Causal powers: Why Humeans can't even be instrumentalists

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What this paper aims to do

What makes a powers ontology, a *powers* ontology? Many answers are possible. One that matters to thinking about science – at least any science where the analytic method is employed – is that a powers ontology allows not just for powers and the overall results that happen when they act, but for a third kind of item as well intermediate between these two: the exercising of the power, or what I have called its *contribution*. Hume urged that there is no difference between the obtaining of a power and its exercise; others, that there is no difference between its exercise and the overall result that occurs. This paper will rehearse the reasons, based in the success of the analytic method in a variety of sciences (and often in daily life), for taking exercisings to be real and separate from both the obtaining (along perhaps with triggering if needed) of a power and the overall result.

But the central aim is not to defend this view. Rather I aim here to show how the need for exercisings makes problems for the Mill-Ramsey-Lewis (*MRL*) view of laws, which is deeply rooted in the assumption that nature is devoid of powers, causings, necessities, potentialities, and anything else we might try to refer to using modal concepts. If exercisings are real, I shall argue, then the MLR view of laws is in trouble, at least if laws are going to cover much of what happens, since exercisings of powers surely should not be admissible into any ontology the view allows. One might hope to rescue the MRL account by retaining its central demand that laws cover as much as possible as simply as possible but give up the requirement that they be true. In that case laws could involve exercisings but

not as true features of the world, rather as part of an instrument for deriving the kinds of facts that the view lets in to its ontology.

This paper adumbrates this proposal and argues that it will not work. If laws are to cover much of what we think they do, we will have to have exercisings in our ontology, and, the paper argues (contrary to a proposal floated by Richard Corry), that we need both exercisings and powers. So we had best accept that powers and their contributions have full citizenship in the world that science presents to us.

I begin by explaining the role of exercisings in the analytic method which is typical of many sciences, especially much of modern physics and economics. The analytic method is two-stage. First, identify how powers exercise when they operate 'on their own' (in my vocabulary, this is to identify their canonical *contribution*). Second, derive what the overall results will be in a given situation by the use of some rule for calculating what happens when the powers that operate in that situation are exercised together. Then I shall turn to the MRL account of laws.

Contributions: Why we need them

Let us begin by considering the paradigm where contributions have been seen to enter since at least JS Mill onwards: mechanics and its force functions. I am going to take a caricature of mechanics for illustration, supposing that there are only two kinds of powers that affect accelerations: gravitational and Coulomb. As I shall describe more fully in the next section, the MRL view is concerned with what is called 'the Humean mosaic' (*HM*), which consists of all the facts that are in some special category taken to be unproblematic by contrast with facts involving powers, causes, necessities and possibilities. Here are the principles of my caricature theory (CT):

Caricature Theory (CT)

LoG ('law' of gravity): An object located a distance **r** from a system of mass M experiences a contribution to its acceleration $a_G = GM/\mathbf{r}^2$.

CL (Coulomb's 'law'): An object of charge q_1 located a distance **r** from a system of charge q_2 experiences a contribution to its acceleration $a_C = \varepsilon q_1 q_2/\mathbf{r}^2$.

LoC ('Law' of composition): The total acceleration an object undergoes = vector sum of all the contributions to its acceleration.

I maintain that LoG and CL are not descriptions of regularities in the HM because component accelerations should not be in the HM. Component accelerations are rather the contributions that result when the causal powers associated with mass M and charge q_2 are successfully exercised. There are thus (at least) three kinds of items in the ontology of this theory: causal powers, the contribution they make when successfully exercised, and the overall result, which we may assume to be a feature in the HM – here the total acceleration. This, recall, is one of the chief objections Hume had to causal powers: he thought contributions were not legitimate. There is no distinction between the presence of a power and its exercise.

There has been a lot of discussion about whether component accelerations or component forces can be admitted into the HM along with total accelerations and total forces. So perhaps it is a tactical error to use this example. I do so because it is such a familiar one. But it is far from special. Science is rife with contributions. They appear wherever we employ the analytic method. Here for instance is just one of a great many equations from economics I could cite, this from a paper by Nobel-prize-winning labour market expert Christopher Pissarides. Let V denote the value of a vacancy; J, the value of an occupied job; A, the product per worker; κA, the cost of holding a vacancy; q, the transition rate of a typical vacancy; r, a discount factor; and b, the income of the unemployed worker.

Pissarides tells us: 'The Bellman equations giving their values are,

$$rV = -\kappa A + q(J - V) + \dot{V}$$

$$rJ = A - w - \lambda J + \dot{J}.'$$

He also tells us: 'The wage rate is assumed to be a weighted average of the unemployed worker's income and the output per person:

$$w = (1 - \beta 0)b + \beta 0 A, \beta 0 \in (0, 1).'$$[1]

In this case no-one would think that the value of a vacancy has three distinct pieces, each there in the same way that the value of the vacancy is. Nor that there are two proper pieces of wage rate that belong in the HM along with the actual wage rate.

Kevin Hoover in his extended study *Causality in Macroeconomics*[2] backs up this point, illustrating with a mechanical example:

> A gear that forms a part of the differential in a car transmission may have the capacity to transmit rotary motion from one axis to another perpendicular to it. The capacity of the differential to transmit the rotation of the engine to the rotation of the wheels at possibly different speeds is a consequence of the capacities of the gear and other parts of the differential. The organization of the differential cannot be represented as an adding up of influences nor is the manner in which the gear manifests its capacity in the context of the differential necessarily the same as the

[1] Unemployment and Hours of Work: The North Atlantic Divide Revisited, Christopher A Pissarides, 2006
London School of Economics ms.

[2] ref

manner in which it manifests it in the drill press or in some other machine ... (*p.*55f)

Perhaps it is so easy for certain contributions to slip in to our census of the HM because we tend to focus on rules of composition like LoC above, involving some kind of addition, so we fall into thinking of the contributions as being like stones in a dry-stone wall: Of course both the stones and the wall are there since the wall is nothing but the stack of stones. That's why I should like to stress that the vector addition of LoC is very different from simple addition and to cite Pissarides or Hoover, or the rules for calculating the flow of a current through a circuit. Capacitors contribute capacitance; resistors, resistance; and inductors, inductance. There's a formula for calculating the overall current in a simple circuit, but it's not as if these three contributions add up to it. For complicated circuits we proceed in steps. There are a series of rules that allow the reduction of a complicated circuit to a representative simple circuit; then the first formula comes into play.

A powers ontology

I have for a long time, along with other authors in this volume, been arguing that causal powers are not migrant workers that need a special permit for admission to our ontology. Nor are they isolated individuals. They come with retinues: families, attendants, friends, enemies, and co-workers. Some causal powers need to be triggered if they are to be exercised; some may need facilitating even once triggered; some can be inhibited so the contribution they produce is diminished or distorted; and even, if famous examples by metaphysicians are correct, an interference may stop them from being exercised although all the other conditions are right. So exactly what must be in place for the power to be exercised and the contribution to appear varies from case to case. Moreover some powers are deterministic – they are always exercised when the conditions are right; some are probabilistic – they are exercised with a fixed probability; some may be erratic – sometimes they are exercised and sometimes not, but there's no fixed probability to it.

What really matters for the philosophy of science is to admit the three distinct categories: powers, their contributions or canonical exercisings, and the overall result

that happens when they exercise. This can be consistent with different metaphysical accounts of what powers are, many of which will be represented in this volume. I have tried to develop the basics of an account that makes immediate sense of what I see happening in scientific practice. With that end in mind I identify powers by their canonical contributions. Different canonical contributions, different powers. This means that, like John Pemberton,[3] I am committed to a very great many causal powers indeed. The Scientific Revolutionaries mocked Aristotelian natural philosophy for multiplying powers in this way. But just where the bite is in that? Many philosophers of science take parsimony to be an epistemic virtue. But I would urge that we heed the arguments of Larry Laudan that none of those things usually called 'epsitemic virtues' in philosophy of science are knowledge conducive.[4] Of course they may be virtues, but in that case there are a great many other virtues that matter, and many often matter more than parsimony, as Helen Longino argues when she offers a list that includes novelty, ontological heterogeneity, mutuality of interaction, applicability to human needs and diffusion or decentralization of power.[5]

Nor need we be troubled by the Moliere-type scorn of the scientific revolutionaries: 'What makes heavy bodies fall?' 'Gravity!' 'And what is gravity?' Well, in part, I answer, just as they scoffed, 'that which makes heavy bodies fall.' This tight circle of explanation and identification does not make science ridiculous, or useless. Heavy bodies fall in part because the earth has a power of gravitational attraction, a power of a very specific kind, and gravitational attraction is, in part, the power to contribute a force of a very specific strength and direction. We mustn't confuse explanation with the ability to predict and to mould the world to our purposes. The advance of mechanics did not rest in breaking this explanatory circle but rather in a variety of key scientific tasks:

T1. Learning to identify what the contributions are. (In the case of gravity, $a_G = GM/r^2$).

T2. Learning how to recognize when the causal power is there. For instance, it seems an object with a mass (M) always has the power of gravitational attraction. My recent

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buying experience suggests that it takes a great deal more knowledge than I have to tell if a dishwasher really has the power to get ordinary dinner dishes clean. But even I can tell what to plant in order to grow daffodils rather than oak trees.

T3. Learning the right conditions for the power to be successful in producing the contribution, and whether the production will be deterministic, probabilistic or totally chancy. This includes learning to recognize interferences when we see them even though the theory does not provide a catalogue of them.

T4. Learning other powers that will also make contributions to the outcomes we care about.

T5. Learning rules of composition.

T6. Learning telltale signs that other factors than the contributions covered in our theories are present that may influence the outcomes in question.

That's enough about ontology. It is now time to turn to the MRL view.

Mill-Ramsey-Lewis: An objection and a rescue

In the hands of most of its defenders the MRL view has the exact opposite aim to the aim of this paper and many in this volume. We aim to defend powers and their retinue. MRL, to eliminate them, and all else that smacks of modality, like causes, necessity, potentiality, and possibility. There are just the facts, no causal or necessitating relations among them, nothing that makes them hold, no reason they hold; just the facts in the order and arrangements in which they actually occur. What then of the *laws* of nature? According to my colleagues Craig Callender and Jonathan Cohen, 'The heart of the MRL approach to lawhood is to say that a true generalization is a law if and only if it is an axiom of all the "Best Systems" — axiomatic systematizations that best balance strength and simplicity.'[6] Strength and simplicity in deriving the facts.

[6] Craig Callender and Jonathan Cohen, 'Special Sciences, Conspiracy and the Better Best System Account of Lawhood', UCSD ms, 2011

Of course if the views defended in this volume are correct, powers and their exercisings are actual. They will be among the facts so the MRL view will not get off the ground. But the MRL view presupposes that all the facts there actually are fall into some special kind. They are just those that appear in the HM. So let me begin by explaining some doubts about the very idea of a HM, which arise from the fact that it is generally not very clear what is supposed to be in this mosaic and why. I know that it is supposed to exclude most of what I see in the world around me, happenings that fall in the philosophical category of singular causings: pushings, pullings, smotherings, boostings, insultings, encouragings, and indefinitely many more. I take it that it is also meant to exclude causal powers and everything that falls into the more abstract categories connected with them, categories that are so essential to the way I – and much of modern science – understand and manage the world around me, categories like *interfere, inhibit, facilitate,* and *trigger*.

There are three kinds of reasons for worry about the idea of the HM: 1. The difficulty of identifying reasonable criteria for sorting what is supposed to be in from what is out. 2. The difficulty of identifying the special characteristics that are supposed to give whatever these features are their ontological privilege. Why are they to be regarded as the native inhabitants whereas the features that live in my neighbourhood have to earn their right to live here? 3. The usual candidates do not have any higher epistemic status than many facts about causal powers. Many of the features that those who employ the concept of the HM let in are from high physics, like the value of metric tensor at space-time points. Suppose we take observability as a mark of high epistemic status. These certainly aren't observable. And even the features that Hume claimed to have impressions of are no more observable than the lapping up of the milk by G.E.M. Anscombe's cat.[7] Nor are they any more readily measurable than capacities or causal powers – that was the point of the title of my book: *Nature's Capacities* ***and their Measurement****,* where there are extended discussions of how to measure powers. Nor are we generally justified in being more certain about them. It is far easier to be mistaken about what values physics quantities take on than about whether the cat is

[7] Ref to her inaugural lecture

lapping up the milk; and we know we can also be mistaken about even the most immediate sensations, like pains. Finally, the concepts that pick out the features that the Humean border guards are loathe to let in are no more unintelligible, no harder to define, than those they prefer. They are only harder to define if you disallow all the concepts one would employ in characterizing them, just as it is difficult to define 'triangle' if the concept of a straight line is forbidden. 4. I think the Humean border guards cheat, as I shall explain in more detail in Section X. They often let in lots and lots of powers, contributions, interferences and the like. They just give them names that sound ok.

Despite misgivings about the very idea, in order to pursue other concerns about the MRL view in a powerless world, I shall assume from here on that we do have some clear sense of what the HM consists in, and whatever that involves, it disallows powers and the whole retinue that accompanies them, like contributions, exercisings, interferences and enhancers. With Dr. Zeus's Sneeches in mind,[8] I propose to call the facts that are supposed to be in the HM, whatsoever they are, *gold star* facts.

In discussing the MRL view, I turn to the Callender and Cohen (*C&C*) account of it because theirs sidesteps some well-known difficulties. First, although they follow the convention of talking about a balance of coverage and simplicity, they acknowledge that there may be other virtues that one might demand from a set of laws as well or instead, certain mathematical constraints perhaps, or fruitfulness or maybe some of the virtues that Longino describes, like heterogeneity and social usefulness.

The major C&C addition, though, is to make the best system relative to a 'distinguished set of kinds', where what will be the best axiomatic systemization of one set of kinds will not be the best for all others. This solves two problems in one fell swoop. First it acknowledges that what is a simple axiom system depends on the choice of basic kinds: axioms about green and blue things look very complicated if grue and bleen are

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taken as basic kinds. Second, it allows laws to the special sciences, that is laws 'in terms of kinds that cannot be understood as the fundamental natural kinds and that couldn't be laws when written in terms of the fundamental natural kinds.' This is important because Callender and Cohen, like me, 'don't put much stock' in the strategy of capturing special science generalizations as corollaries entailed by statements of regularities involving 'fundamental natural kinds'.[9] I naturally think this is a big step forward because it fits more closely with how we actually make correct predictions in science, technology and daily life.

Though they make this particular improvement in the MRL view, C&C maintain a further standard requirement that seems to me unmotivated from a 'Humean' point of view: the requirement that the axiom system should itself consist of true generalizations. The HM is supposed to contain the facts and nothing but the facts. As Helen Beebee puts it, laws are supposed to provide for 'particular matters of fact that obtain' [10] (p 572). It is an additional piece of metaphysics to suppose that there are many true generalizations about the facts in the HM let alone that these true generalizations will encompass the mass of matters of fact that our sciences can predict. If I am right, they will not. I have just been arguing that the analytic sciences make heavy use of laws about contributions in order to derive gold star facts; and laws about contributions do not report regularities in the HM. Nor do laws of composition, which are equally essential to the derivations. So I think the requirement that laws be true generalizations is too restrictive. If we keep it, the best theory by MRL standards in the domains where the analytic method is employed successfully will likely be a very poor one.

The MRL view has many well-known defences and there are many well-known objections to it, including the two that the C&C version avoids. I want to raise three difficulties that arise specifically from the assumption that the world consists entirely of

[9] pg ref to c&c

[10] *Philosophy and Phenomenological Research* 56 (2000), pp. 571-94. Reprinted in J.W. Carroll (ed.), *Readings in Laws of Nature* (Pittsburgh: Pittsburgh University Press 2004), pp.250-76

gold star facts and no others, particularly not facts involving powers and their retinue. These problems are general but to make them easier to see I shall set them in the context of my caricature theory CT. Imagine a simple pretend world, *W*, that consists of situations consisting only of charged compact masses behaving in accord with CT. I take it that an overall acceleration is a gold star feature, as are charge mass and separation. But the power of gravity, the power of Coulomb repulsion and attraction, and their separate contributions to overall acceleration are not. So none of the 'laws' in CT state regularities in the HM. This is unsatisfactory for three related reasons.

1. The results we get when we use CT are 100% reliable, and they are not lucky guesses. They rely on a system that works, and works all the time, for predicting accelerations. Yet they cannot count as laws on the MRL view.

2. Are there any regularities in W? There are of course counterfactual regularities: one for every possible arrangement of masses and charges as input and the acceleration of each particle as output. But counterfactual regularities are not regularities in the HM. So whether there are any depends on what arrangements actually occur and with what frequency. Imagine that one arrangement, β, which results in the set of accelerations A, occurs twice and all the others only once in the history of W. Is the true claim 'Whenever β, then A' to count as a law? That looks to depend on how the balance of strength and simplicity is made. But this is odd since all these arrangements are much the same except for the accident of one having a doppelganger.[11]

3. The issue about which, if any, of the regularities that hold are to be counted as laws raises another issue that frequently comes up in discussions of MRL. It is generally supposed that if it is to be an adequate account of laws, the MRL view should deliver something like the items we write down in our best theories. But then, across a wide variety of scientific areas where the analytic method has proved successful – those like

[11] It seems especially odd from the point of view of CT since, from that point of view, they all hold for the same reasons – because of the laws of CT. Following the warnings of Helen Beebee [XXX] though we must be careful how much force we give this consideration. The MRL view stars out from the assumption that there are just the gold star facts and any patterns they happen to have. Laws just summarize that efficiently. So starting from an MRL point of view, whatever regularities there are do not hold for the same reasons since there are no reasons that regularities hold. They just do.

physics, economics, and engineering – that means claims like those in CT, which are precluded by MRL.

How damaging are these objections? That will depend on more fundamental views about laws, for instance, that laws of nature are what support the successful practices of the sciences, so that our account should admit as laws enough to make sense of why the practices that work so well can do so. When it comes to meeting that demand, it seems these three objections make real trouble for MRL in any field that successfully employs the analytic method.

There are though two simple changes that can fix these problems and still allow that laws are the best way to encode the facts, deriving as many as possible in the simplest way. First is to give up on the demand for only gold star facts and allow power related facts into the world, yet still demand that laws express true generalizations that best balance simplicity and coverage of the facts. The second is to admit only gold star facts but to give up on the demand that laws express true generalizations. Rather take laws to be the best way to systematize the facts by allowing for their derivation, best in the sense of providing most coverage in the simplest way.

This latter seems a reasonable strategy to try from a 'Humean' point of view because the requirement that the laws be true is at any rate hard to motivate from a point of view in which there is no governance of Nature, just one event after another after another – and C&C themselves stress that MRL is a non-governing view – indeed, the best of these.[12] From a governance point of view it is easy to see why laws have to be true (though not why they have to be reports of regularities). But what is the reason either for truth or for supposing that theories must consist of regularity claims if theories are just ways of organizing gold star events, or of predicting some from others? I don't mean to suggest that they should be false but rather that good devices for organizing the facts

[12] In 'A better best system account of lawhood'

and providing for their derivation need not be candidates for truth at all. As Helen Beebee warns, we must be careful not to import into the reasoning of 'Humeans' demands and assumptions that sit uncomfortably with the basic ideas of the 'Humean' point of view.[13] So I propose on behalf of the MRL view a more instrumentalist stance. Theories are there to derive gold star facts from other gold star facts. How they do so is open. The best theories are the ones that predict the most in the simplest way.[14] That's that.

The instrument in the case of our caricature mechanics theory CT would work like this. To predict the acceleration of a system in a given situation, look to see the arrangement of masses and charges in that situation. Use LoG and CL to write down functions of the form GMm/r^2 and $\varepsilon q_1 q_2/r^2$ for all the pairs. Predict as the acceleration of the system in that situation the vector sum of these divided by the mass of the system. That is, do just what we do, but don't make any claims about LoG and CL being true generalisations. They are just part of an efficient instrument for prediction.[15]

Causal powers and their markers

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[14] Of course there will always be the standard problems of how to measure breadth and what counts as 'simple'.

[15] Callender and Cohen think we do not need talk of causal powers, so presumably they will feel that my rescue attempt is not needed in the first place. I find two specific claims they make in aid of their avoidance of causal powers. 1. Once MRL admits relativization to different sets of kinds, special science laws can be admitted. So that motivation for causal powers disappears. I don't see how that saves the day, though, since laws in the special sciences are not true generalizations – unless we take them to be true generalizations involving the successful exercise of a power and the obtaining of its contribution. And anyway, this is no special problem for the special sciences: it is a problem wherever the analytic method is employed. (Harvey Brown has argued, in conversation, that that's okay since basic physics – space-time theory, general theory of relativity – does not employ the analytic method. I take it that this is in line with the view of John Earman and others that basic physics is what's really true and basic physics simply assumes values of certain quantities at space-time points – quantities that are acceptable from a 'Humean point of view'. I of course dispute that basic physics can predict, even in principle, most of what even I can predict in practice.) 2. 'True generalizations' need not be true reports of exceptionless regularities. After all, many generic claims – 'Bees sting', 'Frenchmen eat horsemeat' – are *true* but do not report exceptionless regularities. I am not optimistic about their hope here for two reasons. First, many of these are true, I maintain, because they are correct ascriptions of causal powers, and this will include a very great many of the laws of the special sciences. Second, I suspect that the appropriate semantics for many others will also involve features not in the HM.

I don't in the end think this will work though. The reason is familiar: Individual causal powers, their triggers, interferers, inhibitors and contributions – the whole retinue of powers-related items that matter to the production of an outcome – do not in general reduce to gold star facts, and we need these as inputs if we are to hope to get the right outputs using our instrument for prediction. The concern is the same as Wilfrid Sellars' worries about the manifest, as opposed to the scientific, image. Some predictions can be based entirely on facts in the manifest image but others require facts from the scientific image to be added in. All people will die but only those with a certain genetic structure will be subject to Tay-Sachs disease. If we insist on looking only at facts in the manifest image, we will lose a lot of predictive capacity, and especially the ability to make the bulk of the very precise derivations we depend on in modern life. Similarly, I argue, we lose huge predictive capacity if we refuse to look at features that fall into the categories picked out by power concepts.

Too much focus on the simple world of my simple mechanical world W may obscure this. The only things in that world are particles and the only features that can be relevant to the targeted outcome, acceleration, in that world are all covered by my caricature theory: mass, charge, separation. This simple pretend world is far from the blousy reality in which we actually live, where the wind blows objects about and charges that are at first moving in readily calculable trajectories, say at the Stanford Linear accelerator, can get unpredictably disrupted by small earthquakes. In particular the example makes a number of special assumptions:

A1. A power of a given type occurs if and only if some specific kind of gold star feature occurs. For example, the power to produce a contribution of size GM/r^2 to the acceleration of an object occurs in systems that have mass M and are situated at a distance r from the object.

A2. Nothing can interfere with the exercise of a power once it is present and properly triggered.

A3. There is a law of combination for all contributions.

A4. Nothing affects the outcome except the exercisings of the powers listed.

These all matter because we are trying to use our theory as an instrument that directs us how to start with only gold star features as input to arrive at different gold star features as output. We can use whatever graphic language we want to describe the instrument, including talk of powers and the like, but we must be sure to restrict the input to facts involving only gold star features. Using the analytic method in the conventional way, it looks as if our theories lead us to proceed like this:

> *Chain of Inference*: Gold star input (like mass, charge) --> *powers* (like gravity, Coulomb attraction and repulsion) and *triggers* --> *contributions* (like GM/r^2, $\varepsilon q_1 q_2/r^2 m$) --> *composed contribution* (like the vector sum of the contributions listed) --> gold star output (like the actual acceleration).

On an instrumentalist interpretation, the theory provides instructions for how to proceed from each step to the next. The italics show concepts in which the instructions are couched. The instructions at step 1 might look like this: "If a system has mass M and is separated from another by **r** then write down 'the system has the *power* of gravity'; if it has charge q, write down that it has the *power* of Coulomb attraction or repulsion." In our case there are no instructions for moving from gold star inputs to triggers since we suppose that neither gravity nor Coulomb attraction and repulsion require triggers in order to be exercised. Step 2, represented in the second arrow, might look like this: "If you have written down that the power of gravity obtains, then write down as a *contribution* to the acceleration of any object a distance **r** away, $GM/\mathbf{r}^2$. If you have written down that a Coulomb power obtains then write down $\varepsilon q_1 q_2/\mathbf{r}^2 m$ as a *contribution* to the acceleration of any object a distance **r** away. Step 3, represented in the third arrow, says to vector add these contributions to the acceleration of each object. And Step 4 says to predict the result of the vector addition as the final acceleration of the objects.

We will end up deriving a correct acceleration if each step is reliable, which will depend on each arrow being secure. The first arrow is secured by A1, the second by A2, the third by A3, and the fourth by A4. The problem is that for many cases, at least one of

the arrows will fail to be secure unless we input not just gold star features, but also facts about powers and their retinues. Powers do not function as just a turn of phrase used in the instructions. They have to be there in the facts that we input at the very start or our derivations will be not lead us to true results.

The problem is not though that we make mistakes in predicting. Surely that is ineliminable in science. It is that we do so unnecessarily if we do not allow ourselves to input facts about causal powers and how they operate. And we also lose one of the central guides we have about when not to make predictions.

You can find a number of possible examples of this latter in the metaphysics literature on conditional analyses of dispositions, which is rich in proposed counterexamples to A2. If those examples work, they provide cases where the power is in place and properly triggered but the canonical contribution is not produced. The second arrow in our inference chain is broken. The point is that we can often tell that there is something in the situation that will interfere with the exercise of the power, and if we write that down, our instructions should tell us that we are not in a good position to predict outcomes. But we are not allowed to write it down if we restrict our input facts to gold star facts.

We could write it down within Humean bounds if every interference has a gold star base without which it cannot occur AND if our theory has instructions that recognize those and say 'Don't predict if this occurs.' But I don't think we have very strong reason to think the first is true; as to the second, inspection of our most successful theories shows how hard these are to find and articulate. But doing so matters or we will make wrong predictions we could avoid. That is why I include T6 among the central tasks of science that make power knowledge useful: 'T6. Learning telltale signs that other factors than the contributions covered in our theories are present that may influence the outcomes in question.'

We have exactly the same sort of problem in philosophy of science with A1, which I want to look at it in more detail since it has had far less attention in the powers/dispositions literature.

If A1 is true then for each power (like gravity, G), there is some gold star feature (like mass, M) that is a certain mark that that power (G) occurs. Many will suppose that there must be such gold star features because powers need a causal base. I don't know good arguments for this. One doesn't have to go so far as some powers advocate in saying that gold star features are nothing but clusters of powers in order to think, as I do, that they are at least as occurrent as whatever the HM is populated with.

Of course causal powers are not much use to us if we do not have some way of figuring out that they are there short of waiting for them to make their contribution. As I indicated in T2, finding markers for them is one of the important tasks of science: witness the two laws LoG and CL or the experts' ability to tell a nasturtium seed by looking. Sometimes the markers will be substructures, and sometimes we don't find them but build them.

You can see one of my favourite examples pictured in Cartwright and Hardie 2012. I have a wonderful pencil sharpener, designed by Rube Goldberg. You don't turn a crank to sharpen the pencil but rather fly a kite. Flying a kite does not usually have the power to sharpen pencils. But you can see a clear set of markers for this causal power there in the Rube Goldberg design: The kite is attached to a string that goes under one pulley and over a second and is tied at the opposite end to a small easily sliding door of a cage containing moths. When the door lifts, the moths fly out and eat flannel lying on a scale, and so on till a rotating knife eventually sharpens the pencil.

I use this example to underline that many causal powers are derivative. They are there because something else is, often something we would think of as a substructure. And often the substructure not only ensures the presence of the causal power but can

provide a marker for us that it is there. That however does not imply that the substructure that guarantees the presence of the causal power can be sufficiently characterized in terms of gold star features.

Consider the Rube Goldberg pencil sharpener. An essential part of the sharpening apparatus is the pair of pulleys that the kite string passes under and over. The Rube Goldberg machine does not have the power to sharpen pencils by flying kites without these pulleys – and certain features of them. Most notably that they have the power to ensure that the range of downward forces exerted by normal kite flyings produces an upward contribution to the force on the little door larger than the net effect of all the downward pulling contributions on the door. That is not a gold star feature. There are gold star features that are markers for this next-level-down power: there's the double pulley set-up; the wheels are nylon; the rope is galvanized wire; the slides on the little door are oiled; the little door is made of bamboo; the rope is firmly attached at both ends; I live in a very windy neighbourhood; etc. But – here again a familiar point – any list like this is defeasible. These are markers for the causal power, not a set of features that constitute it nor a set that is either necessary or sufficient for the causal power to obtain.

Recall among my initial concerns about the HM my complaint that I feel that 'Humeans' often cheat. They suppose that because a term appears in proper science or technology, because it is well-understood, because it is regularly employed, and because it figures in successful prediction that it is not a causal power term. Names of simple machines, like 'pulley' and 'lever', are a prime example. A lever is something that, when balanced on a fulcrum, has the causal power to contribute an upward force F on an object a distance D from the fulcrum of size F = F'D'/D when a total downward force of F' is exerted a distance D' on the other side of the fulcrum. And to the extent that there are true regularities in the offing they are of the form, 'A lever will contribute an upward force on an object a distance D from the fulcrum of F = F'D'/D when a total

downward force of F' is exerted a distance D' on the other side of the fulcrum if this causal power is successfully executed.'

In cases where assumptions analogous to A1 do hold, there is a temptation to take the gold star features that are sufficient for the causal power to hold as what constitutes the power. It is in this context that Richard Corry suggests, if I understand him, that, once we have secured a place for contributions, we might be able slim down the powers ontology by eliminating the causal power and making due just with the gold star features that are sufficient for it.[16] But that seems to require that causal powers can be reduced away to gold star features. I hope I filled in enough of an argument to show that this strategy won't work for the Rube Goldberg machine, and this machine may be unusual, but with respect to the connection between powers and the gold star features that might mark them, it is not atypical. Corry's proposal also supposes that A2 can be relied on. So inferring contributions directly from gold star markers for powers, without putting the step through powers in between, coalesces two arrows in the Chain of Inference for the analytic method into one; correlatively, it conceals two places where the move from gold star facts to the presence of a contribution can fail. If we ignore these two, we will be in danger of making predictions that won't be borne out.

Moreover, I am not at all sure that it works for classical mechanics, from which my caricature is drawn. The rules that take you from the gold star descriptions to causal

[16] Corry says, 'One indication of the difference between Cartwright's position and the one I am advocating here can be seen by noting that nothing in my argument requires level (1) of Cartwright's hierarchy. Recall that level (1) contains dispositional properties, including dispositions to produce causal influences. We should only posit level (1) as a distinct, irreducible ontological category if we think that these dispositions are irreducibly dispositional in the way that Ellis and Lierse describe. But nothing in our discussion so far requires this. In particular, given the existence of influences at level (2), one could choose to analyse the disposition 'to have a causal influence of type *X* in circumstances *C*' in terms of a conditional just as Humeans do—the only difference being that the conditional will mention elements at levels (2) and (3) instead of just the Humean level (3). If we take this route, we can do without the kinds of thing that Cartwright, Ellis and Lierse all seem to agree are necessary: irreducible causal powers, capacities and the like. Now I am not suggesting that we should take this route—I am personally rather fond of causal powers, and there may be other good reasons for endorsing them—my point is that nobody seems to have noticed this possibility. [Toby Handfield Chap06. P 180]

powers are what used to be called ‘bridge principles’. Do we really want to assume that every time there is a contribution to acceleration, that there is a source of that, a force, of a type that appears at the front end of a bridge principle? A 100% association between that specific contribution and some gold star features that allow us to write down just the right force function. Must there be something else than the power or force itself without which a force cannot obtain? Surely robust scientific realists should resist this assumption. It is just a special case of reducing a theoretical feature to other ones deemed more acceptable. A robust realism would suggest that forces themselves are real since they are the heart of the theory. They are not simply a shorthand way of talking about something else.

This probably brings to mind issues of supervenience. Do theoretical features supervene on observational ones? Do causal powers and their relatives supervene on gold star features? I have never seen convincing arguments that they do, in either case. And supervenience carries the wrong suggestions: that the causal action is at the base level, with features at the higher level accommodating to that. But as a realist about both theoretical features and about causal powers, I take it that just the reverse is the case. Heavy bodies fall because they are subject to the power of gravitational attraction. Even if the force of gravity does supervene on the presence of the earth, the laws of nature are written in terms of the posers we call forces. At any rate we do not need to take a view here on supervenience because it is not enough to rescue the MRL view. So let’s return in conclusion to my instrumentalist rescue attempt.

Why instrumentalism will not rescue MRL

I say that supervenience is not enough for the instrumentalist recue of MRL. What’s needed is something stronger: real bridge principles. Every occurrence of a causal power needs not just a supervenience base in the HM. That base must also fall under some kind which is linked by a bridge principle with that kind of causal power, so that

whenever that kind of gold star feature occurs, so too does the related kind of causal power.

Why? Because the theory is to supposed to provide an instrument for deriving gold star facts from other gold star facts, and nothing more than other gold star facts. Suppose that, contrary to my worries, power features do after all always supervene on some gold star features or other. That means that every situation fixes something that 'should be' written down as input in order to get the process of prediction under way. But that's of no use if there is no device in the instrument that dictates what this is. There has to be a rule available in the instrument that describes these gold star features and instructs us what to do next when we see them. But if my claims have been correct, those are just the kinds of rules we don't have nearly enough of. Powers generally do not reduce to gold star features. And at any rate it is contributions we need for our analytic-method calculation, and they depend not just on the presence of the power but on the right facts about triggers, interferences, masks and the like, none of which is generally reducible to the gold star features allowed in the HM.

Conclusion

My conclusion is a mere simple summary in 2 sentences of what has come before:

I see no way to rescue MRL from admitting causal powers and their relatives if the scientific theories they reconstruct are to be of serious use, even just for predicting results in the HM. And anyway, there's nothing wrong with the causal power family to begin with.