Introduction

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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]

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

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[1] This is not because I am a realist (or for that, 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.

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 judgments 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 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.