Reply

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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 then 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 $\underline{m}$ brings with it a capacity of magnitude Gm/r^2 to cause another mass a distance $\underline{r}$ 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;

[1] Hopkins, "Pied Beauty".

[2] Perhaps when Paul says, citing p. 81, 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".

and there are also outcomes it *can contribute* to – such as holding a feather in midair, in conjunction with a vigorous updraft.

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 "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 metaphysicist'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 characterize 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 views that laws associate capacities

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[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 circumstance, or with 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.

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 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 metaprinciples that tell when a capacity operates and further metaprinciples (principles of compositions) 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 characterization 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 takeovers 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 program in biology,

[5] Plus of course some cost/benefit analysis.

evolutionary psychology and game theory. These all, I believe, get disproportionate attention and funding just because of their promise to be universal.[6]

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 $\underline{r}$ from another mass of magnitude $\underline{m}$; we can employ $\varepsilon q/r^2$ only to a charged object located a distance $\underline{r}$ from a charge of magnitude $\underline{q}$; 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 "idealizing 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

[6] We do here, of course, also have to allow for human competences. 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.

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 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 "idealized" 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 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 counterfactuals". Then the truthmaker 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

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[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 $f = ma$.

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 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 be 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 metaprinciples of composition to be 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 to 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.

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, 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.