BOOK SYMPOSIUM

The Dappled World: A Study of the Boundaries of Science

By NANCY CARTWRIGHT
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SUMMARY

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The Dappled World is not only, as the subtitle says, *A Study of the Boundaries of Science*; it is also a study of the boundaries of natural law. From my empiricist point of view the two are intimately connected. The best way to learn about laws of nature is by looking at the laws of our most successful and admired sciences. I focus on the exact sciences, in particular on physics and economics. I do not look at biology, geology, anthropology, sociology or any of the other natural or social sciences. Superficially it looks as if they should be more, rather than less, open to the kind of interpretation I give; still, they may teach different or additional lessons.

The central thesis is what Peter Lipton here calls "anomalous dappling". Laws of nature (if conceived of as necessary or counterfactual regularities between so-called 'occurrent properties') are limited in their range; in regions that seem to overlap, the separate laws may be helpful in calculating what happens, but the overall outcome may be highly context dependent or there may be no rules at all for composing the separate effects; and some situations may not be subject to law at all—what happens happens by hap. I give three major arguments in favour of dappling.

As with many philosophical views, a first step is to show that the view is possible. So one of my central aims is to show that dappling is consistent with our most impressive scientific successes and that it is consistent with realism—that is, it may be true even of the set of all nature's 'true' laws.[1]

1. This is not because I am a realist (or, for that matter, an anti-realist) but rather because the view seems easier to defend for many versions of anti-realism; and I do not think we can reject dappling just because we are realists.

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I do this by pointing out that the most we are entitled to assert in even our most exact and successful theories are regularity claims with a big *ceteris paribus* clause in front: 'So long as nothing happens that cannot be represented in the language of the theory, then . . . .' When we construct an experiment to test one of our favourite laws, we make every effort to control the situation so that we can model all the influences that occur and calculate their contributions; any influences that we do not know how to model, we work very hard to exclude. Only then do we demand that the behaviour predicted in the law should be the behaviour that occurs. Similarly when we use theory to build a piece of modern technology, such as a SQID or a microchip, to ensure that the apparatus behaves in the way expected, we screen out all influences that we cannot model in our theory.

This strategy is of course consistent with the assumption that what we cannot model can nevertheless in principle be modelled in the theory, or in some successor theory or in some grand composite theory that dictates how all our separate theories must fit together. But it is important to remember that a hypothesis can be consistent with the data without being the hypothesis best supported by the data. We have good empirical reasons, I suppose, for extending our inductions to the laws with the *ceteris paribus* clause included. We have subjected them to severe tests across a wide range of circumstances, particularly those where we think they are most likely to fail. But the empirical evidence does not take us beyond this, to the removal of the *ceteris paribus* clause about what we can and cannot model.

Is there further empirical evidence from elsewhere? Not that I can find. One thing I think we should definitely avoid is anecdotal judgements about the history of science. There have been some striking extensions of theories into domains that they never before covered; and there have been hosts and hosts of failures. Neither seems to me to be a good source for predicting further extensions of any particular theory, or of all theories for all time. For that we need to look at the specific details of the specific hypotheses proposed and estimate their promise by a detailed, context-dependent assessment of the empirical evidence.

There is one underlying assumption in this argument that I use throughout *The Dappled World*. Our startling success at precise prediction and technological control may give good reason to believe in the truth of our theories. But, then, what we are entitled to are just the law claims that are supported by those successes, not anything bolder or grander.

My second set of arguments claim that the very way theories in the exact sciences work when they produce the precise predictions we value so highly shows how the limits of their domain are drawn. I discuss both the case of physics and of economics in my reply here.

My third set of arguments looks at cases where we have great confidence that we understand and can rely on a particular regularity. In almost all the examples I have looked at, the models that provide this confidence have certain crucial similarities. They are all models of what I like to call a "nomological machine". These are models in which we have a fixed arrangement of parts each with a known capacity operating together in a way that generates

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regular behaviour, so long as nothing interferes (or 'so long as the machine is shielded'). One good example is the planetary system, which generates the regular motions of the planets. An ordinary battery is another.

So, when we have regularities, we very often understand just where they come from. They come from the repeated operation of a well shielded nomological machine. The shielding is important. Imperfectly shielded machines give rise to shaky regularities.

How, then, do we understand the operation of a nomological machine? Do we not employ laws of nature? Yes, I maintain. But not laws of nature in the sense of necessary or counterfactual regular associations between occurrent properties. Rather the more fundamental principles teach us about capacities; they tell us what capacities are associated with what properties in a reliable way (as I say, "by the nature of the property" or as others might say 'by law').

For many capacities (such as the capacity of a mass to attract other masses and of a negative charge to repel other negative charges) it looks from our scientific successes as if nature provides rules of composition—what will happen when both capacities operate together. But just as with our first-order principles, the rules of composition are empirically supported only in situations where nothing occurs that we do not know how to feed into the formula for composition—that is, only so long as nothing *interferes*. This is one of the reasons that capacity claims cannot be reduced to claims about sets of regularities using only the language of occurrent properties. Another is the open-ended nature of capacity claims that I discuss in the reply below.

There is an obvious response to my observation that our best evidence for both our first-order principles and our principles of composition supports only claims about what happens so long as all relevant factors can be correctly described within the theory: In the really true theory everything can be so described! It is only our attempts at theory that seem to leave space for anomalous dappling. My reply to this is along the lines of Hume's *Dialogues concerning Natural Religion*. If I had solid independent assurance of the universal governance of law, there are a lot of good reasons I could think up to account for why all our best science is patchy. But if I have to make my inferences from the world as I see it, the wiser bet is for the dappled world.

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LIMITED REALISM: CARTWRIGHT ON NATURES AND LAWS

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A leaf falls to the ground, wafting lazily on the afternoon breeze. Clouds move across the sky, and birds sing. Are these events governed by universal laws of nature, laws that apply everywhere without exception, subsuming events such as the falling of the leaf, the movement of the clouds and the singing of the birds? Are such laws part of a small set of fundamental laws, or descended from such a set, which govern everything there is in the world?

Yes, say most realist philosophers of science.

No, says Nancy Cartwright.[1] Laws of nature are not universal. Instead, reality is governed, when it is governed at all, by a patchwork of laws. "There is no universal cover of law" (p. 6).

The view strikes many as bizarre or incomprehensible. What does it mean to say that laws of nature are not universal, that they only apply to small patches of the world? Even more puzzling is Cartwright's view that some parts of the world are not subsumed by any laws at all (pp. 27–8, 31–2, for instance). How can such a thesis be acceptable to anyone but the more radical sort of anti-realist? Cartwright should not be labelled a radical anti-realist, but her answers to these questions depend on a rich, comprehensive view of how theories and models are used to characterise the world that will take some work to explicate.

The usual realist thesis about laws is that, whatever they are, they are exceptionless regularities that govern our universe by governing the interactions between all objects. Such laws might supervene on fundamental regularities that obtain necessarily, as opposed to accidentally, or they might be relations (of some special sort) between universals that lawful regularities instantiate.[2] The picture includes a measure of deductive elegance: there will be a small number of fundamental laws whose descriptions function like axioms, and a larger number of less fundamental laws whose descriptions follow from the axioms as theorems. Realists also hold that it is the principal task of the natural sciences (or perhaps the natural sciences along with other

1. All page references are to *The Dappled World* unless otherwise specified.
2. Defenders of such views include David Armstrong, *A World of States of Affairs* (Cambridge University Press, 1997); David Lewis, *Counterfactuals* (Harvard University Press, 1973); Fred Dretske (1977), 'Laws of Nature', *Philosophy of Science*, 44 (1977), pp. 248–68; Michael Tooley, *Causation: A Realist Approach* (Oxford University Press, 1987); and C.G. Hempel, *Aspects of Scientific Explanation* (The Free Press, 1965).

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empirical disciplines) to discover such laws, and the concept of law is strongly related to the concepts of causation, natural property, necessity and probability, amongst others.

By denying that laws govern universally, Cartwright denies a central tenet of the realist picture. In essence, what Cartwright is denying is that there exist a small set of fundamental laws under which all phenomena are subsumed (p. 23). How can this make sense? A law *just is* a thing that governs universally. It applies everywhere and always, even if there are no events or properties that instantiate it. So how can it make sense to think of only some parts of the world as law-governed while others are not? Our world doesn't *seem* to be random in some places and organised in others. By denying the universality of law, is Cartwright advocating the view that our theories about the world are merely reflections of our interests and limitations, fashioned so as to be consistent with the observable macro-phenomena we can measure in various ways? In other words, is she abandoning realism and advocating a strong version of anti-realism?

According to Cartwright, her view occupies a middle ground between realists and anti-realists (p. 47). Realists take their views to be supported by current scientific practice and success. Cartwright, in reply, argues that her views are better supported by current scientific practice than are the realist views that take laws to be universal (pp. 11–12). Because of her emphasis on the way science is really done, as opposed to an emphasis on logical consequence and the abstract structure of scientific theory, she even claims to shoulder the empiricist mantle.

So how are we to make sense of such a view if it is not merely a variation on familiar themes of anti-realism? What sense are we to make of a concept of law that does not involve the concept of governance, and how can this be consistent with contemporary scientific success, let alone contemporary scientific realism?

Many, including myself, have found Cartwright's writings on this topic difficult to understand. So what I'm going to try to do here is construct the metaphysical picture that seems to underlie Cartwright's account of the world, in the hopes of going some ways towards explaining her approach to those who find it difficult to make sense of. Let me be clear that I'm by no means sure that I've got her view right—in fact, I'm pretty sure that some of the details will be wrong, if only because sometimes Cartwright seems to say things that contradict bits of the picture I'm going to sketch. But I think that the account will be consistent with the spirit, if not the letter, of Cartwright's view, and it may help realists to see the importance and the interest of the program she is advocating.

As we shall see, although a kind of anti-realism comes into the picture, much of Cartwright's view is consistent with a robust metaphysical realism about necessity, causation and truth. Cartwright's view is not only original and exciting but parts of it—the parts that don't involve anti-realism—can provide the groundwork for a new metaphysics for a realist account of laws. Moreover, I think she can be located within a small but growing group of realists about the metaphysics of science who have recently argued for related

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or similar theses.[3] In any case, whether I've got the intended interpretation right or not, Cartwright's arguments can be used to suggest a quite metaphysically robust story about the world, one which is worth the telling—so I'm going to take advantage of the opportunity to do so.

The Picture

Cartwright recognises a role for our interests to play in constructing theories, and, most importantly, she gives pride of place to the fact that descriptions of laws of nature, taken as general claims about the world, are *ceteris paribus* claims. Start with the thesis that the law statements scientists take to describe the fundamental laws of nature are not true in every situation. Instead, at best, they are true *ceteris paribus*, and hence when taken to be universal claims they are strictly speaking false.

Now, if law statements are merely true *ceteris paribus*, but laws are exceptionless generalisations that govern reality, how can we take them to describe laws at all? Many realists address this problem by holding that the descriptions of laws that science gives us are at best *approximate characterisations* of what the real laws are. The laws themselves are universal, but we've only got the characterisation of them approximately right.

Cartwright rejects this realist position, arguing instead that some parts of the world are governed by laws, and others are not. But how can this be consistent with any brand of realism? Worse, how can it make sense? To see how Cartwright draws her conclusions about the nature and application of laws we need more information about her metaphysics. First of all, in Cartwright's picture, we reject the picture where we have objects plus something extra, the laws which govern the actions of the phenomena. Instead of thinking that we have the laws of nature plus the objects that they govern, we have objects in which certain causal powers or 'capacities' inhere, and the world consists of these objects arranged in different ways.

Cartwright takes objects to be collections of properties or "structures" (p. 81). I understand this to mean that objects are bundles of properties (collections of properties) or perhaps substrates with attributes attached (structures). Presumably, for an object to have a property is either for a property to be included in the bundle that is the object or for the property to be ascribable to the bundle that is the object, or for the property to be among the attributes attached to the substrate or to be ascribable to the substrate-attribute complex. The point here is that the conception of objects Cartwright endorses is *not* (neo)Aristotelian: objects are not substances identified by falling under sorts or by having certain forms.

3. For example, Alan Chalmers, *Science and Its Fabrication* (University of Minnesota Press, 1990); Brian David Ellis, *Scientific Essentialism* (Cambridge University Press, 2001); and C.B. Martin (1994), 'Dispositions and Conditionals', *Philosophical Quarterly*, 44 (1994), pp. 1–8. Alan Chalmers has argued for connections between Cartwright and Roy Bhaskar's work in 'Is Bhaskar's Realism Realistic?', *Radical Philosophy*, 49 (1988), pp. 18–23.

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Leaving the account of what a property is aside for now (are property instances tropes? or something else? Surely Cartwright would reject transcendent and immanent universals), properties had by objects include those such as charge, momentum and the like. Objects, by having certain properties, are able to behave in certain ways. A particle, in virtue of being excited to a higher energy state, can emit light, and in virtue of having a particular momentum, can impart a force on impact. Cartwright calls such abilities to behave *capacities*, and I think it is fair to identify them with causal powers. So an object has certain capacities in virtue of having certain properties. Cartwright holds that the capacities an object has are part of the object's *nature*.

Objects can interact in different ways; in so acting they express certain capacities to behave in such and such a way, given the circumstances they are in. So objects' capacities to behave are indexed to particular circumstances, since they will express different capacities depending on the interactions they have with other objects, i.e., depending on the circumstance they are in. When an object acts according to its nature it expresses a capacity in a situation (p. 72).

According to Cartwright, properties can also have capacities and natures (p. 82, for example). Moreover, properties are individuated by their capacities, so that what a property is, in an important sense, is defined by its capacities. Cartwright suggests that it may even be the case that a property is just a conglomeration of capacities (p. 70).[4] In light of this, I'm not sure whether objects are collections or structures of properties, collections or structures of capacities, or collections or structures of properties and capacities.

I think capacities for Cartwright are taken as primitive entities, and properties are also taken as primitive entities—perhaps, as she suggests (p. 70), properties are just "conglomerates" of capacities. But now a puzzle arises: if objects are just bundles of properties and properties are conglomerates of capacities then objects themselves are just bundles or 'conglomerates' of capacities.

I find it hard to think of objects as just bundles of capacities or causal powers—objects seem to be, speaking metaphorically, more substantial than this. There are alternative theories of objects, but I'm not sure which one Cartwright would prefer. Perhaps she would prefer to hold that objects are substrate-attribute complexes, and so an object is a substrate with a bunch of powers. For those who find substrates metaphysically acceptable, this might make the best sense of the way objects include their causal powers. If we took properties as primitive but distinct from capacities, we could hold that objects are bundles of some properties that somehow endow objects with powers, but then it would be nice to know why the having of certain properties implies the having of certain capacities. (Another necessary connection we take as primitive?) Talk of natures, while evocative, is no help here: according to

4. I find some of Cartwright's remarks on essentialism puzzling. On pp. 82–3 she argues that the "nature of charge" as expressed in certain interactions does not reveal the essence of charge. Yet earlier (p. 70) she argues that "what a property empowers an object to do is part of what it is to be that property". This sounds like essentialism to me, so I'm not sure what Cartwright is rejecting in the later passage.

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Cartwright, natures are also just conglomerates of capacities that (somehow) allow objects to express different capacities in different settings in virtue of the properties the objects have.

In any case, it should be clear that there are some robust metaphysical assumptions here about objects, properties and powers that I think Cartwright should flesh out. The view has many virtues, but until the details are worked out it is hard to assess the overall attractiveness of the thesis. Metaphysicians interested in a realist account of laws will have many questions about this ontology, especially about the nature of the capacities or causal powers that are being relied on so heavily.[5]

Leaving worries about defining objects, properties, natures and capacities aside, we now have the basic machinery needed to construct Cartwright's picture of the world. We build from the bottom up, so that when we have a world of objects, we have a world that includes the powers that inhere in those objects. For Cartwright, a description of a law is a description of the capacities objects express in particular situations. Once we have the world of objects we have all their capacities too, expressed in various ways according to their natures. This means that we already have everything that we need to have laws—we don't need to impose anything extra—on top of the objects.

Thinking of laws as about objects' natures or causal powers gives us a nice way to capture the sense of necessity we need when making nomic claims. Since we build the metaphysics in from the start, we don't need to impose any extra ontological layers on top of the particulars in the world in order to get an explanation of why objects behave the way they do. We don't have to go as far as stipulating that there exist universals and nomic connections between universals, yet we have a—much needed—stronger kind of necessity than traditional regularity theories, where laws are just (selected) regularities of events, can bestow.

According to Cartwright, when we consider a particular experimental set-up, what we are considering is a collection of objects that are behaving in accordance with their natures in virtue of expressing the capacities they have relative to the particular circumstances of that experiment. When we repeat experiments in order to confirm results, we are conducting experiments in order to confirm the claim we want to make about the natures of the objects, or, more precisely, the claim we want to make about the capacities the objects express in that set-up. To ensure repeatability the environment of the experiment must be constructed in just the right way, i.e., so as to exclude factors that could cause some other interaction and hence interfere with the production of the result. This makes sense in terms of natures if we say that the objective is to confirm that an object has a particular capacity: if we change too much about the experiment the object(s) may behave differently—there may be interactions with other objects that cause a different aspect of the

5. What is the structure of a causal power? By taking powers as primitive, isn't Cartwright just shifting much of the ontological structure the defender of universals uses to characterise laws from outside the object to inside the object, and then refusing to explicate the ontological structure by making it primitive? Worse, the move seems to add more problems than it helps: for example, laws become necessary (see the discussion below).

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object's nature to be expressed—and we cannot then confirm the original hypothesis.

So for Cartwright, when laws apply they cover relations between capacities, what experiments measure is the expression of capacities, and what scientists want to know is which capacities objects will express in which circumstances. Now return to the questions about the application of laws discussed at the start. Cartwright claims that scientific practice and success suggest that laws are piecemeal and that governance, if it occurs at all, is patchy. How can her views about natures help with this?

Recall that Cartwright defines what there is in the universe from the bottom up. Thus far, what we have are a lot of objects behaving in certain well-specified ways, in other words, behaving strictly in accordance with their natures. Objects can behave different ways in different circumstances, but if we have correctly identified a capacity of an object, i.e., some fact about its nature, its behaviour will be *repeatable*. In the very same circumstances, the object will express the very same capacity. Now reconsider the usual realist notion of law: it has two parts, the part about governing the universe and the part about being an exceptionless regularity that is necessarily true.

To understand Cartwright's position on laws we need to remember that Cartwright thinks the lawlike claims of science are only true *ceteris paribus* and that she is reading the results of scientific practice literally: she takes scientific experiments that give results which fail to conform to the letter of the very general lawlike characterisations of science to be giving us evidence about laws *themselves* (p. 9). If an experiment gives us a repeated regularity, it gives us evidence of a law. If a somewhat different experiment gives us a somewhat different regularity, it gives us a *different* law, even if the laws are similar and we could come up with some sort of lawlike generalisation that approximately describes both. On this approach, laws really are exceptionless generalisations, but the lawlike claims of science aren't describing these laws. Instead, the lawlike claims of science should be seen as rough generalisations that are, strictly speaking, false in most situations. (They might be strictly true when relativised to certain well-defined contexts.)

If we accept this view, then how should we think of laws applying to the world? One thing we could think is that there are many different, albeit roughly similar, laws governing different parts of the world, and the law statements we use are generalisations capturing only some of the content of these many similar laws. Another thing we could think is that some parts of the world are governed by laws, and others are not. Yet another interpretation is that laws don't really govern anything at all—this seems to collapse into the claim that there are no laws, merely false generalisations that we must use in order to make sense of a disorderly world.

Cartwright does not see how we can infer that we have universally true laws given that descriptions of laws are only true *ceteris paribus*. But she does see how laws could be regularities that involve necessitation of some sort, i.e., regularities involving the expression of capacities. So what she does, in effect, is restrict the notion of law to regularities involving (causal) necessity while jettisoning the part about governance (p. 37). "It is a different question to ask,

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'Do Newton's laws govern all of matter?' from 'Are Newton's laws true?' " (p. 48). So laws are *exceptionless regularities* of a certain type, regularities of objects expressing capacities. If we accept this restricted notion of law, then we can see how we can have many different laws for many different parts of the universe. We can have a law that obtains for each situation where we have repeated expression of capacities and precise results. If the expression of capacities is even a little bit different, the law that obtains is just that much different.

A situation that generates a repeated series of events in a well-defined region of space-time is part of what Cartwright calls a *nomological machine*. A nomological machine is either an experimental set-up designed to identify and isolate a capacity or set of capacities, or a fortuitous event of nature (such as the solar system). Cartwright emphasises that for a situation to be a nomological machine, it must be an environment that is shielded or not affected by outside disturbances or influences so the events that occur in it are controlled or regular. The controlled environment allows the capacities of the objects to be expressed in a stable (i.e., repeatable) way. If the environment is not shielded there can be interference that prevents repeatability (pp. 50, 87–90).

In this picture, laws are just regularities of the expression of capacities of objects: laws are regularities generated by the capacities that different objects express when combined in certain ways (p. 49). We can now see how to make sense of the idea that the universe could be characterised in terms of a patchwork of laws. What we should do when we make claims about laws is make strictly true claims about the relations between capacities of objects that arise given particular circumstances. In this sense, a few fundamental laws do not govern the whole; rather, a bunch of different laws apply to a bunch of different parts of the whole, since the capacities expressed by objects changes depending upon the details of the situation. This gives us enough to understand the view that "we have a patchwork of laws".

But what about Cartwright's further claim that there might be parts of the world where laws don't apply? Cartwright doesn't just opt for the view that different laws apply to different parts of the world—she thinks there are some parts where no law at all applies. "Laws need nomological machines to generate them, and hold only on condition that the machines run properly" (p. 59). Does this mean that there are situations in which things happen but capacities are not expressed? There are many naturally occurring situations that would not count as nomological machines because of the lack of shielding and general chaotic nature of the events involved. According to Cartwright, laws don't apply to these situations. But this seems wrong—if a leaf falls from a tree or clouds move across the sky, surely capacities are being expressed. But if capacities are expressed then why doesn't a law apply?

I've argued above that once we see how the metaphysics of law is supposed to work for Cartwright the idea of a patchwork of laws is understandable and interesting. But nothing I've said thus far will help the realist understand the further claim that there are patches of the world where laws do not apply. To make sense of this further suggestion, perhaps we can interpret Cartwright's position like this: since laws are regularities, we need a situation in which a

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capacity is expressed by an object *repeatedly* in order for there to be a law about it. In other words, to have a law, we need a regularity to obtain. Not all situations are those to which laws apply, because a situation to which a law applies *must* involve a controlled series of repeated events. But many would find this unconvincing: why aren't capacities expressed all the time, even in unrepeatable instances? And if capacities are expressed all the time, why aren't there laws that apply to them, whether or not the expression of the capacity is repeated? The problem is one faced by regularity theorists in general: requiring that there is an actual regularity of some sort seems to be too strong a constraint on laws.

Perhaps Cartwright simply bites the bullet: we need a regularity to give rise to a law, because what laws *are* are claims about capacities or relations between capacities that *are* repeatedly expressed. In unrepeated, uncontrolled instances, there is no repeated series of events, hence no regularity, and so no law, even if some capacities are expressed. We can distinguish between patches of the world that are governed by law and patches that are ungoverned by law by pointing to differences in structure: patches governed by laws must involve repetition.

But Cartwright doesn't bite this bullet.[6] Instead, she devotes a reasonable amount of space to exploring how nomological machines that generate some events that are never actually repeated can still give rise to laws (pp. 87–90). What is required to have a nomological machine is not that a series of repeated events is actually instantiated, but rather that the events that are the expressions of capacities are *in principle* repeatable in a particular way. "Most situations do not give rise to regular behavior. But we can make ones that do. To do so, we deploy facts about the stable natures of the processes we manipulate and about the circumstances that will allow these natures either to act unimpeded or to suffer only impediments that can have a stable and predictable effect. When we have such a situation, we are entitled to generalize from even a single case" (p. 89). But what counts as in principle repeatable? Surely *any* event or expression of capacities that occurs anywhere in the world is *in principle* repeatable, no matter how complex, and no matter how unlikely it would be that a repetition would actually occur. Cartwright's requirement seems to bring in an objectionable kind of constructivism: in cases where there is no natural repetition, what makes something a nomological machine, hence a situation where a law applies, is whether it is controlled by *us*, whether it operates in an environment shielded *by us* (p. 73).

I think that here is the place where Cartwright departs from the ranks of the realists and moves some way towards anti-realism. My interpretation of what is going here is that by 'law' Cartwright means to pick out something that in many cases is at least partly constructed by humans. Laws are things that apply to certain types of situations, situations where we actually have

6. Or perhaps she does, since she says, "[I]t is capacities that are basic, and laws of nature obtain—to the extent that they do obtain—on account of the capacities; or more explicitly, on account of the repeated operation of a system of components with stable capacities in particularly fortunate circumstances" (p. 49). There are similar quotes scattered throughout the book, e.g., on page 4 and on page 49.

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repetition, or ones for which in principle repeatability of a certain sort is possible. In-principle repeatability is (usually) human-driven: because the situation is appropriately shielded and controlled, it can be reconstructed so as to generate a repetition of the expression of capacities even if we never actually do reconstruct it because the experiment is too time-consuming or expensive, etc. When we set up a nomological machine, we recognise that it is the kind of thing that in principle allows repeatability, and so we can use it to infer that a law 'covers' the situation of the machine. In this sense, then, we generate laws by constructing nomological machines, since such machine allow us to make true claims about the capacities expressed there. "Our most wide-ranging scientific knowledge . . . allows us to build new nomological machines never before seen giving rise to new laws never before dreamt of" (p. 4).

This gives us a human element in the natural and social sciences, an element reflected in what we designate as laws. But it is not entirely incompatible with realism, for laws in this sense aren't all that we have: we also have capacities or natures, and these are robustly ontological, i.e., they exist independently of anything pragmatic. Moreover, these natures are what give rise to what we characterise by our laws, and so the world has an independent character after all. Although situations to which laws apply can be constructed by us, not just anything goes, for what can be covered by a law is constrained by the natures of objects, and even if a law fails to apply in a situation, it doesn't mean that the interactions of the objects are random or arbitrary. If the way I've interpreted her view is correct, then Cartwright is indeed occupying a middle ground between realism and (at least a weak version of) anti-realism, which is where she explicitly places herself (p. 47).

If realists are prepared to argue that in-principle repeatability can be defined in some way that is independent of human interests, they don't need to follow Cartwright towards the anti-realist camp. If expressing a capacity, even once, is sufficient for a law to apply, then they can agree with Cartwright that the world is governed by a patchwork of laws but reject the idea that any part of the world is ungoverned: all the world is law-governed, even if no law governs all the world. Only when one goes further and adds pragmatic constraints restricting the situations to which laws apply is it the case that some expressions of capacities may well not count as laws, even if all the metaphysics we could need or want is there. So strong realists may be able to make good use of much of Cartwright's program.

Now that we have a characterisation of Cartwright's view of laws, we can see why one potential problem for her might not be such a problem after all. Cartwright's account of the capacities had by properties of objects is strongly reminiscent of Sydney Shoemaker's thesis of properties as causal powers. (Cartwright notes the similarity (p. 70).) But if Cartwright individuates properties by their capacities or powers, then, like Shoemaker (and like some types of regularity theorists), it would seem that she faces problems involving the modal status of laws. We tend to think that the laws of nature are contingent, such that there could be worlds with different laws but where the same properties or collections of properties occur.

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But if the natures are determined by properties, then in worlds with the same properties we will have the same natures. And if natures are what ground laws, then in worlds with the same natures we will have the same laws. This makes the laws of nature necessary (given the same properties) rather than contingent. Cartwright seems to confirm this when she says "So, 'How does the Hume world differ from ours?' It would not differ. Any world with the same properties as ours would *ipso facto* have capacities in it, since what a property empowers an object to do is part of what it is to be that property" (p. 70).

Now, some have embraced the necessity of laws as it follows from the causal powers view, and there are different ways to make it reasonably palatable. But as the result of her sympathy with anti-realist accounts of laws, Cartwright may be able to soften the blow. She'd have to grant that in a world where the distribution of properties across the spatiotemporal manifold was exactly the same, the laws would be the same. But worlds only a little farther away, i.e., worlds that are very similar to ours, could have different laws because they could include different nomological machines, even if the capacities of the objects in those worlds were the same. I'm not sure that this way of handling our intuitions about the contingency of laws would be satisfactory to those who have them, but it's certainly a response that Cartwright is justified in giving, and it gives her some flexibility to address a worrying problem.

There are many other interesting aspects of Cartwright's view that I have not been able to address. *The Dappled World* contains discussions of the epistemology of laws for natural science as well as for social sciences like economics and sociology, addresses problems with causal modelling using the ideas of capacities and nomological machines, gives an account of how models are interpreted so as to apply to the world, and much more. It also ties together themes of Cartwright's previously published work and places her overall theory in the context of contemporary scientific and social scientific practice.

Over the past twenty years, Cartwright has been developing and refining an exciting and deep picture of the way we should understand the world through the lens of science and social science, and her work has had a significant impact on theoretical discussions in philosophy, economics and sociology. Metaphysicians and philosophers of science should read this book both for the in-depth and interesting accounts of scientific and social scientific problems as well as for the new approach she takes towards laws, causation and the nature of world.

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THE REACH OF THE LAW

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From Metaphysics to Method

The stimulating programme of *The Dappled World* is metaphysics in the service of methodology. To say that the world is dappled is to say that the laws of nature only apply to certain regions. A central argument for this claim is epistemic. Although the laws, especially laws of physics, are typically thought of as universal, in fact we have only managed to construct precise quantitative models for a very limited range of cases, most of which lie within the artificially simplified environment of the laboratory. We lack models for many real-word situations not because we haven't tried to build them, but because we have tried and failed. This failure is compatible with the existence of a complete set of physical laws, perhaps never to be known, which governs all regions; but the evidence of our history of failures points the other way, to a dappled world.

Nancy Cartwright draws two methodological morals from the inherent patchiness of the nomological. Firstly, if we want to intervene in the world, one good strategy is to construct situations where the laws do apply. Secondly, if we want to understand what is going on outside those simple situations, we should use autonomous methods, rather than attempting to extend nomological science beyond its remit. In these comments I consider three big questions about this programme. What exactly would it be for the world to be dappled? How strong is the case for dappling? How tight is the connection between dappling and the methodological morals drawn from it? Covering so much ground in such a short compass runs the risk of misconstruing Nancy Cartwright's project or her arguments; fortunately this symposium gives her the opportunity to set me right.

What is Dappling?

As a first approximation, I have said that for the world to be dappled would be for only some regions of the world to be nomological or law-governed. The motion of the planets might be nomological, the motion of a fluttering thousand dollar bill in St. Stephen's Square might not be. How dramatic this claim is depends on what one means by a law. Let us distinguish two broad possibilities. The first is Humean: a law is an exceptionless, projectible regularity among occurrent and, some would add, observable properties. The non-Humean options are various, but I will focus on the idea that a law is a stable relationship between capacities.

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Humean dappling is very plausible and, I think, very widely accepted. The world is mostly a messy place where many forces and other capacities are simultaneously in play, and much if not all of what happens is governed by unobservable entities and processes. Exceptionless regularities among occurrent properties, and certainly among occurrent and observable properties, are the exception, not the rule. This claim is difficult to resist without maintaining heroically that these superficial properties are all the properties there are.

To deny Humean dappling would be to assert that the superficial properties the Humean is willing to cite form a closed system, so that whenever one set of these properties is instantiated, so is another similarly superficial set. But we have no reason to believe that this is generally so, to believe that what follows from the antecedent set is completely independent of all the other, deeper property instantiations. To suppose that the superficial properties form a closed system is like supposing that if the blue light follows the green light on a black box once, then this will happen every time. We have no reason to suppose this, because there may be many different inner configurations that yield a green light, for only some of which a blue light will follow. Perhaps we will be lucky enough to find some especially simple boxes, for which there are exceptionless superficial generalisations, but such generalisations will not be applicable to all boxes. This is the very plausible picture of Humean dappling. It seems to me true, but also weak, because it is compatible with the existence of a more fundamental physics of deep properties that governs all the superficial properties, indeed all properties whatever.

The situation becomes more complicated and interesting when we consider non-Humean dappling. Our first approximation then becomes the claim that there are anomalous regions, even when laws are construed as stable relations between capacities, or between capacities and any other properties. This is bolder than the claim of Humean dappling, since capacity laws may hold even where there is no exceptionless regularity between occurrent or observable properties. Thus many who gladly admit that there is no Humean regularity for the fluttering bill would insist that a gravitational law nevertheless covers this case, since a stable relation between masses and gravitational forces still applies to the bill, though other forces are also in play.

How shall we now interpret the more radical dappling claim, in the context of capacity laws? Not, I take it, as the denial of capacities outside the privileged regions. Such a claim seems plainly false, as gravity, for example is an unshieldable long-range force. Whether or not all of the world is governed by laws, the dappler and her opponent the fundamentalist seem to agree that it is shot through with capacities. Indeed it would seem that dappler and fundamentalist should also agree that the laws of physics say something about complex systems, given the extent to which engineers use physics to build and control extremely complicated systems.

There are, however, other more interesting interpretations of non-Humean dappling, of which I will mention two. 'Pluralist dappling' is not the claim that there are anomalous regions (when laws are construed as relations between capacities), but rather that different regions are subject to different laws, because different regions are subject to different combinations of capacities.

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'Anomalous dappling' is the view that there really are anomalous regions, because although capacities are everywhere, they do not always combine in lawlike ways.

Fundamentalism entails that everything that happens is nomologically governed, so anomalous dappling is clearly incompatible with fundamentalism. But what about pluralist dappling? Here we may need to distinguish two versions of fundamentalism. One is simply the view that everything that happens is governed by law. This is the completeness of the nomological, but it places no constraint on how many laws there are or over the range that each covers. A second version of fundamentalism is that everything is governed by the laws of physics, where these laws are understood to have universal scope, so that the same laws apply throughout. Pluralist dappling is compatible with the first version of fundamentalism but not the second. I shall take it that we want a conflict between dappling and fundamentalism, so opting for pluralist dappling would force us to take the imperialism-of-physics version of fundamentalism.

It seems to me, however, that this is the less attractive reading of fundamentalism, because it is hostage to murky questions about just where the boundary of physics is to be drawn and just what it means for a law to be universal. For to say that the laws of physics are universal is not to say that each of them is everywhere instantiated. The issue here is unclear, in somewhat the same way as the mind-body problem. Dualists claim that there are non-physical properties, but once it is made clear that what determines whether a property is physical cannot be the physics we happen now to believe but rather the complete and perhaps never to be known physics, it becomes unclear what would make a property in the relevant sense non-physical. Similarly, the pluralist dappler claims that there is behaviour that does not supervene on the laws of physics but, insofar as that behaviour is conceded to be nevertheless law governed, it is unclear on what grounds we would deny that the relevant law is part of the ultimate physics. So I prefer to focus on the ambitious claim of anomalous dappling, which does make for a clear foil to the cleaner version of fundamentalism, the version that maintains just that everything is governed by laws of nature, whether these are laws of 'physics' or not.

Anomalous dappling is not simply the view that although we have laws that apply to certain simple regions, we do not now have nomological models to cover the rest. This epistemological point, though the basis for a central argument for anomalous dappling, should not be confused with the metaphysical claim. Nor is anomalous dappling simply the view that our current models, and what we currently take to be laws, cannot be extrapolated to cover all regions. That is a consequence of anomalous dappling but it is a weaker claim, since it is compatible with the universal reign of law, just not of laws as we know them now. Anomalous dappling is the claim that there are regions or situations, such as that of the thousand dollar bill, where not all behaviour is nomological, however deep and capacity-laden one's conception of law. This is strong stuff, distasteful to those who think there can be no capacity, cause or even physical property or object without associated laws, but it is a claim that, as Cartwright shows, can backed by arguments.

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Why Believe in Anomalous Dappling?

There seem to be two main types of argument for anomalous dappling, one metaphysical, the other epistemic. A metaphysical argument for anomalous dappling appeals to the nature of capacities and of the ways they interact, making out the case that some capacities are not apt for nomological relations or that even capacities apt for such relations in certain simple situations fail to enter into laws in more complicated environments. Thus it is claimed that what happens in situations where two or more forces are in play need not be the 'sum' of what each force would do alone, with the consequence that even if there are laws for each force acting in isolation, we cannot combine these to have a law for the forces acting in concert. This is what examples such as that of the thousand dollar bill are meant to suggest.

Certainly forces and capacities may interact in complicated ways that are not in any intuitive sense 'additive'. This is easiest to see in cases where one capacity changes another: elastic bands become brittle, food becomes inedible and drugs lose their potency. The simple picture of a composition of forces as vector addition is inapplicable to most interactions between capacities. But the complexity of capacity interaction does not appear strongly to support anomalous dappling, for at least two reasons. The first is that this complexity does not discriminate anomalous dappling from pluralist dappling. Non-additivity may support the idea that laws vary by region, but it does not, I think, suggest that regions where non-additive forces are in play are therefore anomalous. A fundamentalist must admit that capacities change, but he may maintain that this is itself a lawful process. Just as a breakdown of occurrent regularities should not lead us to deny lawfulness—we move to capacities—so a breakdown of capacities should not lead us to deny lawfulness —we move to lawful change in capacities, or second-order capacities.

A second reason why it does not seem that the complexity of capacity interaction provides a good argument for anomalous dappling is that it would prove too much. The realms of law are supposed to be those of the lab or perhaps deep space, where situations are simple enough for laws to hold. The trouble is that shielding is only a matter of degree. We can create or occasionally find regions where the forces other than the ones we want are minimal, but we never completely eliminate gravity, dust, friction, interference, noise. So, from a metaphysical point of view, if a multiplicity of forces and the complexity this involves meant that we have no laws, then the conclusion of the argument from complexity threatens to deliver not anomalous dappling, where laws hold in some regions but not in others, but nomological chaos, a completely anomalous world.

I turn now to an epistemic argument for anomalous dappling. We have good theoretical models for certain properties in certain regions. The success of these models provides the best reason one could have for believing that the laws they cite govern the regions to which the models apply. Conversely, where we have no successful models, we have no reason to believe in nomological control. This perhaps only supports what we might call 'agnostic dappling', according to which we do not know whether laws rule the unmodelled

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regions; but even this more modest position might be strong enough to support the methodological morals that Cartwright wishes to draw. In any event, the epistemic argument can be strengthened by adding the observation that in many cases the absence of models remains even after concerted scientific effort. Here perhaps we have some reason to believe the full claim of anomalous dappling, by a kind of inference to the best explanation, where the best explanation for our failure to find laws in a certain region is that there aren't any there.

The crucial question then is whether the best explanation for our failure to find successful models for a given region is metaphysical or just epistemic. Is the best explanation of our failure the absence of laws or just that we are too dim to find them? I find this a difficult question to answer. Perhaps scientists and those who empathise with them tend to adopt fundamentalism as kind of regulative principle, acting as if the laws can be found if only we are sufficiently clever. And even a dappler can I think endorse this stance, since it need be no part of her claim that we have already discovered all the laws there are. The presumption of lawfulness may be the best way to uncover the remainder. But this pragmatic justification for a fundamentalist stance gives no reason to believe that fundamentalism is true, and so no reason to believe that anomalous dappling is false. Indeed it may suggest that we have a tendency systematically to overrate the probability of fundamentalism, as we tend often systematically to turn hopes into beliefs.

Perhaps the reason that the epistemic argument for dappling is difficult to assess is that the inductive evidence for fundamentalism provided by our modelling successes and the evidence against fundamentalism provided by our failures are both weak. (It would be instructive to compare these two arguments to the familiar miracle argument from success to realism and the pessimistic induction from failure to anti-realism.) Taken alone, the fact that we have good models in certain regions hardly compels the conclusion that such models exist for all regions; but the failure to find models seems similarly inconclusive, since the fault is as plausibly explained by our cognitive weaknesses as by an anomalous world. Fundamentalists seem to face no particular difficulty in accounting for scientific failures: the Lord may be very subtle without being nomologically malicious.

One familiar feature of good inductive arguments is counterfactual tracking. Thus, my inference that p from the fact that this is what I am told by a reliable instrument or a reliable informant is inductively strong because, supposing that p is true, had p not been the case, the instrument or informant would not have said p. When you tell me that you have a headache I believe you, because I am confident that you would not have said you have a headache if you didn't have one. Suppose now that the world is in fact dappled, with the evidence of successes and failures in modelling being as we actually find them. The question then is whether that evidence would have been different, had there been no anomalous regions. Would we have had more success in modelling, if the world had not been dappled? I have no idea. Maybe we would have, because we would have managed to model some of the regions that are actually anomalous. But maybe we would not have,

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because those regions, even if nomologically governed, would have been too complex for us effectively to model. Insofar as one cannot choose between these two possibilities, one must I think judge that the argument from failure to dappling is weak.

Would an inductive argument in the opposite direction—from successes to fundamentalism—do any better on this counterfactual criterion? Supposing that fundamentalism is true, would we have had less success in our modelling efforts had the world been dappled? Would any of the regions for which we now have good models have resisted modelling? This too is very difficult to assess: maybe yes, but maybe these are just the regions that would have remained neat and nomological even in a dappled world. The moral I draw is that neither scientific successes nor scientific failures bear effectively on the deep metaphysical question of anomalous dappling. If the question can be answered at all, this can only be done on a metaphysical basis and with a detailed examination of how we should understand what it is to be a law of nature, an examination that I think would have to go beyond the discussions we currently have available to us.

Does the Metaphysics Support the Methodology?

My final question concerns the connection between anomalous dappling and the two methodological injunctions I mentioned at the start. One is to attempt the construction of situations that are law-governed, that is of nomological machines, especially where we have practical need for effective control over the course of nature. The other is to develop autonomous methods of understanding for situations outwith those machines. Presumably those methods will largely be the methods of the special sciences, where the generalisations of those sciences do not qualify as laws of nature and are not reducible, even in principle, to the laws of physics. Let's call this pair of injunctions the methodology of Construction and Autonomy (C&A, for short).

If I understand her correctly, one of Cartwright's main motivations for developing the case for dappling is the support that it would provide for C&A. The thought is that if we accept a metaphysics in which nomological models can have only a limited scope, then we will also accept C&A. After all, if models are scarce, then you want to create situations where the models apply and you don't want to use the method of modelling where it won't work.

The methodology of C&A is attractive, but it is not clear how much it has to do with dappling. Certainly a fundamentalist may be an enthusiastic proponent of C&A. As we have seen, he may agree with the dappler that we shall never manage physical models for all regions, differing only about the reason for that failure. And the fundamentalist may also agree with the dappler that, given that limit on what we will achieve in physics, we are well advised to adopt the policy of C&A. Indeed, even a fundamentalist so epistemically sanguine as to believe that models for all regions will eventually come our way may embrace C&A, both because 'eventually' may be a long time coming and because the special sciences may provide special benefits in both

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understanding and prediction even for regions where models from physics are already available. Someone who thinks that physics should replace psychology is not a fundamentalist: he is a crazy fundamentalist.

So fundamentalists can endorse C&A. Conversely, a dappler may hesitate fully to endorse C&A. Even if the laws of physics are limited in their scope, it is unlikely that they are limited to the boundary of our current scientific achievements, especially in light of an induction on the increasing reach of the physical sciences over their history. It is agreed on all sides that physical models are very desirable where they are possible, so one may hold that we ought to be devoting substantial resources to seeing just how far physics can stretch, even if we are certain that it cannot cover everything. And even with dappling there remains a great deal of scope for enquiry by fundamental physics about the world entire, such as physics of long-range forces, of conservation laws and of the constitution of matter.

There may be a further internal difficulty facing an argument from dappling to the injunction to construct nomological machines. The motive for this construction is a gain in control. Some control must be possible outside the machines, not least because otherwise the advice to construct machines would be useless, since the act of construction requires control outside a machine. The thought is just that control is better within a machine, because it is more efficient. But a fair comparison of the choice between trying to control a phenomenon within a machine or 'in the wild' requires that we factor in the cost of building the machine. Since either way we must exert control outside a machine, the argument from dappling will not in itself tell us which way is more efficient.

For various reasons then, it seems that even an irresistible case for anomalous dappling would not provide a particularly powerful argument for C&A. What it would do is to deprive the fundamentalist camp of one argument, the argument that physics deserves disproportionate funding because it will provide the Theory of Everything. But I suppose that the fundamentalist has better arguments than this in any case, and that the weaknesses of the argument from a Theory of Everything can be exposed more easily than through the deep and difficult metaphysics of dappling. But those metaphysical depths fascinate some of us quite independently of any methodological morals, and we are in Nancy Cartwright's debt for encouraging us to dive deeper and for prodding us to face up to the possibility that the universal reach of physics might be little more than a prejudice.[1]

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CAPACITIES, NATURES AND PLURALISM: A NEW METAPHYSICS FOR SCIENCE?

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Nancy Cartwright's new book is a thought-provoking exploration of the role of laws and models in the sciences, with special emphasis on physics and economics. Cartwright proposes a novel metaphysics for science that repudiates the fundamentalist view that the world conforms to a single set of simple and elegant laws in favour of the view that the world is a dappled array of phenomena—sometimes ordered and often times not—that conform, at best, to an untidy patchwork of laws. The book also contains a sustained critique of empiricist views that assign a primary role to regularities and occurrent properties in their interpretation of scientific activity. In opposition to these views, the book advocates an Aristotelian metaphysics that assigns primacy to the capacities or causal powers enjoyed by objects and properties in virtue of their natures.

Many of the nine chapters of the book originate from already published articles, though they have been considerably revised and enlarged. They rework a number of themes from her earlier books *How the Laws of Physics Lie* (1983) and *Nature's Capacities and Their Measurement* (1989), but also introduce some new ones. The book is an enormously stimulating and rewarding read. Quite a few times I paused to reconsider some familiar philosophical problem after seeing it from a new angle. While it is not always a straightforward matter to understand all the details of her argument, it is easy enough to get the general gist of what she is saying. The process of comprehension is made the more enjoyable by some delightful illustrations by Rachel Hacking and examples by Towfic Shomar.

In this discussion I shall explore the cogency of Cartwright's arguments for her new metaphysical framework. In particular, I shall examine her attempt to reorient the metaphysics of science away from Humean regularities and occurrent properties to Aristotelian capacities and natures; and also her attempt to advance a new pluralist conception of laws in place of the orthodox fundamentalist one. While I am sympathetic to her general metaphysical position, I see gaps and unclarities at important places in her arguments.

Humean Regularities versus Nomological Machines

One of the recurring themes of the book is Cartwright's critique of the standard empiricist construal of laws of nature as exceptionless regular associations between occurrent, measurable or observable properties. She argues persuasively that the empiricist repudiation of capacities and powers in favour of

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occurrent properties has its origins in the copy theory of ideas and the associationist theory of concept formation (pp. 68–70). In view of the widespread rejection of these theories, there is no reason for continuing the empiricists' ban on capacities and powers. In any case, there are very few exceptionless regularities among occurrent properties of any theoretical interest. Most laws known in science are *ceteris paribus* laws that state how certain kinds of system behave provided nothing interferes. Naturally occurring phenomena are the outcome of many causal influences operating at the same time. One of her favourite examples is that of the motion of a pair of charged, massive particles. Coulomb's law tells us how force due to charge influences the motion of the particles. The law of gravitation tells us how the force due to gravity influences the motion of the particles. But the actual motion of the particles does not conform to either law (pp. 53–56). Consequently, laws of this kind cannot be construed as regularities in occurrent properties because the laws do not concern what actually and observably happens.

In her alternative metaphysical framework, Cartwright relegates regularities in occurrent properties to a secondary role, giving pride of place to what she calls 'nomological machines'. The concept of a nomological machine is one of the key new concepts introduced in the book. She tells us that a nomological machine is a configuration of objects and properties that have stable capacities or powers (p. 50). It may be very simple, such as a rigid rod placed on a fulcrum that serves as a lever; or it may be very complicated, such as the experimental device of the Stanford Gravity Probe. The important feature of such machines is that they possess causal powers or capacities that generate regular behaviour when the machines are set running in the right conditions. The right conditions include the fact that the machine is shielded from causal influences that are extraneous or extrinsic to its operation. Exceptionless regularities are hard to find because nomological machines operating in shielded conditions seldom occur naturally. Often enough it requires experimental intervention and control to shield a nomological machine in the right way for it to generate the appropriate regularities (pp. 57–8).

In Cartwright's view, nomological machines are more fundamental in science than the empiricists' regularities among occurrent properties. They are more fundamental epistemologically since knowledge of capacities is more widely useful in science than knowledge of regularities. She illustrates this point with a discussion of the Stanford Gravity Probe experiment (pp. 85–95). The purpose of the experiment is to measure the rate of the precession of gyroscopes in space to see how they are affected by the space-time curvature relativistically induced by the earth. The experiment involves a complex instrument consisting of four fused gyroscopes, electromagnetically suspended in a cryogenic dewar, whose rate of precession is measured by a superconducting quantum interference device. Cartwright argues that it is impossible to understand the physicist's activity in designing and constructing this device in empiricist terms. They do not design and construct this complex one-of-a-kind device on the basis of regularities between occurrent properties, but rather on the basis of the causal powers and capacities of the individual component parts and their manner of arrangement in the complex device.

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Cartwright also argues that capacities are more fundamental ontologically than regularities since the existence of regularities depends on the exercise of various capacities in a nomological machine (pp. 64–8). For example, to build a nomological machine we have to compose causes to produce a targeted effect, but the composition of causes only makes sense in terms of capacities. Consider again the way in which Newton's law of gravity and Coulomb's law work together to produce the trajectory of two charged bodies. Newton's law describes the capacity of a body to move and to produce motion on account of its gravitational mass, while Coulomb's law describes the body's capacity for the same on account of its charge. It is simply not plausible to render either law as a claim about regular associations in occurrent properties. Furthermore, in order to ensure that a nomological machine operates in the right shielded conditions we have to ensure that certain capacities are exercised in the right way. For example, to determine the motion of two charged bodies we add vectorially the force in Coulomb's law and the force in Newton's law and then we substitute the result into Newton's second law. But in doing so we suppose nothing inhibits either object from exerting both its Coulomb and its gravitational force on the other; no other forces are exerted on either body; and that everything that happens to either body that can affect their motions can be represented as a force. All these suppositions are most plausibly interpreted in terms of capacities and their exercise.

For my own part, I find Cartwright's claims about the fundamental epistemological and ontological status of capacities in science to be very plausible indeed. I think that the alternative Aristotelian metaphysics that she is proposing is more faithful to the presuppositions of scientific practice than the rival empiricist metaphysics.[1] Nonetheless, there are some aspects of Cartwright's metaphysical framework that are unclear and stand in need of clarification. Suppose that we accept her claim that capacities are more ontologically basic than regularities, the latter arising from the former in felicitous circumstances. How exactly are we to understand the nature of capacities or causal powers? What in the metaphysical scheme of things are capacities? What makes a claim about capacities true?

Cartwright is not insensitive to such metaphysical questions and tries to say something in reply to them. At one point she discusses the question of what would make true the claim 'Inversion in a population of atoms has the capacity to produce coherent radiation'. She writes: "In simple Tarski fashion, just that: the fact that inversion has the capacity to produce coherent radiation" (p. 73). At face value, this might be taken to mean that capacities are ontologically primitive. Since capacities are ontologically irreducible, there is nothing more informative that can be said about the truth-makers for capacity claims. Despite its popularity among metaphysical quietists, this answer is

1. Recent work in cognitive and developmental psychology demonstrates the pervasiveness of Aristotelian metaphysical categories in human thought. See D. Medin and A. Ortony, 'Psychological essentialism', in S. Vosniadou and A. Ortony (eds.), *Similarity and Analogical Reasoning* (Cambridge University Press, 1989), pp. 179–195; F. Keil, *Concepts, Kinds, and Cognitive development* (MIT Press, 1989); and S. Gelman, 'The Role of Essentialism in Children's Concepts', *Advances in Child Development and Behaviour*, 27 (1997), pp. 56–98.

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profoundly unsatisfying from an explanatory point of view. It does not do anything to answer the pressing questions: What is the nature of capacities? How do they relate to counterfactuals, causation, and laws?

It is gratifying, then, that Cartwright does not actually settle for this quietist response. Cartwright endorses the intuition that inversion allows coherent radiation "by virtue of the structure of the world" and says that the best account of this intuition is to be given in terms of Aristotle's doctrines of natures. She writes: "Capacity claims, about charge, say, are made true by facts about what it is in the nature of an object to do by virtue of being charged" (p. 72). As she says, taking this stance is to make a radical departure from the usual empiricist view about what kinds of facts there are. But how radical a departure is involved does not become clear until we can answer the question 'What exactly is the meaning of a capacity claim?'.

Capacities and Natures

In order to answer the question 'What makes a capacity claim true?' we need to know the meaning of a capacity claim. It is not possible to determine the truth-maker for a claim like 'Inversion has the capacity to produce radiation', for instance, until we know under what conditions the claim is true. This is not to say that we have to produce an analysis that reduces capacity concepts to simpler concepts. That may not be possible if the concept is primitive. Nonetheless, an informative explanation of the truth-conditions for capacity claims, even when non-reductive, may help to answer the ontological question of truth-makers. But unfortunately Cartwright is silent on this issue: she does not even attempt to give an informal explanation of what is meant by a capacity claim.

Despite this omission, Cartwright does say quite a few things about capacities themselves, from which we can infer how she understands the capacity concept. She attributes the following characteristics, among others, to capacities:

(1) Capacities are multi-track in the sense that they can manifest themselves in many different ways. How capacities manifest themselves in occurrent behaviour depends essentially on the setting. For example, in one setting similarly charged particles may repel each other due to the capacities described by Coulomb's laws, while in a different setting they may actually attract each other due to the very same capacities (pp. 59–64).

(2) Capacities are grounded in the natures of objects. For example, the capacity that similarly charged particles have to repel each other is due to their nature, *qua* charged particles. These particles have this nature even when they do not manifest it in occurrent behaviour. For example, if two similarly charged particles do not repel each other because of their large gravitational masses, they still possess this capacity in virtue of their natures, *qua* charged particles (pp. 77–82).

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(3) Capacities manifest characteristic behaviour when their natures are held constant and shielded from interfering factors. So, when similarly charged particles have the capacity to repel each other, they will do so provided the right conditions occur for the capacity to exercise itself 'on its own', for instance, if they have very small masses so that their gravitational effects are negligible (pp. 83–4).

I find these characterisations very plausible. But how do we know that they are characterisations of the same kind of thing? How do we know that capacity claims are not multiply ambiguous, referring to different kinds of things? One could allay such worries by showing how the different characteristics Cartwright attributes to capacities issue from a single unified nature captured by the univocal meaning of capacity statements. But, as I have said, Cartwright does not provide any such explanatory account.

Still it may be worth considering briefly what kind of unified account might be given of the meaning of capacity claims that would make sense of the different things Cartwright says about capacities. I suggest that the following truth-conditions provide a coherent account of Cartwright's doctrines about capacities:

> *x has the capacity to manifest response r (in response to stimulus s) in circumstances C* iff there is some intrinsic property *F* of *x* such that if *x* were to receive stimulus *s* in circumstances *C* while possessing property *F*, then *x* would manifest response *r*, provided all interfering forces were absent.

The guiding idea is that the truth-conditions for a capacity claim are explained in terms of a counterfactual about a characteristic stimulus-response pair. A multi-track capacity concept would be spelled out in terms of a battery of such counterfactuals, each counterfactual linking a set of circumstances with some characteristic stimulus-response pair.

Many questions might be raised about whether this account really represents Cartwright's intentions. Is her idea about the multi-track character of capacities captured accurately by relativising the defining counterfactual to a set of circumstances? Is her talk of the nature that grounds a capacity best captured by talk about the intrinsic properties of objects? However, I propose to set aside these questions for now. Even if this account does not capture every nuance of Cartwright's views, it gives us a close enough approximation to be able to tell what would count on her view as the truth-maker for a capacity claim.[2] Clearly, the truth-maker for a claim about some capacity possessed by an object will be the instance of the intrinsic property that represents its underlying nature.

2. I would also argue that it is independently plausible in virtue of its ability to handle some recently much-discussed problem cases about finkish dispositions. See C.B. Martin, 'Dispositions and Conditionals', *The Philosophical Quarterly*, 44 (1994), pp. 1–8; and David Lewis, 'Finkish Dispositions', *The Philosophical Quarterly*, 47 (1997), pp. 143–58.

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What exactly is this truth-making intrinsic property? At first sight, there seem to be two possible answers to this question, each of which poses considerable difficulties for Cartwright's view.

One answer is that the intrinsic property is the capacity itself. So, the ontological ground of the capacity of two similarly charged electrons to repel each other in certain circumstances is that very capacity itself. This sort of answer harks back to a scholastic conception of capacities as occult properties that can only be defined and identified in terms of what they are capacities to do. The problem with this conception is the famous problem raised by Hume. Consider a capacity possessed by an object *x* to give response *r* to stimulus *s* in circumstances *C*. What is the relationship between the stimulus *s*, the instance of truth-making intrinsic property *F*, the circumstances *C* (including the absence of interfering forces), on the one hand, and the response *r*, on the other hand? If the intrinsic property *F* is the capacity itself, then the connection must be logically necessary. For the capacity, taken in conjunction with the stimulus condition and circumstances, logically implies the response. However, the capacity is presumably a distinctive ontological item, separate from both the stimulus condition and the circumstances. Accordingly, if this is indeed the case, the logical implication between these entities would be a straightforward violation of the principle that there are no logically necessary connections between distinct existences.

For Humean reasons of this kind, it is much more plausible to think of the truth-making intrinsic property as different from the capacity itself. Indeed this seems to be the way that Cartwright seems to think of the natures that ground capacities. She writes:

> Modern explanation similarly relies on natures, I will argue, though modern natures are like Bacon's and unlike those of the Scholastics in that they are attributed to structures and qualities we can independently identify. Generally they differ from Bacon's in that they do not lie on the surface and are not to be observed with the naked eye. We often need very subtle and elaborate experiments in order to see them. Modern science insists that we found explanation on experimentally identifiable and verifiable structures and qualities. But, I maintain, what we learn about these structures and qualities is what it is in their natures to do. (pp. 80–1)

This passage makes it clear that she thinks that the nature that grounds a capacity is different from the capacity itself. It is an independently defined and identified structural feature of the thing possessing the capacity. Her Aristotelian doctrine of natures is a scientific one that stresses the explanatory character of natures.

Given this identification of the truth-making property *F*, it is very plausible to think that the relationship between the stimulus *s*, the instance of property *F*, and the circumstances *C*, on the one hand, and the response *r*, on the other hand, is contingent. But this prompts the following questions. How do the stimulus condition, the instance of the intrinsic property, and circumstances contingently give rise to the response? What has to be added to the instance

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of the intrinsic property and the circumstances to justify the counterfactual inference from the stimulus to the response? The answer that will spring to the minds of most philosophers is: the laws of nature. For most philosophers would regard the counterfactuals that are conceptually linked to capacities as having to make essential use of the laws of nature to link a counterfactual antecedent with its consequent. But if this is right, we have a picture that contradicts the ontological priority Cartwright gives to capacities over laws of nature. It now appears, contrary to what she has proposed, that the ontological grounding of capacities presupposes the independent existence of laws of nature.

This problem does not have anything essentially to do with counterfactuals. The crucial point can be made in terms of the concept of supervenience that is traditionally employed to spell out relations of ontological dependence. One way of formulating the thesis that an intrinsic property is the ontological ground of a capacity is to say that the capacity strongly supervenes on the intrinsic property. A supervenience claim of this kind would amount to this: the fragility capacity, say, strongly supervenes on the property *F* iff, for any objects *x* and *y* and any worlds w_i and w_j from a given set of worlds, if *x* in w_i and *y* in w_j are indiscernible with respect to property *F* then they are indiscernible with respect to the possession of the fragility capacity.[3] A crucial question that has to be answered to determine the strength of the supervenience concerns the size and composition of the set of possible worlds over which the world-variables w_i and w_j range. If the set is the entire set of possible worlds, the supervenience claim is maximally strong. But most philosophers would regard such a supervenience claim as far too strong. For there is good reason to think that an object in a logically possible world might have the same intrinsic properties as a fragile object in an actual world, but lack the capacity of fragility by virtue of conforming to different laws of nature. So it is reasonable to think that the set of worlds invoked in the supervenience claim must be circumscribed in some way so as to make the connection between underlying ground and capacity contingent. And here the view of the overwhelming majority of philosophers is that the set of worlds must be restricted to those worlds that share the same laws of nature. Once more, then, the laws of nature enter into the specification of the ontological grounds of capacity statements in a central way, casting doubt on Cartwright's claim of the ontological priority of capacities over laws.

It would be interesting to see how Cartwright would respond to this difficulty. Unfortunately, she never addresses it directly. The nearest she comes to touching on the issue is a discussion of the relationship between an individual's nature and what it is in the nature of the individual to do. She writes: "There is a 'brute-fact' connection between what charge is and how charged particles behave *qua* charged" (p. 83). The reference to the " 'brute-fact' connection" suggests that she sees the connection as contingent. But it also suggests that she does not see the need to explain its contingency. This is

3. For discussion of the various concepts of supervenience see the articles in J. Kim, *Supervenience and Mind* (Cambridge University Press, 1993).

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disappointing because there is a pressing need to answer the ontological question, 'In virtue of which feature of the world does an object's nature, when taken in conjunction with a stimulus condition and circumstances, yield a response characteristic of its capacity?'. Perhaps this question can be answered without reference to laws of nature. Perhaps it could be argued that the contingent feature of the world that connects a capacity's stimulus condition, underlying nature, and circumstances, on the one hand, and response, on the other hand, are singular causal relations that do not depend on laws of nature. Whatever answer is given to this question will have profound implications for the ontological picture of capacities.

Critique of fundamentalism

One of the targets that Cartwright constantly has in her sights throughout the book is a doctrine she calls fundamentalism. She says that she may have been mistaken in the past about her principal enemy. In her earlier book *How the Laws of Physics Lie*, she thought it was realism that she needed to combat. Now she thinks the real enemy is fundamentalism.

She characterises this as the view that there is a set of fundamental laws that are "universal, holding everywhere and governing in all domains" (p. 24). Opposed to fundamentalism is the doctrine that she endorses and calls pluralism. Pluralism is the view that "nature is governed in different domains by different systems of laws not necessarily related to each other in any systematic or uniform way; by a patchwork of laws" (p. 31).

On close inspection, one can see that her discussion of fundamentalism lumps together two quite different theses.

1. A thesis of the *universal cover of law*. Fundamentalism in the natural sciences, for instance, is the view that the fundamental laws of physics apply universally, even to phenomena outside the special shielding conditions of the laboratory.
2. A thesis of *explanatory reductionism*. Fundamentalism in the natural sciences, for instance, is the view that the fundamental laws of physics apply to all physical systems and explain all physical phenomena.

Cartwright recognises that these are distinct theses. At one point she says that her intention is to challenge not only traditional fundamentalist views about 'downwards reduction', but also less familiar fundamentalist views about 'cross-wise reduction', by which she means the exportation of laws from highly contrived experimental settings to less regulated situations (p. 25). At any rate, given the different logical and evidential status of the two theses, it is worth discussing them separately.

Her denial of the first thesis of the universal cover of law follows straightforwardly from her claim that all laws, even those of fundamental physics, are *ceteris paribus* laws. As we have seen, she argues that the fundamental laws of physics hold only in very special circumstances, usually artificially contrived

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experimental settings. If she is correct in this claim, as I believe she is, it follows straightforwardly that these laws do not apply universally. So this point seems correct.

Where she goes wrong, it seems to me, is in drawing strong metaphysical conclusions from this observation. She insists that the failure of universality supports the pluralist picture of nature falling into different domains covered by different laws that are unrelated to each other. But at most her observation shows that physical laws do not apply when their *ceteris paribus* conditions do not hold; and that scientific explanations of what happens in non-experimental settings must proceed differently from explanations of what happens in experimental settings. But even on this point, her doctrine that explanations often proceed in terms of capacities, grounded in stable natures, runs counter to her pluralism. For her doctrine about capacities that are based on stable natures supports the view that explanations of what happens in experimental settings can be exported into explanations of what happens in non-experimental settings. As she herself puts it: "To ascribe a behaviour to the nature of a feature is to claim that that behaviour is exportable beyond the strict confines of the *ceteris paribus* conditions, although usually only as a 'tendency' or a 'trying'" (p. 29). In terms of her standard example, we can say that it is in the nature of a force to produce an acceleration *ceteris paribus*. But even when the *ceteris paribus* conditions are not met, the given force still 'tries' to produce the requisite acceleration, this assumption being the basis for the standard story of vector addition of forces. So it would seem that the doctrine about the exportability of capacities on the basis of their stable natures does not support, but rather undermines, the idea that explanations of experimental and non-experimental phenomena are unrelated.

The second thesis of explanatory reductionism seems much more relevant to the issue of monist versus pluralist views of metaphysics. She argues against this second thesis that we must adopt a 'scientific attitude' towards fundamentalist reductionist programmes, judging them by their actual success in constructing explanatory models of the phenomena that fall in their domain. When judged in terms of this criterion, most reductionist programmes should be regarded as failures: they are expressions of faith rather than well-confirmed theories. By way of illustrating this point, she considers a fundamentalist attempt to use classical mechanics to explain the motion of the dollar bill that is dropped from a height but swept away by the wind (pp. 27–8). She argues that mechanical models are of no use in explaining fluid motions of this kind. She rejects the idea that since the wind is composed of millions of little particles that must exert all the usual forces on the bill, it should be possible in principle to construct a mechanical model of its motion. She argues that until we have a good fitting molecular model for the wind and we have in our theory systematic rules that assign force functions to the model, then we have no good scientific reason to maintain that the wind operates via a force. This example makes it look as though Cartwright may be vulnerable to the criticism that her metaphysics is excessively restricted by the practical computational limitations involved in model construction.

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Nonetheless, her general point that we should judge reductionist programmes more by their explanatory successes than their promises is a good one.

However, Cartwright goes beyond criticising the overweening ambitions of reductionist programmes to suggest an alternative metaphysics that would vindicate the explanatory autonomy of the different sciences by assigning them their own proprietary domains of irreducible properties and laws. This alternative metaphysics rejects the thesis that all reality supervenes on the distribution of microphysical properties and relations (p. 32). Many will find the rejection of this thesis hard to swallow, especially since no convincing counterexamples to it are offered. Indeed, her discussion of supervenience seems to labour under the misapprehension that supervenience amounts to a form of token-token reductionism (p. 32). It is true, to be sure, that philosophers of mind such as Davidson[4] who have rejected type-type reductionism about the mind have endorsed both the supervenience of the mental on the physical and token-token identity theories of the mind. But these are logically distinct doctrines. The supervenience of the mental on the physical can be articulated as a thesis about properties that does not, by itself, imply anything about the identity conditions of events. Thus, to say that the mental supervenes on the physical is to say that no individuals can differ in mental properties without differing in physical properties. To get from this to the identity of mental and physical events one needs to appeal to some very contentious assumptions about the nature of events.

By repudiating the microphysical supervenience thesis, Cartwright creates unnecessary difficulties for herself. As I have argued above, supervenience provides a clear way of explaining the ontological dependence of capacities on their underlying natures. In any case, the thesis that macro-level properties supervene on micro-level properties seems very plausible and its denial hard to defend. Does she really think that two situations could differ in their colour properties, say, and yet be identical in their microphysical properties? She mistakenly thinks she must deny microphysical supervenience because it amounts to a form of reductionism, albeit token-token reductionism. But recent discussions in the philosophy of mind and metaphysics indicate that interesting forms of supervenience can be formulated that allow for the explanatory autonomy of macro-level properties and laws.[5] At this point Cartwright's discussion could have drawn profitably from the analytic framework of supervenience that has served well to clarify issues of reductionism. This framework shows, I believe, how one can believe in explanatorily autonomous levels of properties and laws without having to deny microphysical supervenience.

4. See his 'Mental Events', in his *Essays on Actions and Events* (Oxford University Press, 1980).
5. See J. Kim 'Concepts of Supervenience', *Philosophy and Phenomenological Research*, 45 (1984), pp. 153–76; J. Kim 'Supervenience as a Philosophical Concept', *Metaphilosophy*, 21 (1990), pp. 1–27; and P. Teller 'A Poor Man's Guide to Supervenience and Determination', *The Southern Journal of Philosophy*, 22 (1984), pp. 137–62.

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REPLY

These three pieces look at different aspects of *Dappled World* in a serious and careful way, and I am very grateful to the authors for their comments. I am almost entirely in agreement with Peter Lipton. Occasionally I overstate the case for the dappled world. That's because the vision of a dappled world delights me. With Gerard Manley Hopkins, I love "all things counter, original, spare, strange".[1] It is also because many take the alternative 'fundamentalist' world to be the only reasonable view consistent with the successes of modern science. So it is important to state the arguments for dappling in the strongest terms possible.

My own assessment of the pros and cons is essentially Lipton's. The case is still out. I, though, would bet that matters will remain that way for a very, very long time. These are grand metaphysical issues and so long as we are loyal to our empiricist strictures we are likely not to find answers to them. I engage in metaphysics myself primarily for methodological reasons. The image of a world unified under the universal rule of law has a powerful grip; it influences scientific decisions that should instead be made entirely on their empirical merit.

My resistance to metaphysics makes me disappointing to both *Laurie Paul* and *Peter Menzies,* who look for answers to a number of metaphysical questions that I remain silent about. *Paul* asks, How are objects and properties related? Are objects collections of properties; or is there some substantial substratum; or . . . ? This is an important issue but I have nothing to say about it. As with all metaphysical issues, there are a number of views on offer, some better than others, all with some problems. I do not think that my views about dappling add to these problems in any significant way—you can add them to your favourite metaphysics of substance, accident, object and property.

The metaphysical view I defend is about laws. Many of our most well-confirmed law claims in the exact sciences ascribe capacities to properties.[2] For example, 'A mass of magnitude m brings with it a capacity of magnitude Gm/r^2 to cause another mass a distance r away to move towards it'. They also tell us, derivatively, what capacities an object or structure has by virtue of having the designated property. Because the property ensures the associated capacity, there are a variety of things an object *will do* by virtue of having that property—for instance, attract other objects with masses; there are a variety of things it *can do* by virtue of having the property—such as cause another object to move in a near elliptical orbit around itself when other causes of motion are negligible; and there are also outcomes it *can contribute* to—such as holding a feather in mid-air, in conjunction with a vigorous updraft.

1. Hopkins, 'Pied Beauty'.
2. Perhaps when Paul says, citing p. 81, that I see objects as collections of properties or structures (which I at least did not intend to say), it is because she supposes that laws assign natures or capacities to objects and hence infers this view about objects from my claim there that "we [as opposed to Aristotle] assign natures not to substances but rather to collections or configurations of properties, or structures".

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Are capacities additional properties that the object has whenever it has the designated property? This is again a question whose answer is not central to my main theses. I do have views about it, but these should be separable from the main theses about the extent of law in nature.

I personally find it impossible to understand the distinction between occurrent properties and dispositions or causal powers.[3] So I am inclined to say that there just are properties. Sometimes we refer to them using 'occurrent property' language; but sometimes by a capacity or power word—often when we wish to highlight either some specific way of finding out that the property obtains or some specific effects an object can produce in virtue of having that property. As Paul notes, I think this proposal fits nicely with Shoemaker's account of properties and powers. If I am right, it is like cases Rudolf Carnap discusses. We express the distinction in the material mode, but it would be more perspicuously cast in the formal mode.

Paul also asks if the having of certain capacities by certain properties is a primitive necessary connection. That depends on how one thinks about laws of nature. When I say that it is *in the nature of*, say, mass to attract other masses, I mean to imply that this is a *law of nature*—though not in the regularity sense of law. Again, I am disappointing from the metaphysician's point of view. I have nothing to say about what makes a law a law, except to protest that most law claims in exact science will not come out true if we see laws as regularities, or counterfactual regularities or 'necessary' regularities among what are conventionally labelled 'occurrent' properties.

When I say that the connection is 'brute fact' that is not to deny that it holds reliably, nor that it would hold counterfactually.[4] Rather I mean to deny that it holds by definition of the property. Scholastic philosophy hoped to find the 'true' definitions of properties from which all other reliable facts about them would follow. This project seems not to work. Now we characterise our properties loosely, and have a great deal to say about how they will behave that does not follow from any definition.

So I have, unfortunately perhaps, nothing of interest to offer about what an object *is*, what a property *is*, what a capacity *is* or what a law *is*. My view that laws associate capacities to properties should be consistent with a variety of different answers to these metaphysical questions.

There are two issues of concern to Paul that I do have views about: repeatability and realism. Paul asks, "Why aren't capacities expressed all the time, even in unrepeatable circumstances?" There is one immediate answer. Some

3. I also have no metaphysical views about dispositions versus capacities versus powers. I choose the word 'capacity' since it is less often used by others; hence it carries fewer presuppositions with it. (I do note in *Dappled World* that one conventional view of dispositions, the view that ties them to a single manifestation, is too narrow for capacities, which are more like Gilbert Ryle's "generic dispositions" or what Menzies calls "multi-track".)
4. A connection between a property and a capacity need not hold absolutely reliably. It may hold only in certain circumstances, or with a certain probability, or possibly sometimes, sometimes not. I take it that it is the job of science to spell this out for us. The associations I have studied most intensively in physics all seem to be universal; those in social science are more likely to be relative to certain institutional and political arrangements.

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capacities need triggering; some capacities express themselves only in circumscribed circumstances; some express themselves only probabilistically; perhaps some in a haphazard manner.

Another answer depends on what we mean by 'express'. For many capacities, we have a word that describes their operation whether or not the canonical result is achieved. For instance, one mass can *attract* another, even if the second does not move. Other such words are *repel*, *pull*, *damn*, *brake*, *harden* (as in steel), . . . Very often a capacity will operate under any circumstances, even those not favourable for achieving its canonical effect (or operate always when triggered, or always in certain kinds of circumstances). Masses seem to be like that; they always attract each other. If we see *attraction* as the expression of the capacity of the mass, then the expression of the capacity is repeated across different circumstances. Moreover, it *would be* repeated in circumstances that for some reason or another are never repeated.

We may, on the other hand, think of the expression of the capacity as what ultimately happens, described in the language of occurrent properties. Does the second object move or not, and how? In this case we have a different question about repeatability. For every situation in which a capacity obtains and an outcome *o* eventuates, is there some description, *D*, of that situation such that whenever *D* is satisfied, *o* results? A 'yes' answer is supported by the assumption that there are always meta-principles that tell when a capacity operates and further meta-principles (principles of composition) that fix what happens under any arrangement of capacities and any concrete interferences. A 'no' answer gives us what *Peter Lipton* calls "anomalous dappling", which is the view that I propose.

I advocate anomalous dappling. But I agree with everything that *Lipton* says when he asks, "Why believe in anomalous dappling?" The evidence is not compelling either way. That is why I urge us not to allow a metaphysical conviction on this issue, one way or another, to affect our strategies for future research or our assessment of the acceptability of proposed scientific hypotheses and policies. Lipton describes my methodological advice as "Construction and Autonomy". Again, I agree with his characterisation and with his claim. One can reasonably advocate construction and autonomy if one is a fundamentalist and even the anti-fundamentalist should expect many of our better theories to stretch further than their current boundaries. That is why I urge that research proposals be judged on their actual detailed promise.[5] I am, however, less sanguine than Lipton about the possibility of bringing reasonable metaphysical arguments to bear. Indeed, I am doubtful about the practical effectiveness of any of our arguments against take-overs by a single discipline or method or theory. Consider just a few of the most well-known: the Theory of Everything in physics, the gene programme in biology, evolutionary psychology and game theory. These all, I believe, get disproportionate attention and funding just because of their promise to be universal.[6]

5. Plus of course some cost/benefit analysis.
6. We do here, of course, also have to allow for human competencies. It may be reasonable to fund a programme which we know we can carry out well over one with slightly more promise that we can not carry out properly.

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The one thing I can say to Paul and Lipton against repeatability and in favour of anomalous dappling is to remind them of the arguments in *Dappled World* that look at how our successful models work in exact science. Across a very wide range of physics theories, I argue, central terms (like 'force' or the tensor of general relativity) are used as *abstract* terms: they always need some one or another from a handful of more concrete descriptions to obtain before they can be properly applied. These are the descriptions supplied by our bridge principles. I call them "interpretive models".

For instance, we can legitimately employ a description of a system as subject to a force Gm/r^2 only if it is a massive object located a distance r from another mass of magnitude m; we can employ $\varepsilon q/r^2$ only to a charged object located a distance r from a charge of magnitude q; and so forth. The same, I claim is true for quantum theory, quantum electrodynamics, quantum field theory, classical electricity and magnetism, statistical mechanics, and probably many other theories in physics. This gives us a clear delimitation of the boundaries of these theories. A theory stretches only so far as its interpretive models fit.

Economics theories face the same kind of constraints, but for different reasons. They use not abstract but rather highly concrete concepts. But they do not have a lot of principles available about how these concrete concepts behave. This makes it difficult to produce rigorous derivations of the kind that economists demand. My investigations suggest that we manage to get deductive proofs in models in economics by adding a lot of further specific assumptions to make up for the lack of general principles. These are often (misleadingly to my mind) called 'idealising assumptions'. These assumptions tend to be true of at best very limited economic situations. So, as in physics, the models again do not look on the face of it as if they fit a very wide range of real world situations.

I assume that Lipton, in the spirit of his other claims, would respond that the evidence one way or another about how far these models really fit, independent of our knowledge of it, is not conclusive. Again I would agree. The evidence against fundamentalism in physics or economics or elsewhere is not compelling. Nor is the evidence in its favour. The world may be dappled after all, or it may not be.

Lipton has another worry about repeatability. If, as I urge, we get regular outcomes only when a situation is shielded then we will get few regularities indeed since perfect shielding is rare. Yet again I agree with Lipton. Where shielding is imperfect, we cannot expect perfect regularities. But it seems to be a fact about the world that, where there are principles of composition for the different capacities at work, if the disturbances are small, the principles are approximately satisfied. What this amounts to in a given situation will depend on the concrete details, as will the question of whether a given factor counts as a small disturbance or not. In some cases the exactly predicted behaviour will result most of the time, but there will be exceptions; in other cases the results will hover around the predicted behaviour; and so forth.

These are matters that are often well understood in specific cases. A small virus can produce a large disturbance to the regular functioning of an

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organism; whereas a fairly large shove may have little effect on a heavy machine. We may ask what kind of fact it is that we understand when we understand that a given factor constitutes a small or a large disturbance. Do we not need some kind of law to determine this? I do not see why. We successfully shield against disturbances all the time; we make calculations about how much shielding is likely to be enough and we are very often right. Very often these calculations are not based on laws at all, but on knowledge expressed directly in the vocabulary of shielding and disturbance. I do not see why nature must speak a different language from us.

Let us return now to *Paul* and her concern about realism and universality. She is right that my views are consistent with realism because our claims ascribing capacities to properties might well be true. As she says, these connections "exist independently of anything pragmatic". She is also right in her suggestion that repeatability can be secured "in some way that is independent of human interest". That is true for both senses of 'repeatability'. If it is repetition of the outcome that is at stake, that can happen whenever there is appropriate shielding; and nature can—and does—build her own shields without our help. If it is repeatability of the expression of a capacity that we want, then, as we have seen above, that may be fairly widespread. So the commitment to anomalous dappling and the reasons for it need not carry one far "towards the antirealist camp".

Peter Menzies is particularly concerned with the metaphysics of capacities. What makes a capacity claim true? My simple answer, Tarski-style: 'x has the capacity to r (in response to stimulus C)' is true iff x does have the capacity to r. . . . This will seem satisfactory only if we suppose that *capacity* is an unproblematic enough notion to figure on the right-hand-side. That is what I argue, both in *The Dappled World* and in *Nature's Capacities and their Measurement*. There are no special kinds of problems that beset causal concepts, including those having to do with capacities, that do not equally beset whatever may be your favourite choice for 'okay' concepts—measurable properties, 'occurrent' properties, 'intrinsic' properties, pure quantities, . . . Causal and capacity concepts have no special semantic, epistemic or ontological problems.

Menzies himself admits that we do not need a reductive account of capacities. The three characteristics he attributes to them as part of an informative account are indeed ones I advocate. In *Nature's Capacities* I say a lot more. This includes discussions of how we measure capacities, including both probabilistic and experimental methods; how capacities relate to John Stuart Mill's tendencies; how they are represented and studied in 'idealised' models; and how capacity claims relate both to what are more usually thought of as laws, both causal and associational, and to singular causal claims. Menzies asks, "How do we know that capacity claims are not multiply ambiguous, referring to different kinds of things?" We know that, I take it, by empirical research. This is the way we have found the world to be.

Menzies's own proposal is that "the truth condition for a capacity claim are explained in terms of a counterfactual about a stimulus response pair", where "a multi-track capacity would be spelled out in terms of a battery of such

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counterfactuals". Then the truth-maker for the capacity claim is the intrinsic property that 'grounds' the counterfactuals.

I do not think this proposal will work. That is not because of the problems Menzies raises about the need for laws to flesh it out. I agree that there is not only the grounding property but a law as well. But the law connects the property with a capacity, not with a display of the capacity that can be described in the language of occurrent properties.[7]

One problem in limiting ourselves to laws that associate the grounding property with a display of the capacity is a complicated one about knowing what we are testing in a controlled experiment. This problem is discussed in the chapter about Aristotelian natures in *The Dappled World*. Here I shall rehearse two more central worries. Both have to do with the open, or multi-track, nature of capacities.

Counterfactuals are too weak to handle openness. My breakfast cereal box tells me, 'Shredded Wheat can improve the health of your heart.' Or, when my daughter and I use a magnet to try to retrieve our earring from between the floorboards, we do so because the magnet *may* or *might well* lift the earring. Not only do we need funny modalities; we also have trouble specifying the exact responses. We say that the magnet might well *lift* the earring, but there may be no fact of the matter about the exact motion. Of course there will be if all causes of motion and all interferences can be represented as forces and forces always add vectorially.[8] But much of my argumentation should cast doubt on this. I think it goes far beyond our evidence. The overall conclusion I draw from both these considerations is that there is no way to refer to all of the varied responses and all of the various modal truths about them at once, except by describing the capacity on account of which they are true.

My second objection is that, when we can, we should render law claims in science in a way that resembles what we actually tend to assert. I say, masses *attract* other masses or that skill loss during unemployment *tends* to perpetuate high unemployment rates. There are three standard ways to render these claims without using capacity language.

One is as *ceteris paribus* laws: If nothing interferes, then. . . . This has two drawbacks. First it includes the term 'interference', which for many is as abhorrent as 'capacity'. We might hope to replace this by an 'occurrent' property description. I am not sure that we can. In mechanics we have our best chance: 'If no other force occurs, then. . . .' This supposes that all causes of motion can be represented as forces. I have already explained why, because of the way 'force' is used in those very successes that argue most powerfully

7. If we think of an occurrent-property concept and a capacity concept as referring to one and the same thing, then we will read the law as telling us about the association of one capacity with another set of features that we have already associated together under the occurrent-property concept.
8. Lipton says that I deny that there is a rule of composition for forces. On the contrary, I think we have good inductive evidence for vector addition. What I deny—or wish to remain agnostic about—is that there is a rule of composition for *causes* of motion, since these may not all be appropriately represented as forces, as well as for interferences that might undermine the principle $f = ma$.

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for the truth of mechanics, we should be suspicious of this claim. The second reason is less controversial. This rendering does not say anything about all those cases where interferences do occur and where we want to use these laws to help calculate the result.

The second way is to assert that the characteristic response really is there after all. There are two versions of this strategy. One uses words like 'attract' to describe the characteristic response. Here we employ a word referring to the successful operation or, in Paul's words, 'exercise' of a capacity instead of referring to the capacity itself. Surely this will not satisfy those who are unhappy with capacities to begin with.

The other version assumes that the response, described in purely 'occurrent' property language, obtains even if it does not look that way. People have mixed intuitions about motions. Is the stationary object suspended between the magnet and the earth both moving up and moving down? Whatever we say in this case, the analogue is implausible in other cases. For instance, we know how to calculate the characteristics of a current in circuits from the capacities of the components to affect those characteristics, but the rules look nothing like addition. In fact, I know of only one case where this strategy is entirely plausible—in structural models in econometrics. There each capacity is represented in a separate equation; when different capacities act together, all the equations must genuinely satisfied at once, so that the behaviour described in each and every equation will be obtain.

The third way takes the laws as we use them and our meta-principles of composition to be a shorthand referring to an unending variety of complicated laws where all possible combinations of factors occur in the antecedent. My objections to this are the ones I have already made to Menzies's proposal involving a vast array of counterfactuals: I think neither the laws nor the counterfactuals exhaust the truths that capacities can produce. The point is that there is nothing wrong with capacities. We use capacity language in a coherent way all the time. We do not need to lose the power of capacity language to convey truths about our world that reference to laws and counterfactuals cannot convey.

Menzies urges not only that capacities supervene on counterfacts; he also says that I am wrong to deny that "all reality supervenes on the distribution of microphysical properties and relations". My reasons for not adopting supervenience have nothing to do with a narrow focus on token-token identity, as Menzies suggests. I think, rather, if this incredibly strong thesis is to be countenanced, convincing arguments should be on offer; and I do not know any. Most of the effort in the literature is devoted to trying to formulate it correctly, not to defending it.

The structure of thought seems much like that of Humeans vis-à-vis causality. We begin with the assumption that some properties—microphysical properties of microphysical systems—are okay. Then we challenge: How can we make sense of the rest? But there is no principled reason to admit the one at the cost of the other to begin with. As with causal concepts, concepts of other sciences or other theories have no special semantic, epistemic or ontological problems of a kind that do not beset the privileged properties.

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For most concrete instantiations of most laws of physics, the eliciting and shielding conditions cannot be described in the same theory as the law itself and indeed, generally features must be included that are not described in any known physics at all. Importantly, this is true of our most successful uses of laws that provide the evidence we need for belief in their truth. In the face of this, supervenience theses need strong evidence in their favour since exactly the reverse of supervenience is what we see in our best physics.

Concepts from macrophysics and from various branches of technology and engineering are required *in conjunction with* those of 'microphysics' to obtain true law statements (in the usual, regularity or counterfactual sense of 'law'). Why then should we expect that the requisite factors that do not come from microphysics supervene on those that do? Menzies asks about colour. The question needs a detailed empirical investigation that I have not carried out. So I shall restrict myself to a case that I have studied.

In *The Dappled World* I give a number of examples of how quantum and classical concepts cooperate in producing accurate accounts of the kind that can convince us of the truth of quantum claims. Neither alone suffices. We must not be misled by familiar reconstructions of quantum theory that tell us that the quantum state provides probabilities for what values classical quantities will take on measurement, and that's all. That principle is not often called into play in real models. The connections between the two theories in the successful models I have studied are highly various. Sometimes quantum quantities cause classical quantities, sometimes the reverse and sometimes there are local identifications. Nothing in any treatment I know, of either experimental tests or of any of our highly successful quantum technologies, provides support for the reducibility, the eliminability, or the supervenience of classical concepts on those from quantum mechanics, nor the reverse.

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