitation. Truths regarding how some mathematical properties are related to others are standardly taken to be necessarily true. Yet natural properties have to be nonmodal; they cannot introduce necessary connections! At least, not without thereby being incompatible with the entire Humean project that Lewis advocates.

Finally consider the first issue that Lange identifies: properties that metalaws express hold of regularities rather than spacetime points. Relaxing this requirement in order to allow properties of regularities to be perfectly natural conflicts with the Humean Mosaic. No longer would the world fundamentally be a distribution of perfectly natural properties across spacetime points. Perhaps this is no great loss. We might be willing to sacrifice the mosaic metaphor in order to preserve the spirit of the account, as suggested in chapter one. But the issue goes deeper than just that. How would we recombine properties of regularities? Presumably not freely, since they are dependent on there being appropriate regularities at a world in order to be instantiated there. To make matters worse, remember that these regularities are regularities in the distribution of natural properties. So instantiation of the properties found in metalaws is dependent on certain arrangements of natural properties. Classing such properties as perfectly natural as well blatantly violates the requirement that perfectly natural properties form a mutually independent supervenience base for all other properties. And this requirement does not look negotiable, since this is a (or perhaps even *the*) central feature of naturalness. If this line of response to Lange involves making such a drastic change to the nature of naturalness, it seems far more promising to take an alternative line where naturalness is no longer connected to laws in the same way.

This leads us to the third way in which we might respond to the problem of the properties in metalaws not being natural. In short: remove the requirement that they be *natural* properties! The immediate worry is that this opens the account up to the problems Lewis solved with his choice of language. Would we not just have systems with gerrymandered properties – like the aforementioned F and G – being counted as best? That would be a damning result. And how would we respond to the worry that there is no way to compare systems stated in different languages?

A way to resolve these issues has been offered by Callender and Cohen, and Schrenk. In chapter four, I lay out the details of this approach and then consider how this Humean account of metalaws works in practice. At the present point, however, let us note that the changes to be considered shortly have little bearing on the majority of this chapter. The final regularity view of symmetry principles treats those invariances in the way that has previously been suggested. Hence the discussion in section 3.2 of this chapter on the roles played by the second-order account still applies.

Chapter 4 The Humean account of metalaws

The previous chapter ended on something of a cliffhanger: the Best System Account looks to have a natural extension that covers symmetry principles understood as metalaws, but Lange has brought out a problem for that extension. As the language used by physicists in statements of metalaws does not match the language of perfectly natural properties, it looks like the extended BSA will either not recover any metalaws or it will give us the wrong ones. Given that this entire manoeuvre was motivated by a desire to capture more of scientific practice, that's a pressing issue. Fortunately, there's a solution available. This chapter turns to the Better Best System Account and its loosening of the strict language requirement. We can combine this very similar approach to laws with the second-order extension that has already been discussed to offer an account of metalaws that does not run into the same problems of language. Once this has been set out, I turn to two applications for the account. The first can be seen as a challenge to Humeans: either offer an account of the relationship between symmetries and particle behaviour or accept a form of necessity. The approach that this thesis has been working towards allows Humean to take the first route and thereby avoid being forced into a radical reconceptualisation of the world. The second application looks ahead more speculatively. Much of the science mentioned thus far has been that of physics – be it classical or quantum. It is natural to wonder whether there is anything to say about metalaws and the special sciences, especially given the connection between the Better Best System Account and sciences other than physics. Biology is used as the example, and I suggest that there is a mutually beneficial relationship here: as the Better Best System Account provides assistance to Humeans seeking to capture metalaws, so too does this interpretation of metalaws help Humeans to provide a regularity-based interpretation of biology.

4.1 Better Best Systems

As just indicated, there is a model for how we might answer the question of language raised in the last chapter: the Better Best System Account (BBSA). As the BBSA is concerned with offering an alternative to the usual BSA, it makes no mention of symmetries or metalaws. But there is no reason why we could not extend the approach so as to include these. The core insight behind the view – that the original BSA needs adjustments to properly accommodate special science laws and that modifying the vocabulary used is the first step to making these adjustments – has been recognised by various philosophers. In what follows I will largely be following the presentation of the account favoured by Cohen and Callender, as I take it to be the most widely known version (and the source of the account's name).[195]

The central change to the BSA that Cohen and Callender make is to remove the claim that any particular choice of basic predicates or properties is picked out by the world as objectively special. There is no sense in which any language that we choose to formulate systems of laws in is better than any other – at least, that is, no sense that does not make appeal to our particular contingent interests or abilities. As an example of this, we might compare a language with green and blue to one with bleen and grue. The former language might be easier for us to work with (and so is special to precisely that extent) or come to mind more immediately, but this does not mean that there is anything objectively better about it when it comes to picking a language to formulate laws in.

In their paper, Cohen and Callender associate the BBSA with the adoption of an attitude of 'explosive realism' concerning natural kinds. Rather than there being one objectively privileged way to carve up the world at the joints, there are many different carvings, none of which mark out objective joints better than any other. It just happens that some of these carvings are more useful to us than others. (Compare this to unrestricted mereological composition: the moon and the pennies in my pocket might compose an object, but it is not one that we therefore need to

[195] For examples of this move, see Roberts (1999), Halpin (2003), Schrenk (2008), Schrenk (2014), Cohen and Callender (2009), and Callender and Cohen (2010).

pay any special attention to.) This, they argue, is more amenable to empiricism and what scientists are doing when they posit new fundamental kinds. Further, it will give us an account of lawhood that includes more than just the fundamental laws of physics. The standard BSA is unable to incorporate the laws of sciences like biology, since the kinds that they appeal to are not fundamental ones. As perfectly natural properties correspond to only fundamental kinds, systems of special science laws suffer from the same problem Lange identifies for systems of metalaws: they score badly on simplicity and so will not be the best.[196] But an account of lawhood that is not tied to natural properties avoids this worry entirely.

That said, it is worth mentioning that we do not have to be as thorough-going as Cohen and Callender's rejection of natural properties. All that is required for their account of laws is that systems of laws don't have to be stated in the language of natural properties. It's an entirely separate matter as to whether we reject such properties altogether. While we might see no need for them to be as closely tied to laws as they are in the BSA, we might think that they do useful work in other areas of metaphysics. Recall the extensive list given in the previous chapter of uses that Lewis put them to! It might initially seem surprising to have an account where the laws are not tied to that kind of fundamental properties while still accepting their existence, but it is not incoherent. And if we adopt the view of laws that the BBSA urges, the sense of surprise is diminished.

So to the details of this view. The nature of the laws is still given by the system which best balances the competing virtues of strength and simplicity. However, rather than having to be stated in a single specific language, systems of laws can be formulated in any language. Lawhood, then, becomes a language-relative notion. We might still take which regularities are the laws to be an objective matter; we do not get to freely choose what the laws are. But this is a rather constrained notion of objectivity, since they are objective only relative to a choice of language. Each language has its own

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[196] That is only half the problem, of course. The BSA also demands regularities be exceptionless and so encounters difficulty with ceteris paribus laws. See Schrenk (2014) for a suggestion that allows both the BSA and the BBSA cope with such laws. For worries about whether the BBSA copes any better than the BSA with ceteris paribus laws, see Backmann and Reutlinger (2014).

competition for best system, and different languages might (and almost certainly will) have different laws. Some languages might not even recognise any laws, should they lack the resources to describe the world's regularities. Other languages, with overlap in their basic kinds, might judge the same system to be best and so have the same laws.

This is how the BBSA is able to acknowledge laws of sciences other than physics. Pretend for the moment that our current science is complete and that we have discovered what the laws are. Then if we pick a language whose basic kinds include the kinds used in physics, the best system will be one which recognises the same laws that physicists do. So the laws – relative to the kind of language used in physics – will be the laws of physics. But we could also pick a language whose basic kinds include the kinds used in biology. Then the laws – relative to the kind of language used in biology – will be the laws of biology. A systematisation of the laws of physics will not be judged best relative to this second language as such a system will not be simple in that language. Rather, its properties will appear to be horribly gerrymandered.

We do not need to view the special sciences as fully autonomous from physics in order to make use of the BBSA. As an example, recall that Albert and Loewer's advocacy of the Past Hypothesis, the claim that the world started in a low entropy state, runs into a snag when combined with the orthodox BSA. Entropy is not a fundamental property and so the Past Hypothesis will look wildly disjunctive when translated into the language of perfectly natural properties. A move to a form of the BBSA could therefore be motivated for them: if we consider the best system for a language that includes entropy among its basic predicates, it becomes plausible that statements of the Past Hypothesis will be simple as intended. Although Albert and Loewer do not appeal to the BBSA, doing so would allow them to avoid the issues that are associated with either ignoring the problem or stipulating that the correct language for the best system is one that includes entropy. That said, more work would need to be done here to get the result they desire: that the special sciences end up being derivable from physics.

The BBSA does not solve the problem of cross-language comparisons as do those who stipulate a favoured language. Rather, it completely rejects the problem. For this is only a problem if we think that what the laws *really are* is independent of language. Then it makes sense to look for a single once-and-for-all best system. But without such a conception of lawhood, there is no need to compare systems in different languages; even if doing so were possible, we would not discover anything deeper about the world by making the comparisons.

Similarly, there is no need at this level to reject languages that include predicates like F: predicates true of all and only those things in our world. Languages which include predicates like that will have uninteresting best systems. But so what? That system has no privileged place above any other. Its disconnection from empirical inquiry is clear: science will not discover that $\forall x\ Fx$ is our one and only law because scientists do not seek to understand the world through the use of kinds that predicates like F pick out. So we can tell from the armchair which systems will be best in certain languages, but should not be concerned because we simply lack any non-philosophical interest in these languages.

Might the standards for judging which system is best differ across different languages? They might, although whether one wants to defend that depends on how committed one is to the objectivity of the standards in the first place. Allowing the standards to vary sits well with a 'look to science' approach. Since there is little a priori reason to think that the standards applied in, say, biology are the same as those in physics or sociology, there is some reason to want the standards to vary. We could then say that the standards are whatever those employed by the relevant scientific field are. It is for the scientists to pick their own standards: neither philosophy nor the universe nor even any other scientific field forces a choice upon them. This respects the autonomy of the various sciences.

Varying standards bring with them some issues. For one, it makes the laws dependent on our choices. This looks worse for the standard BSA than for the BBSA of course. With laws relativized to languages, we already get a plethora of laws. That they depend on our choice of standards does not change anything deeper about the world. Lewis may have been worried by ratbag idealists trying the change the laws by changing their thinking, but the worry loses a lot of its force when laws are deprived of a metaphysically active role.[197] What does it matter if we pick out some descriptions of patterns as more important than others? The distribution of fundamental properties doesn't change according to our choices! Nor is it clear that, given such a distribution, there is sufficient variation in the standards that could be applied to make the laws come out as anything we want. Making the account doubly relative would result in the laws depending on both language and standards choices. Perhaps this is pragmatically defensible: some choices of standards would output some rather odd laws, just as some choices of language do. But just as we are not interested in every one of the languages that law candidates can be formulated in, there is no reason to think that we would be interested in every one of judgement standards that could be applied.

There's more that ought to be said in defence of varied standards, but I think that indicates the direction that one ought to look. More problematically, we might worry about our access to the standards in question. If we pick the standards, then the problem is that there exist languages, and so candidate systems, which no human has considered. What standards apply there? If each language has its own objective standard, then we lose the sense in which we are letting each science decide how best to find its laws. There is no good reason to think that every scientific field will chance upon the correct standards. (And how would we know that, for example, the geologists are using the objectively correct standards but the cosmologists just can't get it right?) Finally, disagreement. Which standards are in play when a scientific field disagrees (explicitly or implicitly) on the standards? Or when the practices of that community change? None of this is to say that there is no similar problem for the standard BSA. On the contrary, just how objective the BSA standards are is a matter of debate, and Lewis' defence of their objectivity is far from confidence-inspiring. This is not the first time that someone has worried about whether scientists are discovering what the Humean takes to be laws! I take the upshot of this to be that the issue of objectivity of standards in the BBSA is similar to the issue of objectivity in the BSA. If one is happy that the balancing of strength and simplicity is not

[197] Lewis (1994a).

dependent on us, then the language relativism move need not change anything. If one started with concerns, then the BBSA provides an opportunity to double down on what will look to be an unavoidable relativism of the laws.

Let us set these unresolved issues to one side. Since the problem that the BSA has with metalaws is analogous to the problem it has with special science laws, it should be clear how the extension of the BBSA goes. Instead of there being a single non-relative best system (or tie of a few non-relative best systems) that gives us the metalaws, we can make the account of metalaws language-relative. For a given system of laws, we can use many different languages to formulate the metalaws of those laws. Relative to each of those languages there will be a competition for the best second-order system. If we wish to recover the metalaws that physics recognises, then we had better not choose a language which has predicates corresponding to the basic kinds of economics. That choice might recover some striking regularities in the world, but it lacks the resources to pick out the symmetry principles that physicists care about. Similarly, we should not choose a language with basic predicates only for perfectly natural properties. Such a language will have as its best system regularities that physics does not recognise for the reason that Lange identified. But since there is no requirement to choose that language, no sense in which it is objectively better than the other choices, there is no problem with this. The problem that Lange discovered with this approach to incorporating symmetries into our account of laws simply does not occur.

It is theoretically possible to preserve the original BSA when we are dealing with laws, but use this Extended Better Best System Account (EBBSA) to deal with metalaws. But such an account is not an appealing one. Doing this would mean claiming that the laws which pick out striking regularities in the Mosaic are independent of us and our choice of languages, but that the metalaws which pick out striking regularities in the patterns in the Mosaic are language-dependent. That is a major asymmetry and one that is hard to motivate: nothing like that seems evident in the practice of physics. Such a hybrid account is vulnerable to the (accurate) accusation that it is simply an *ad hoc* move made to escape Lange's objection. Language-relativity would be better motivated if it were embraced more thoroughly. That is, accept Cohen and Callender's arguments for the BBSA and then incorporate symmetries as metalaws by extending their account of laws in the natural way.

4.2 Symmetry principles and necessary connections

With the structure of a Humean treatment of symmetry principles now in place, we can look for places to apply the account. Given the claims that Humeanism pays insufficient attention to modern science, deploying the EBBSA to further engage with scientific practice would be ideal. McKenzie has provided an opportunity to do just that.[198]

Look to particle physics and make the assumption that a law associated with some particles exhibits a symmetry. Since all of the fundamental interactions that we know of are connected to symmetries, this is an entirely reasonable assumption to make; it is hardly a special case. In such cases, we find multiplets as indicated in the previous chapter: a family of particles which share in their determinable properties. As has already been discussed, there is considerable heuristic importance attached to the holding of symmetry principles. For one thing, they figure in the prediction of particles that have not yet been observed, with the prediction of the omega minus particle via the Eightfold Way being one of the most famous. With the prevalence of symmetries in our fundamental theories, we should expect the world to have a number of these multiplets.

The problem for Humeanism, McKenzie goes on to say, appears when we consider otherworldly duplicates of actual particles. The mathematics of our theoretical understanding of these multiplets proceeds via semi-simple Lie algebras and a consequence of their treatment in this way is that a multiplet will correspond to one algebra only. Hence if we examine a world with particle duplicates:

> [I]f we understand the laws operative there along quantum-mechanical lines it follows that those laws *must possess the symmetry of the laws of the*

[198] McKenzie (2014) pp. 45-61.

> *actual world*. But that represents a *hugely* informative and non-trivial constraint on the laws that any such set of duplicates can accord with … information about symmetry is *highly* non-trivial information from an empirical point of view.[199]

Humeanism – particularly in the style of Lewis – is built upon a foundation of categorical properties, those that do not impose any constraints upon the laws that they enter into. For example, there are worlds where Coulomb's law for charged particles is an inverse cube law as opposed to an inverse square law, and other worlds where there is a change of sign so that like charges attract rather than repel. Whether there are any limitations on the form that a law for charged particles can take remains an open question; McKenzie points out that the writings of various metaphysicians seem to indicate that there are no non-trivial limitations whatsoever. This ties in with the contingent status of laws: since there are worlds with duplicates of charged particles that do not obey Coulomb's law, that law cannot be necessary. At least, not in an unrestricted sense, although there is still room in the view for it to be physically necessary. If the form that the laws for particle duplicates can take is limited by symmetries, then those particles cannot only possess categorical properties. If they possess non-categorical properties, then there are associated laws which are not contingent in the way Humean laws are normally taken to be. This is no mere cosmetic change: the package of contingent laws and categorical properties is tied into the denial of necessary connections. But if there are constraints on how things can behave, the central Humean commitment is in trouble.

The laws for duplicates will not in general be uniquely determined by the holding of these symmetries. McKenzie considers renormalizable local gauge theories as a candidate for our best theory of fundamental laws, and here the laws will be almost uniquely specified by the relevant symmetries. But this makes little real difference to the Humean position. The problem is that taking symmetries to impose constraints introduces necessary connections. How tight those constraints are is very much a secondary issue, since it is their very presence that causes difficulties. (As McKenzie's goal is to rework the Humean – anti-Humean debate, she also thinks the

[199] McKenzie (2014) p. 55, italics in original.

Humean's opponents are in trouble and that this does not indicate a triumph of one side over the other. I ignore the issues for dispositionalists here as tangential to this project.[200])

Before turning to the reply that I favour, it is worth considering McKenzie's suggestion for how Humeans ought to respond. The main idea is that the position may need updating, but that something resembling it is still a live option. Humeans, after all, do not have to reject all necessity. Their denial is generally taken to be a denial of necessity in nature, of there being necessary connections between distinct existents. Mathematical or logical necessity, on the other hand, is not usually treated with the same suspicion. Since McKenzie's problem gets going because the relevant mathematics associates a multiplet with only one symmetry principle, if this is a mathematical necessity then it does not need to be treated as having anti-Humean implications:

> is it not at bottom a *mathematical* fact that a set of particles, defined by a given set of determinate values, cannot participate in laws of quantum-theoretic form with arbitrary symmetry structure? It seems to me that it is; and as such, it seems to me that the mere fact that the laws describing a given set of particles may be unique and in that sense *necessary*, it is not a necessity that Hume himself need have felt particularly troubled by.[201]

The upshot is that Humeans can deal with necessary laws, and not just in the standard reductive way. The usual belief that they cannot arises from an outdated debate, and had the participants in that debate paid more attention to the relevant physics, we would not be burdened with such misconceptions. There is a further consequence too: since Humeans end up with a necessitarian position, a stance typically adopted by anti-Humeans, the latter group must come up with a new way of characterising their position.

In addition to being interesting, McKenzie's suggested modified Humeanism gets right to the core of what forms of necessity are acceptably un-mysterious. However, I think it would be a mistake for Humeans to accept her offer. The issue is not that

[200] But see French (forthcoming a) and French (forthcoming b) which bring out the tension between modern physics and disposition-based accounts.
[201] McKenzie (2014) p. 59, italics in original.

the lines of the debate become less clear: if anti-Humeans do not find this new Humean account acceptable, then it is up to them to set out an alternative. That said, I suspect that many would be more sympathetic to it. If necessity is back on the table, exactly what part of the account would they consider to be lacking? Nor is acceptance of mathematical necessity the problem. (I sympathise with the cry that distinct existents being 'forced' to go together is mysterious. I have very little grasp on the analogous claim that there is no reason why two and two should not make five.)

My worry is that the introduction of necessity is treated as acceptable when we take the particles in question to be defined by the mathematical values assigned to them by quantum theory. No part of that manoeuvre is specific to the theory being a *quantum* one. The same move appears to work in the classical context too: as long as we define the entities in question by the mathematical values that some theory assigns to them, we can then claim that there are unmysterious necessary connections between them of a mathematical nature. But if we can get necessity in the world whenever properties can be represented mathematically, then the floodgates have truly been opened. The 'unreasonable' effectiveness of mathematics in the sciences is no secret; its very indispensability is a key feature in attempts by philosophers to give an account of the subject.

We might want to point out that this particle definition could be meant in two ways. In one way, the particles are physical entities with the usual sorts of properties. Mathematics comes in as a representational tool, but nothing more: there is a reality whose nature is not essentially mathematical. If this is McKenzie's meaning, then I think the concern just expressed above is relevant. Representations often have features that their represented targets do not, and vice versa ('surplus structure' is a case in point). Justifications of mathematical necessity in the representation are not by themselves sufficient to justify necessity in the world. The sense of mystery, I think, is undiminished.

The other sense of the definition might be that the nature of the particles is tied to the mathematics. We might get at this by saying there is nothing more to the particles than their mathematical treatment in the theory. I am less sure what this sort of talk is supposed to mean. Perhaps that we have no understanding of the particles beyond our mathematical treatment. If so, then that is granted. But this form of ignorance does not mean that we must conceptually revise what we take to be the nature of the particles involved. That our theory does not say anything more than its mathematical content does not mean that we must think there is nothing more. That sort of claim seems to make more sense if we adopt a kind of anthropocentric arrogance in our epistemology, but I see little reason to believe that we ought to be able to understand all of reality in a non-mathematical way. Alternatively, perhaps this is the stronger claim that the world is mathematical in an ontological sense. That would not be unprecedented, but such a result requires a massive shift in our accounts of the fundamental. For just one example of this, consider what becomes of causation when mathematical entities are often characterised as abstract and so non-causal.[202]

In short, I am sceptical that accepting the necessity suggested by McKenzie's considerations is harmless. Even if we were to grant for argument's sake that it might preserve the letter of the Humean account, there is considerable unresolved tension with the spirit. If there were no other line of response, then that might simply be a bullet that Humeans would have to bite. Fortunately, there is an alternative response: we can deploy the EBBSA. The main move here might already be obvious, in that it is a denial of necessity. McKenzie's challenge arises because of the claim that duplicates of actual particles *must* behave in ways that are ultimately captured by appropriate symmetry principles. On the face of it, that is a similar claim to the usual ones involving laws. Two like-charged particles, for example, must accelerate away from one another in the absence of any other forces. Since the laws are necessary, duplicates of those particles in similar conditions must also accelerate away from one another. But for all this talk of the connection between laws and necessity, Humeans have a standard line of response. That particles always behave in ways described by the appropriate law. This is not because the law makes them

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[202] Tegmark (2006) is a good example of someone who takes seriously the claim that the world is fundamentally mathematical. For a different approach to a mathematical ontology, see Dipert (1997). For clarity's sake, I should note that I do not think that McKenzie is advocating for this radical a conclusion.

do so, it is because the law is an accurate description of their behaviour. That we expect duplicates to behave similarly is understood as a restriction of possible worlds: among those worlds with the same physical laws (that is, the same descriptions of particle behaviour), particles behave in the same way. But that is no great surprise!

As for laws, so for metalaws. If we understand the relevant symmetries through the lens of the EBBSA, they end up as higher-order descriptions of the mosaic, including the particles and their behaviour. So, that actual particles behave in ways described by the symmetries is because those symmetries are accurate descriptions of what happens (albeit somewhat indirect descriptions; the metalaws are more directly concerned with the laws). As before, the related necessity is reduced away to restricted quantification over worlds. Among those worlds with the same metalaws as ours, particles behave in the same broad ways – although if multiple first-order laws are compatible with the same set of metalaws, there is some more room for variation in behaviour than there was in the previous case. Providing something like the EBBSA is important for the same reason that the original BSA is important to Humean views. It is all well and good denying that there is any necessity in the world, but this negative claim alone is not sufficient. The onus is on Humeans to provide a positive account that says what is going on if not necessary connections. The account that I have indicated in this thesis provides the resources to not just deny that symmetries impose anti-Humean constraints, but also to offer an alternative metaphysical interpretation of them. This conclusion might be somewhat disappointing for those who share McKenzie's desire to reconceptualise the debate, since it amounts to the claim that Humeanism in its current form possesses the resources to make sense of this aspect of physics. But this is also an unsurprising result, as it is notoriously difficult to read metaphysics off physical theory. In lacking empirical consequences, metaphysical accounts are especially resilient when confronted with the findings of empirical science. Compare with the debate around spacetime substantivalism and relationalism. Despite the replacement of the Newtonian background against which it was originally conceived with the theory of General Relativity, the philosophical argument over the nature of that spacetime is still a lively one!

That said, while McKenzie does not consider the EBBSA – for obvious reasons – she does anticipate the more general denial of the necessity involved. Three reasons are offered against this. First, against the claim there could be worlds with classical, rather than quantum, physics, it is complained that it is not clear that there could be a classical world studied by physics.[203] Perhaps not. Ultimately, it makes little difference to Humeans whether our actual science of physics could make sense of a fundamentally classical world. There are, after all, physically impossible worlds with different physics to us. Some of those worlds will have observers broadly like us who systematise their knowledge of the world into empirical sciences. Some of those worlds with scientists may have theories that we would be happy to regard as an equivalent to our physics. But even if it turns out that non-quantum worlds fail to have any observers like us (perhaps because of consciousness being essentially tied to quantum states or some such), that would be no argument against the existence of non-quantum worlds. The Humean denial of necessity appeals to the plurality of worlds, but does not require that there be something like our science of physics practised at each or that our actual physics have the resources to make sense of every world.

Second, there is the more substantive question of why this response is being made now and not much earlier in the debate. Grant to Humeans that there are worlds whose laws that cannot be described in a quantum theoretic way. Why then is the existence of such worlds not an argument against anti-Humeans who think that the laws are necessary? It would cut a lot of unnecessary work out of justifying the Humean position if we could simply point to the existence of worlds with other physics and conclude immediately that the laws are not unrestrictedly necessary. Well a good reason not to try this approach is that no anti-Humean is going to look impressed! The reason why this response to McKenzie is not utilised in the canonical debate is that the difference between Humeanism and anti-Humeanism comes down to a difference in presuppositions. If one starts out with a commitment to there being worlds with wildly different physics, then the usual anti-Humean

[203] This is an appeal to considerations in the first chapter of Mirman (1995). While I utilise the resources of modal realism in my response, I see little reason why the usual modal alternatives could not also be applied.

accounts are going to look unsatisfactory. If one lacks that commitment, Humeanism looks to be appealing to a very questionable plurality. Insistence by either camp that they have the right account is not sufficient to sway the other side because it does nothing to change the presuppositions in question. We do not have to remain silent, of course. One side can still remind the other of the costs inherent in their presuppositions. Humeans adopting the Lewisian package of views must bear the cost of accepting the lack of quantitative parsimony that comes with the plurality of worlds. But, presumably, if one is such a Humean then that does not look like a great cost at all! The price is outweighed by the account's advantages, such as not having to accept an unexplained necessity.

One way to engage in this sort of debate has just been indicated: remind the other camp that their presuppositions come at a price. This might sway those still on the fence, but is unlikely to make a difference to the more committed adherents. The other way is to find a problem for the opposing account on its own terms. That is, find a problem that the opposing side will also view as a problem and not just part and parcel of the view. Outright inconsistency is one such problem, albeit a rare one. Tension with scientific practice is often another, assuming that the account is not explicitly a revisionary one. This is why the defence against McKenzie is not also a weapon to use against anti-Humeans. Views that take the laws to be necessary lack the basic presupposition that there are worlds with different laws and so will not regard this claim as offering a problem from the anti-Humean perspective. Since Humeans start with this commitment, there is little point in objecting that one is unsure of the existence of those worlds: that simply reminds Humeans of what is involved in accepting their views, it does not provide a problem in Humean terms.

Third, if we are discussing duplicates of entities, we first need to settle what those entities are. If we agree that they are quantum entities (be they particles or fields), then we must use laws of a quantum template to describe them. Necessity, therefore, is back on the menu. However, I see little way to make this response clear without having it beg the question. What is it for an entity to be a quantum entity? The most obvious response is that it is the sort of entity described in a quantum theoretic way. But if this implies that duplicates of that entity are also quantum entities and so must be described in a quantum theoretic way too, then it straightforwardly begs the question. If calling an entity quantum at the outset enforces a limitation of its otherworldly behaviour, then of course we must use quantum theory to describe it. But that necessity was introduced by calling the entity a quantum one. On the other hand, if an entity's being quantum does not impose any such limitation, then I fail to see what point there is in attaching the label. This settling of the nature of the entity before we start is either toothless or question begging. Similar comments apply to settling which sort of structural templates should count as laws. If this is meant to restrict our attention to the form that our quantum theoretic laws take, then it assumes that the laws *must* the quantum ones which is not an assumption that Humeans need grant. If it is taken as a request for a list of all the possible forms that laws can take, then it is an entirely unreasonable request. According to the Humean view, to do that requires knowing the goings on at every possible world, including those with fundamental properties that are not instantiated at our world. That is an epistemological nightmare!

The upshot of this discussion is that the use of symmetry principles in physics does not obviously motivate a rework of the entire Humean position. There do need to be some changes, so that Humeans are not left flatly denying necessary connections while being unable to explain just what they think is going on with symmetries. This is the function of the EBBSA, to provide the details of a positive Humean account that retains the spirit of the original motivation.

4.3 The special sciences

Let us turn to a somewhat more speculative use of the EBBSA. Recall that one of the original main motivations for the language-relative BBSA was the desire to cover the laws of the special sciences. The standard BSA permitted both axioms and theorems to be classed as laws and so allowed for special science laws in a derivative sense: those regularities that can be derived from the axioms of the best system are also laws. Immediate issues of reducibility arise. If one does not think that the entities studied by the special sciences are ontologically reducible to those studied by physics, then there is no reason to think that those higher-level laws will be derivable from lower-level ones. That is the opposite result one would be hoping for if the sciences are ontologically independent from one another, since it means that there are no high-level laws. Supposing that one is willing to grant an ontological reduction, it might still be the case that there is no in-principle reduction of the special science laws; perhaps there is no good way to translate the language of economics into that of physics. Of course, if we recall the doctrine of Humean Supervenience, we might note that the BSA was not designed for cases like these. It fits much better with Lewis' motivating picture of the world if the special sciences are reducible, both in the sense of their entities being nothing more than distributions of fundamental properties and in the sense of their vocabularies being translatable.[204] But even then, we still might end up with no special science laws. Perhaps the behaviour of macro-entities would require such long and disjunctive descriptions in a language of perfectly natural properties that no system with an eye for simplicity would include them among its consequences.

While the BSA agrees that there can be laws of the special sciences, this agreement is quite weak. The turn to the BBSA resolves all of the relevant language issues; a geological regularity stated in the language used by geology would not need to be very complicated at all. We have already seen that the resources provided by the BBSA are of use to Humeans looking to incorporate symmetry principles into their account of laws. The question that I turn to in this section is to what extent the help goes in the opposite direction. In other words, can the move to higher-order systems offer anything to Humeans with an interest in the special sciences? Laying my cards on the table, I think it has something to offer. Precisely how much it offers depends heavily on how one thinks of the laws of the science in question. To make things more concrete, I will use biology as my example. This, I hope, sidesteps the question of whether we want to count the like of economics or psychology as genuine sciences – although I hope that the structure of this example carries over to these more controversial cases even if the specifics do not.

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[204] In Lewis (1984), a suggestion is made that the less natural properties are connected to the more natural ones via chains of definitions.

Some might worry that biology is an unpromising area to begin a search for metalaws given that there is not even a consensus that biology has any laws! This is nicely illustrated by Beatty:

> All generalizations about the living world: (a) are just mathematical, physical, or chemical generalizations (or deductive consequences of mathematical, physical, or chemical generalizations plus initial conditions) or (b) are distinctively biological, in which case they describe contingent outcomes of evolution.[205]

Although it is not mentioned by name, we might note that the first disjunct corresponds closely to the treatment of biological regularities offer by the standard BSA. This presents a difficulty to the present work, since if there are no biological laws then we can hardly expect there to be any metalaws concerned with biological laws. Option (a) is intended to capture the way in which the likes of Newton's laws of motion are applicable to more than just point particles. Cats and dogs also accelerate when acted on by forces. But no part of that is due to their biological nature, it is entirely due to their being composed out of physical matter. So (a) blocks the laws of physics (or chemistry for that matter) from sneaking into the 'biological law' category merely because biological entities will also possess physical properties. Laws shouldn't be double counted in this way.

The other possibility mentioned in (a) is that biological 'laws' are really just mathematical generalisations. The problem this raises is the lack of empirical content. Mathematical truths can be discovered a priori, and so do not require us to interact with the world. But the laws of science are supposed to be empirically discovered. If a regularity can be discovered without us needing to consult the outside world, we might worry that the regularity is not directly concerned with the world after all.

Option (b) is intended to be no more attractive. As has been mentioned previously, the laws are supposed to be accompanied by a sense of necessity. This comes out clearly in efforts to distinguish them from the merely accidental regularities. The pen had to fall to the ground, but it is merely a (happy?) accident that everyone in the

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[205] Beatty (1995) pp. 46-47.

room was a philosopher. If the biological regularities are contingent then they fail to possess a core attribute of laws.

I do not wish to get drawn too far astray by this debate, but it is reasonable to indicate how a response to this dilemma might look. First note that there are examples of law candidates in biology which are arguably not merely mathematical, physical or chemical regularities. One such example of Kleiber's law, which relates an animal's metabolic rate R to its mass M:

$$R = M^{3/4}$$

Approximately, that is, but let us set aside the complications raised by approximate laws. It is important to note that while the exponent is 3/4 for animals, it is taken to be closer to 1 for small multicellular plants. Is this a mere mathematical tautology? The short answer is 'no'. Early approaches might have suggested something like that since they associated metabolic rates with heat dissipation, and this latter value is dependent on an organism's surface area. In line with this, initial proposals for the value of the exponent put it at 2/3, a value suggested through the mathematical comparison of surface area to volume. In 1932, Kleiber observed that the value of 3/4 fit the empirical data more closely for animals.[206] If Kleiber's law was a purely mathematical theorem, there would have been no need to examine the empirical data; armchair considerations would have sufficed. After all, one does not arrive at Pythagoras' theorem by observing large numbers of right angled triangles! Since empirical data was (and still is) relevant to determining the value of the exponent, Kleiber's law is not distinctively mathematical in a way that prevents it from having empirical content. Is the law a physical one masquerading as a biological one? This is somewhat less clear since to give an answer, we would first need to decide on what the criteria are for a generalisation to be distinctively physical as opposed to biological. There is some support for the negative answer though: the laws of physics do not typically trade in biological terms like 'metabolism' and 'animal'. Given the explicit usage of distinctively biological vocabulary, I am inclined to take Kleiber's law to be a biological law candidate. A rough and ready test: would we expect Kleiber's

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[206] Ballesteros et al. (unpublished).

law to earn its way into an orthodox best system? Presumably not. It doesn't refer to perfectly natural properties and we have already seen the issues that creates! So Kleiber's law is not a candidate for fundamental lawhood.

Turning to the second horn of Beatty's dilemma, we should say something about the contingency of biological regularities. The worry here is that law candidates are supposed to hold necessarily and are invariant under a large range of changes of contingent fact. Biological laws fail to share in this feature and so are unfit to be called laws. Their contingency is nicely illustrated by Rosenberg:

> Consider what was until recently thought to be the most invariant of biological regularities: all genes are composed of DNA. For a long time this regularity was subject to no exception. But because it remained invariant over a very long period, its operation provided an environment that would allow for the selection for any new biological system that could take advantage of the fact that all genes are composed of DNA. Such a system eventually came into existence – the RNA viruses, whose genes are made of RNA and which parasitize the machinery of DNA replication (the HIV virus is the most notable example of these viruses). Thus the regularity that all genes are made of DNA gives way to the regularity that they are all made of nucleic acids (either RNA or DNA). But we can be sure that the arms race of evolutionary competition will eventually undermine this new invariant regularity, by producing an alternative means of genetic transmission that exploits the regularity.[207]

If this was the most well-established regularity, then all other biological regularities will be more restricted in the spatiotemporal regions which they hold in. This is a rather different picture to the one we get from physics where (concerns about the period immediately following the Big Bang aside) the regularities are taken to hold across the entirety of spacetime. Biological regularities are restricted because of the selection pressures involved in natural selection. Should an environment change, different traits will be better adapted and so rise in prominence. Interaction with other species is often even more ruthless and the relevant regularities can change very rapidly indeed. Antibiotic resistance is a clear example due to the short lifecycles of the organisms involved: the rate of change is such that there is a constant need to

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[207] Rosenberg (2012) p. 12 fn. 11.

develop new forms of antibiotics to deal with the changing characteristics of the unwelcome bacteria.

Will the Humean class such restricted regularities as laws? A hard-liner might well reply in the negative, and point to their failure to be exactly true as further evidence of their unsuitable character. Fair enough, it has always been possible to dismiss special science laws and focus entirely on fundamental laws. But suppose that we wish to allow for special science laws. We will need to relax the strict requirement on truth, since candidates like Kleiber's law are only approximately true. This is just an instance of a more general point: finding the curve that best fits the data will often result in a formula that is not precisely true. Assume that this can be done. Then we might say that biological laws will deserve entry into a best system for biology, for they describe particularly striking patterns in the world. We might not want to let all of the candidates in, perhaps some hold of such a small range of organisms or over such a short span of time that they fail to be notable enough to be laws. That is no issue, since the BBSA already has the resources for dealing with those cases. Regularities that fail to contribute enough strength to justify their cost in simplicity do not make it into the best system.

The difference in necessity can also be accommodated in a natural fashion, simply by extending the treatment of physical possibility. There is space between the necessity of the physical laws and the accidental nature of today's weather in which we might locate biological laws. Under the standard Humean account, claiming that a law of physics is necessary is not to claim that it holds in every possible world. Rather, it is a restricted claim. The laws of physics together mark out a set of possible worlds as being physically possible (which is just to say that the histories of those worlds are consistent with our laws of physics). Ontologically, there is nothing special about such worlds, there are many other worlds whose histories mark them out as not being physically possible. It is in this way that physical possibility is a restricted form of possibility more generally. A similar treatment of biological laws can be made, where a set of possible worlds can be marked out by taking the biological laws and picking out every world consistent with those laws. The set of worlds so selected will be distinct from the set associated with physical possibility (the laws of physics can hold at worlds at which there is no life). But this sphere of ‘biological possibility’ is no less a kind of possibility for that fact. Compare: physical possibility is a subspecies of logical possibility, but that is not to the detriment of physical possibility. Since laws having a kind of necessity reduces to their being consistent with a restricted subset of the plurality of possible worlds, Humeans have no problem with taking biological laws to be less necessary physical ones.

One might worry that this does not fully answer the problem. Perhaps necessity was not the real issue with these laws, but rather their failure to be universal. A regularity that fails to hold across all of spacetime is no law. I suspect that this captures the objection more accurately, but we should note that it is also an opportunity. This is where the marriage of the BBSA and Humean metalaws can be put to work. Recall that the fact that the laws of physics hold for every spacetime region is captured by invariance principles: the laws remain the same despite spatial and temporal translations. To claim that the (first-order) laws are universal in this way is to make a claim about the content of the best system of metalaws: it is to claim that there are metalaws corresponding to this spatiotemporal invariance. Seen in this light, the requirement that a regularity be universal in order to be a law of physics is entirely dependent upon the appropriate regularities being in the second-order best system. To emphasise, it is not a part of the first-order best system, at least not directly. Restricted regularities provide less information about the world and so contribute less to system strength. A regularity’s being restricted might therefore prevent it from being in the best system, but this an indirect barring that comes down from the balancing competition rather than coming from a direct prohibition.

The application of this treatment of invariance in the biological case is simple enough. If there is no rule that directly prevents restricted regularities from entering into candidate systems, the fact that biological law candidates fail to be universal does not prevent them from being laws. Given the current evidence from biology, we should expect the best biological system to contain such non-universal laws. This has higher-level consequences: a second-order systematisation of the regularities in the biological laws will not include invariance under spatial or temporal translations. That is an interesting result, since it indicates that an oft-quoted difference between the laws of physics and biology comes out as a difference in second-order regularities. The contingency of biological laws is therefore a difference in degree (and metalaws), rather than a difference in kind.[208]

So we can use the EBBSA in a negative way, by making a claim about the sorts of regularities that do not hold over the laws of biology. But we can also wield it in a more positive way, by suggesting what might enter into the second-order best system. Here is perhaps the best candidate for a higher-order regularity: natural selection. Why is it that the usual biological law candidates are sometimes claimed to not be genuine laws? It is their contingent status, and the fact that each holds for only a restricted length of time. Why is it that they change so readily? The answer to this is nicely brought out in the earlier quotation from Rosenberg. The fact that all genes were composed from DNA provided an opportunity for biological systems to take advantage of that regularity, an opportunity that was seized by the RNA viruses. Every regularity will have properties that different biological systems will be better or worse adapted to. Those better adapted have the opportunity to flourish while the regularity holds and, in their flourishing, change the regularity itself. Since every stable environment allows for certain systems to prosper, no environment remains stable indefinitely. We attribute this 'arms race' for better adaptations to natural selection. It is because of the process of natural selection that no local equilibrium between different species remains that way forever: if the equilibrium can be broken to some advantage, then it will be.

Natural selection, then, acts on the biological regularities that get assigned lawhood. Those lower-level regularities fail to be universal because of the selection pressure imposed by natural selection. Of course, Humeans can deploy the EBBSA to offer a palatable translation of this talk of imposition. Biological systems exhibit regularities. These regularities differ in the regions of spacetime they hold over, with some being longer-lived or more wide-spread than others. Yet none are truly universal. Those regularities that make it into the best system for biology are the biological laws. Because of these facts, the best systematisation of the biological regularities itself

[208] Compare to Dorato (2012), who arrives at a broadly similar conclusion through different considerations.

exhibits (second-order) regularities. While it has no regularities corresponding to spatiotemporal invariance, it does contain the principle of natural selection, capturing the pattern created by the variation among the first-order regularities. This appears to be globally invariant, in the manner of the metalaws that we have already encountered. The metaphysical picture here is still bottom-up, just as it is in the case of physics.

While it is not clear that there are any biological symmetry principles as there are physical ones, we can still find evidence of high-level features being put to similar uses. Metalaws are used to explain laws in a manner like the explanation of phenomena by laws. The most famous example of this is the claim that the conservation laws hold *because* of invariance principles. Earlier we mentioned Kleiber's law, which relates an animal's metabolism to its body mass. There is a notable attempt to explain why Kleiber's law holds, advanced by West, Brown and Enquist (the WBE model).[209] Organisms effectively have a transportation problem to solve: their cells need a supply of materials, but the cells may be located far from the materials that the organism takes in. The organism, therefore, needs a transportation system to ensure the cells get the supplies that they need. WBE make three assumptions about the transportation systems that beings like mammals have, and from these assumptions derive the quarter power law. First, to supply the entire organism, a space-filling pattern is required. Second, the final branch (or capillary) of that system is a 'size-invariant unit'. Third, the energy required to distribute resources is minimised (the evolutionary advantages of this last assumption should be obvious). Optimisation of the energy transportation system results in a tree-like fractal branching. The regularity observed in animals – that their metabolic rates and masses are related in this way – falls out of the branching structure that they all have in common. This structure in turn is a consequence of the deeper 'requirement' that energy costs be minimised. The authors take the same kind of reasoning to explain the other allometric power laws.[210]

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[209] West, Brown and Enquist (1997). See Dorato (2012) for an overview.

[210] The WBE model is not uncontroversial. See Dodds, Rothman and Weitz (2001) for a representative critique. The key point as far as this work is concerned is not whether the WBE model is successful, but that this kind of attempt to explain biological laws by appeal

In summary, I think that biology is a promising area to put the EBBSA to work. To what extent its resources will be used depends on what one thinks about the status of biological laws. If there are no laws, then giving a philosophical account of laws in biology becomes a trivial matter. That is to the good: it is an empirical matter whether there are any and so the responsibility for determining their existence falls upon working scientists rather than armchair philosophers. Supposing that there are regularities we are willing to call laws, we can not only offer a best system account of those laws, but also capture the way in which they differ from the universal laws of fundamental physics. Deployment of the EBBSA here lets us say something about the relationship between natural selection and the usual law candidates. It also draws a parallel between attempts to derive Kleiber's law from underlying principles and similar work in physics, where the laws are sometimes thought to 'drop out' of underlying invariance principles. I leave as an open question the extent to which the other special sciences are amenable to this treatment.

to more fundamental principles has been made. Notice that the paper by Dodds et al. questions the actual mathematical derivation presented, but not whether the entire WBE model is a misconceived project. Seen through the lens of the EBBSA, Humeans might view this as a debate over whether we have reason to think that energy minimisation is a biological metalaw.

Chapter 5 Conclusion

This thesis began with two main aims. The first was to investigate the status of a Humean approach to laws given the difficulties that have been raised for it. Some of these problems are often stressed by metaphysicians, such as the challenge of showing how mere patterns in the mosaic are capable of explaining that mosaic. Others are typically brought up by philosophers of science, such as the challenge of fitting initial conditions into the account. Due to the prevalence of these issues in the literature, I took my first task to be showing how Humeans can respond without losing sight of the spirit of the account. In doing so, I believed that the act of setting out the options would go some way towards accomplishing a second, broader aim. Metaphysics and the philosophy of science do not currently enjoy a peaceful coexistence. We do not have to look too far to find specialists in both camps walling themselves off from the supposedly irrelevant work of the other discipline. While this is sometimes evident in published work, it is even more common to find it in conversations and conference discussions. This state of affairs is distressing to those of us who see no sharp boundary between the subject areas but rather an area of promising overlap. By examining accounts of laws, a topic that I believe resides within this overlap, and drawing upon considerations from both sides, I have helped to demonstrate the benefits of working within this space.

In the first chapter, I examined the standard Best System Account. Of course, calling it 'standard' might be a bit of a misnomer, as we immediately run into questions of precisely what the theoretical standards for judging candidate systems are and how they should be understood. This remains an unresolved area, although plausibly one in which progress can be made by examining the practices of scientists. However, looking at some of those practices leads to a topic that the chapter did examine in more detail: the question of how we might integrate initial conditions into the account. While I noted that it is sometimes forgotten that David Lewis' presentation of the account does leave room for initial conditions in the best system, this does not do full justice to the way they are appealed to in physics. This provides the motivation to adopt Ned Hall's suggestion that we partition candidate systems into their Dynamic Hypothesis and their Initial Conditions Hypothesis. With this distinction in place, we can give a fuller account of how the theoretical virtues are applied in the competition for best system. Namely that systems are better when their DH is strong but their ICH is weak. I closed this chapter by showing why, by Humean lights, scientists ought to care about the resulting laws. I argued that science is not merely concerned with the accumulation of nonmodal information, but also with the construction of explanations.

The second chapter took up the issue of explanation in more depth. The long-standing question of how it is that Humean laws can explain the world has been a topic of recent interest, largely due to Marc Lange's focus on the principle of transitivity involved in the anti-Humean objections. After introducing Barry Loewer's distinction between metaphysical and scientific explanations, I showed how this merely sharpens the objection against Humeanism rather than defeating it. Ultimately, this leads to three routes of response open to Humeans. First, they might deny that regularities are grounded in the mosaic, and then either make the moderate claim that there are no relevant grounding relations or the more radical claim that the mosaic is grounded in the regularities. There cannot be a circle of explanation if the explanation involved is only one-way. This comes at a price, however, since a contrarian Humean is no longer entitled to say that every truth about the world holds in virtue of the mosaic. Second, Humeans can express dissatisfaction with the motivation supplied for the transitivity principle. The examples of it are reliant on scientifically studied entities explaining other scientifically studied entities. But Humean explanations involve a distinctively metaphysical entity, the mosaic, which is not clearly analogous to the motivating examples. Third, Humeans can bring into question the kind of explanation being appealed to here. If the demand is for something to be identified as responsible for the mosaic, then the laws will not suffice. But on the Humean view, nothing is responsible for the mosaic and so we should not be expecting laws to function as the explanans in that kind of explanation. I argued that Humeans might instead appeal to a kind of explanatory pluralism, where laws can be involved in explanations if by doing so they help to increase understanding.

The third chapter introduced symmetries and explained some aspects of their role in modern physics. Yet for all their importance, surprisingly little work has been done on the metaphysics of symmetry principles. Lange has been the main contributor to the project of conceiving of them as second-order laws that hold of other laws, and I drew upon his interpretation to extend the BSA to cover metalaws. This secures a philosophically promising place for them since the regularities of the second-order best system are suited to play roles similar to those of the first-order best system. Explanation and counterfactuals are particularly interesting here. The former because the approach to explanation suggested at the end of the second chapter allows symmetry principles to function in scientific explanations without there being a need to make questionable appeals to Noether's Theorem to justify their explanatory role. The latter because treating invariances more explicitly allows us to see the role they play both in ordinary counterfactuals and in counterlegals. In response to the charge that the close worlds we need to supply truth values for counterfactuals will either have different metalaws or multiple miracles, I have argued that this rests on a mistaken conception of the Humean world-view. According to modal realism, possible worlds are not human constructions and so there is no reason to think that close worlds will suffer from either of these flaws. Yet a problem for the extension of the BSA remained: symmetry principles do not look to be stated in terms of perfectly natural properties, and so, when translated into the canonical language, a system of the actual symmetries used by physicists looks unlikely to win the competition for best system.

While the third chapter ended on an obstacle, the fourth chapter began on a solution. Since the problem arises from the restricted nature of the vocabulary available to express candidate systems in, a suggestive line of response is to weaken that restriction. Here I appealed to the resources of the Better Best System Account and followed it in embracing a language-relative view of laws. With the problem of language avoided, this account was extended in the way previously indicated to arrive at what I take to be the strongest contender for a Humean account of symmetry principles. I then indicated two applications for this account. The first was to show why the structure imposed by symmetries does not represent a problematic constraint on the behaviour of particles. Far from merely banging the table and once more denying the existence of necessary connections, Humeans can give a positive account of what is involved in talk of multiplet structure. For the second use, I turned to biology as a representative member of the special sciences. Assuming that we are willing to grant that there are biological laws, the Extended Better Best System Account lets us say how these laws differ in their universality from physical laws: this drops out as a difference in the metalaws of the second-order best system for biology. It also provides a way to distinguish stronger principles like natural selection from the weaker laws that they apply to.

Where might we go from here? Given the central role that Humeanism plays in contemporary metaphysics, there are many related questions that this thesis has not resolved. However, I will highlight two main questions that I take to be particularly interesting prospects for future academic engagement with this field of research.

First, there is still work to do in giving a Humean presentation of biology and its laws. The regularities here, in addition to being contingent, are often thought to span a range of necessity. Some appear to hold for very brief lengths of time and across very restricted areas. These are easily disrupted and we might say that they are not as invariant as some of their more stable brethren. I have indicated one way in which biological regularities can be more or less necessary: by differing in whether they enter the first or second-order best system. The system of metalaws holds over a wider range of possible worlds and so its members can be intelligibly said to be more necessary. But this allows only for differences in necessity for entire systems, it does not single out members of one order of system as being more necessary than other members of that system. It would be interesting to develop a means for Humeans to do this. Additionally, when combined with the reduction of ‘universality’ into the holding of particular metalaws, I think that it would remove one of the primary obstacles to taking there to be any laws in biology at all. If laws do not need to hold unrestrictedly and can differ in how stable we take them to be, I see little reason not to say that the regularities of biology are not laws.

Second, the approach to symmetry principles developed here might be integrated with different forms of Humeanism. As I indicated at the outset, I have tended to appeal to the standard Humean project developed by Lewis when responding to objections. But the specifics of his treatment of modality, ontology and the roles of laws are not shared by all Humeans. We have already encountered one major deviation in chapter two, where we met the contrarian. Such a Humean might be willing to claim that the world supervenes on the mosaic, but is not willing to claim that it is grounded in that mosaic. One might push this further: if the core of Humeanism is a rejection of necessary connections, then one requires only that everything fundamental in the world be categorical. But this need not be partnered with any kind of mosaic metaphor – perhaps the introduction of the entanglement relations that Darby has examined is a first step on this road. Even more speculatively, the grounding of physical objects in their regularities suggested by the more radical contrarian carries broad similarities with the claims made by ontic structural realists: both advocate for a diminished role for objects at the fundamental level while favouring some wider structure. A world where objects are grounded in laws (understood as regularities), which are in turn grounded in symmetries (understood as higher-order regularities) might well turn out to be a structuralist world – albeit one where the structure involved is categorical rather than modal.

There is still much work to be done here in developing Humeanism of the style that I have adopted. But I hope that this thesis indicates the manner in which that work can be fruitfully approached: through sensitivity to the concerns of philosophers of science without losing out on metaphysical robustness.

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