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In Favour of Laws that Are Not Ceteris Paribus *After All*

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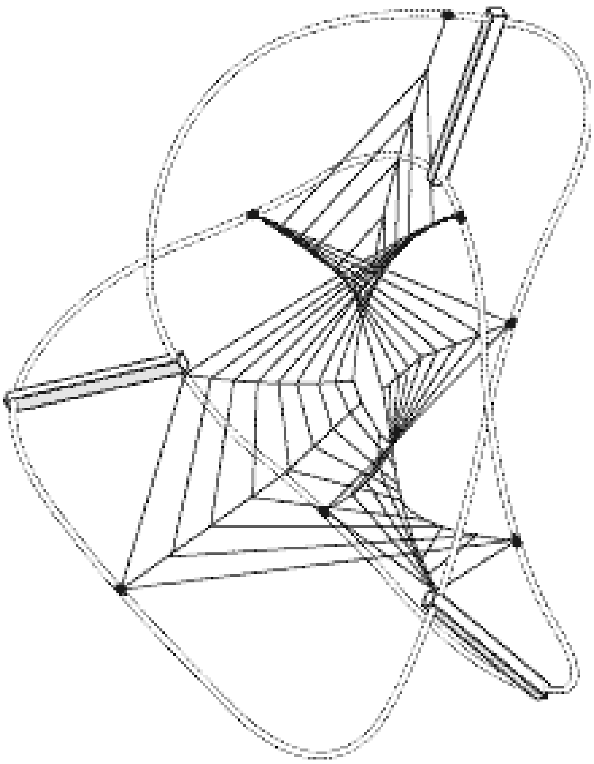

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In Favour of Laws That Are Not *Ceteris Paribus* After All

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Abstract

Opponents of *ceteris paribus* laws are apt to complain that the laws are vague and untestable. I argue that these kinds of claims rely on too narrow a view about what kinds of concepts we can and do regularly use in successful sciences and on too optimistic a view about the extent of application of even our most successful non-*ceteris paribus* laws.

The too narrow view supposes that we cannot talk in science about causal powers and their operations, and what can interfere with those operations. I argue that, to the contrary, use of causal power concepts is both common and legitimate in science. The too optimistic view supposes that theory can in principle always provide us with non-causal concepts to cash out the concept of interference. I argue that our best evidence suggest that that is wishful thinking.

When it comes to testing, we test *ceteris paribus* laws in *exactly the same way* that we test laws without the *ceteris paribus* antecedent. But at least when the *ceteris paribus* antecedent is there we have an explicit acknowledgment of important procedures we must take in the design of experiments—*i.e.*, procedures to control for "all interferences", even those we cannot identify under the concepts of any known theory.

1 INTRODUCTION

I am generally taken to be an advocate of *ceteris paribus* laws throughout the sciences, even in physics. But what are *ceteris paribus* laws? According to John Earman, John Roberts and Sheldon Smith, "The distinctive feature of *ceteris paribus* laws is that they do not entail any strict or statistical regularities in the course of events. Nor do they entail any predictions, categorical or probabilistic."[1] Earman, Roberts and Smith also suppose that a *ceteris paribus* law is not "explicit about what precise conditions have to obtain for the regularity after the ceteris paribus clause to hold"; alternatively that the *ceteris paribus* clause is "vague" and cannot be stated 'in a precise form" or "a precise and closed form."

If that's what it takes, then what I have defended are not *ceteris paribus* laws.[2] The laws I talk about either can be stated in precise and closed form or they entail strict or statistical regularities in the course of events or both. The matter hinges, of course, on what we take to constitute a "precise and closed" description. This returns us to the old issue of how we should police the language of science. I suspect that I am far less strict about what is admissible as a description than are Earman, Roberts and Smith. My reason is that I find that the less restrictive language is the kind of language that is regularly employed in exact science; and that attempts to reconstruct this language away produce scientific claims that are at odds both with the ways we test our scientific theories and with the ways we put them to use.

There are two kinds of formulation that I use to reconstruct scientific laws that I think Earman, Roberts and Smith would reject, the first because of the language it uses, the second because of the limitations it supposes on the descriptive power of theory. I shall discuss each in turn in sections 2 and 3. In section 4 I shall take up the issue of testing. What I say about testing will not only defend the kinds of laws I discuss but also *ceteris paribus* laws as more generally conceived. Section 5 answers some criticisms that Earman, Roberts and Smith make in this volume against a connection I trace between induction and capacity.

[1] Earman *et al.* forthcoming; see also the passage where they advocate that "nature is governed by laws and that these laws entail strict regularities that are true throughout space-time".

[2] I do not mean to imply that I am opposed to them; simply that they are not the kinds of laws I have been thinking about and defending over the last decade.

2 Powers vs. *Ceteris Paribus* Laws

The language I use in reconstructing a number of scientific laws in the exact sciences (most notably physics and economics) is the language of *powers*, *capacities* or *natures* and related concepts such as *interfere*, *inhibit*, *facilitate* and *trigger*. Those of us raised in the joint shadow of the Vienna Circle and British Empiricism were taught that these kinds of concepts must not appear in science. I was puzzled about this from early on since it seemed to me that many of the most important concepts I learned in physics are power or capacity concepts, force being the *first*, simple example.

A big obstacle to debate here is the problem of characterization: what criteria distinguish capacity concepts from OK concepts? Surely we do not want to adopt the characterization of the early British Empiricists that OK concepts are those built out of ideas that are copied from impressions. Operationalization was on offer for a while, but it seems to cut too narrowly since it rules out many central theoretical concepts. Nor do I think we can be content with Carnap's similarity circles and the *Aufbau*.

In my own early attempts to understand these empiricist strictures, I proceeded differently, in a way that generally works best for 'trouser' words (that is, for concepts whose primary meaning comes from what they rule out): figure out what is supposed to be wrong with the illicit concepts; the OK ones are those that don't have those problems. What then is supposed to be wrong with power concepts? One central worry comes from the fact that power concepts seem to be tied either too closely or too loosely to their related effects. This in turn is thought to lead to problems in testing claims about powers. I shall discuss these latter in section 4.

That powers and their effects are tied too closely was the complaint of the old Mechanical Philosophers against Scholastic concepts.[3] What makes heavy bodies fall? Gravity. What is gravity? That which makes heavy bodies fall. For those like Ernst Mach,[4] who wished to provide explicit measurement procedures for the concepts of physics, Newton's second law seems to suffer the same difficulty. $F = ma$. What is it for a body of mass m to be subject to a total force of size F? A mass of size m is subject to force F iff its acceleration is a.

[3] *Cf.* Glanvill(1661).

[4] Mach(1893)

On the other hand, when the power is not defined in terms of the occurrence of its effects, there seems to be no fixed connection between the existence of the power and the occurrence of its effects, neither strict (*i.e.*, universal) nor statistical. Aspirins have the power to relieve headaches; that is surely consistent with the fact that they do not always do so and perhaps there is no fixed statistical relation either. This objection to powers echoes an objection that Earman, Roberts and Smith make to *ceteris paribus* laws. There is a familiar way to fix this problem: insist that the effect is there after all whenever the power is. [5] One well known case of this arises in discussions of the problem of evil. God is omnipotent: He has the power to create any kind of world at all. Couple this with the auxiliary assumption that He is all good. The effect to expect is a benign world, full of delights. Instead we see plagues and poverty and vice. One stock response is that the world is all good despite appearances. We simply fail to see or perhaps to understand the situation properly. I take it that this kind of claim must be judged unacceptable by standards employed in successful science. The world does not appear good; it does not pass any of the standard tests for being good; and its effects are not the effects we are entitled to predict from a world of virtue and perfection.

Or consider Freudian claims, which we know distressed many followers of the Vienna Circle. Consider a crude version of one Freudian example. Freud maintained that the childhood experiences that the Ratman sustained have the power to make one desire the death of one's father. Freud says: "... he [the Ratman] was quite certain that his father's death could never have been an object of his desire but only of his fear ... According to psychoanalytic theory, I [Freud] told him, every fear corresponds to a former wish which was now repressed; we were therefore obliged to believe the exact contrary of what he had asserted ... He wondered how he could possibly have had such a wish, considering that he loved his father more than any one else in the world."[6] The Ratman did not recognize this desire in himself, he appeared to others as a loyal and loving son and he had behaved just like someone concerned to ensure the welfare and safety of his father. But that's alright on Freud's view. The desire is really there; it is just unconscious and thus

[5] Or, to be more fair to the proponent of powers, 'whenever the power obtains and the circumstances are propitious for its exercise.'

[6] Freud(1909), p. 39

does not manifest itself in the usual ways.

Turn now to what Earman, Roberts, and Smith call "special force laws", like the law of universal gravitation (A system of mass M exerts a force of size GMm/r^2 on another system of mass m a distance r away) or Coulomb's law (A system with charge q_1 exerts a force of size $\varepsilon_0 q_1 q_2/r^2$ on another system of charge q_2 a distance r away).[7] These are not strict regularities. Any system that is both massive and charged presents a counterexample. Special forces behave in this respect just like powers. This is reflected in the language we use to present these laws: one mass *attracts* another; two negative charges *repel* each other. *Attraction* and *repulsion* are not among what Ryle called 'success' verbs.[8] Their truth conditions do not demand success: X can truly attract Y despite the fact that Y is not moved towards X.

But perhaps, as with the delights of our universe or the Ratman's desire for the death of his father, the requisite effects are really there after all. Earman, Roberts and Smith feel that the arguments against this position are not compelling. I think they are: the force of size GMm/r^2 does not appear to be there; it is not what standard measurements generally reveal; and the effects we are entitled to expect—principally an acceleration in a system of mass m a distance r away of size GM/r^2—are not there either.

Contrast a different case.[9] In simultaneous equations models in economics each equation is the analogue of a special force law: each describes the operation of a single cause. When more than one cause is present, all the equations must be satisfied at once. So the pattern of behaviour that occurs is one consistent with each equation separately. Unlike mechanics, the 'special force laws' in economics really are strict regularities (if true at all). The effects demanded by each law separately are really there and they meet standard requirements for doing so: the effects of each appear to be there; standard measurements reveal them; and the effects of these effects are the ones we are entitled to expect.

The price level in economics is a contrast. It is calculated by summing the 'contributions' of a variety of different causes. But we do not want to think of the price level as literally composed of a lot of distinguishable parts as a wall is composed of its stones—the level from w to x is that due to the

[7]Note that throughout I take the special force laws to ascribe forces and not motions to situations.

[8]Ryle (1949)

[9]See Cartwight (1989), ch. 4.

stock of money; from x to y, that due to the velocity of money; *etc.* This seems a highly unnatural reading to me. And it seems even more so when we move to engineering examples–say the construction of complex machines from simple ones or of circuits from combinations of resistors, capacitors and impedances—where the rules for how to calculate what happens when the parts act together are not by simple addition as they are in the case of *force* or *price level*.

Kevin Hoover in his extended study *Causality in Macroeconomics* backs up my point by criticizing the assumption of linearity of causal influences; that is, the assumption that "the influence of Y can be added to the influence of M and so forth"[10] in calculating their effect when operating jointly. (In Hoover's example, Y is income, M, money and the effect is the interest rate.) Hoover complains, "But linearity is unduly restrictive."[11] Hoover is particularly concerned with the non-linearities arising from rational expectations theory, which imply that the influences of macroeconomic causes cannot be calculated just by addition. He illustrates 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 manner in which it manifests it in the drill press or in some other machine...[12]

Does all this matter? Pretty clearly it does not matter to the economics or to the physics under discussion. But it does matter to the metaphysics, particularly to the topic under discussion here—*ceteris paribus* laws. To see why let me explain how I see the difference between physics and many of the human sciences. We study capacities throughout the sciences.

[10] Hoover (2001), p. 55
[11] *ibid.*
[12] *ibid.*, pp. 55f.

Many of the central principles we learn are principles that ascribe specific capacities to specific features that we can independently identify, from the capacity of a massive object to attract other masses to the capacity of maltreatment of a child to cause that child to maltreat its own children, or, to mention the example that Earman, Roberts and Smith discuss in their paper in this volume, the capacity of smoking to cause lung cancer.

What is special about physics then? Not that it does not offer knowledge about powers or capacities but rather that it has been able to establish other kinds of knowledge as well, knowledge that we can couple with our knowledge of capacities to make exact predictions. This additional knowledge is primarily of two kinds: 1) We know for the powers of physics when they will be exercised (*e.g.*, a massive object *always* attracts other masses); and 2) we have rules for how to calculate what happens when different capacities operate together (*e.g.*, the vector addition law for forces). This kind of knowledge is missing for many other subjects. That is why they cannot make exact predictions.[13]

Now for *ceteris paribus* laws. Consider Earman, Roberts and Smith's example

(S) *CP*, smoking causes lung cancer

of which they say, "If some oncologist claims that (S) is a law, then, we maintain, there is no proposition that she could be expressing..."[14] I disagree. I take it that the proposition she is expressing is

(S') Smoking has the capacity to cause lung cancer.

a claim exactly analogous to the special force laws of physics. This is a precise claim: it states a matter of fact that is either true or not; it is not vague; and it has no *ceteris paribus* clause that needs filling in. So it does not suffer from those faults Earman, Roberts and Smith ascribe to *ceteris paribus* laws. More central to their objections, it is testable, it makes predictions, and it entails regularities in the course of events, in this case statistical regularities. This is the topic of section 4.

[13]But they often can make rough predictions or give good advice.

[14]Earman *et al.* forthcoming

3 The Limits of Scientific Languages

In the discussion so far I have been more liberal in my reconstruction of the language of science than most modern empiricists. I allow it to cover more, to talk about powers and capacities. Now I shall propose a way in which I think the languages of the different sciences can describe less.[15]

Consider Newton's second law, $F = ma$. What does it say? Many, probably including Earman, Roberts and Smith, take it to describe a strict regularity. I think that it does so only conditionally. The claim we are entitled to believe from the vast evidence in its favour is this: *if nothing that affects the motion operates that cannot be represented as a force*, then... The two views collapse together if all causes of motion can be represented as forces. Why do I think many might not be?

Newtonian mechanics, like many other theories in physics, has, I believe, very much the structure that C. G. Hempel taught us theories have.[16] It consists of internal principles, such as Newton's three laws of motion, which give relations among the central concepts of the theory, and bridge principles, which constrain how some of the concepts of the theory are applied. Many early logical positivists hoped that the bridge principles would lay out direct measurement procedures for all the concepts of the theory. They had to settle for less. The bridge principles match some theoretical concepts with concepts 'antecedently understood'.

In the case of Newtonian mechanics the primary bridge principles are the special force laws. These license a particular theoretical description—*e.g.* '... is subject to a force $F = GMm/r^2$' or'... is subject to a force $F = \varepsilon_0 q_1 q_2 / r^2$'—given a description in the vocabulary of masses, distances, times and charges—*e.g.* '... is a mass m located at distance r from a mass M', or '... is a charge q_1 located at distance r from charge q_2'.[17]

Bridge principles provide strong constraints. The theoretical descriptions—in our example the individual force functions from the special force laws—are allowed *only if* the corresponding descriptions in 'antecedently under-

[15] For a more detailed discussion see Cartwright (2000).

[16] Hempel (1966)

[17] For purposes of this section we can remain neutral about my claim in Section 2. It does not matter for the points here whether we take the special force laws to ascribe capacities of a certain types or instead to ascribe an actually existing force.

stood' terms are satisfied.[18] (For example, "The force on a mass of size m is GMm/r^2 if and only if m is a distance r from a body of mass M".) The same thing is true of quantum mechanics and its bridge principles as well as quantum field theory, quantum electrodynamics, classical electromagnetic theory and statistical mechanics.

It is because of the issue of evidence that I urge that the bridge principles of these theories are so strongly constraining. I have looked at scores of applications and tests of the theories in my list, applications and tests of the kind that we take to argue most strongly for the truth of these theories. In these cases the theoretical terms that have bridge principle are invariably applied via the bridge principles. This interpretation of the demands of bridge principles in turn puts a strong constraint on the descriptive power of the theory. Force functions can be legitimately applied only to situations that are described in bridge principles. Similarly for quantum Hamiltonians, classic electric and magnetic field vectors and so on.

Can all causes of motion be correctly described using just the descriptions that appear in the bridge principles of Newtonian mechanics? To all appearances, not. We have millions of examples of motions that we do not know how to describe in this way. Consider one case where we eventually were successful. For centuries we knew about electricity and magnetism: *e.g.* rubbing a glass rod against cat fur can cause human hair to move; loadstones can move iron filings; and so forth. But we could not add these in as forces in Newton's second law. Eventually we evolved the formal, precise concepts of electric and magnetic charge as well as the bridge principles that assign them force functions. In so doing we came to ringfence a host of macroscopic situations from all the rest of those that could cause motion but that we could not describe in Newtonian theory: *these* are ones involving attraction or repulsion between electrically or magnetically charged objects. But what of the vast remainder?

We have succeeded in applying Newton's second law to a vast, vast number of cases—but always of the same kinds: the ones that appear in our

[18]There are two caveats here. First, it can happen that exactly the same vectorial quantity F that normally is associated with one force law applies to a situation 'by accident' even when it does not satisfy the requisite description because of the particular values of the force properly ascribed to the situation by other special force laws. Second, I would like to remain neutral about how strict we need to be about when 'the description offered in the bridge principle is satisfied.'

bridge principles. And there are still not many bridge principles included in Newtonian mechanics, even after 300 years.[19] We are not constantly expanding the theory, regularly producing new bridge principles to meet either new cases or the old familiar ones. Nor do we have continuous success in bringing these cases in under the old bridge principles. This suggests that these cases may well not fall under any descriptions for which there are force functions. And a handful of striking successes does not discount this worry.

My conclusion from these kinds of considerations is that we need to add to the basic 'equations of motion', like $F = ma$ or Schrödinger's equation, a special constraining condition: The equation holds so long as everything that can affect the targeted effect is describable in the theory. This is the formulation of the law that we have strong evidence for. Notice that it is in 'precise and closed form' and hence does not look like a *ceteris paribus* law on one of the criteria of Earman, Roberts and Smith. But how do we test it?

4 Testing

How do we test my version of Newton's second law or the Schrödinger equation? In *exactly the same way* that we would test them if they had no condition attached. The same is true *mutatis mutandis* for capacity ascriptions and for certain kinds of more conventionally rendered *ceteris paribus* laws. Although there are important differences, let us for the sake of brevity lump all these together and consider them to be of the form, 'If nothing interferes, then ... (some strict or statistical regularity).'[20]

[19] Paul Teller (personal communication) has objected to my claim that in quantum mechanics there are only a small number of bridge principles by pointing out that, as I myself urge, there are a good many 'derivative' bridge principles. These, however, almost never expand the scope of the theory, but rather contract it. For they are in fact, as their name says, *derived*. We start with a situation modeled with a combination of descriptions available from our basic bridge principles. Then we add more facts about the situation to derive a new force function for it, by *limiting* the original force function. The derived bridge principle then provides force functions for only a subset of cases that the original did. Of course sometimes we make approximations as we go along. But that, if anything, narrows the scope of the force function even more. For now it is no longer true even of the originally described situation but only of some approximation to it.

[20] In the case of the equations of motion, as we have seen, the caveat really refers to factors not describable in the language of the theory. Setting aside some niceties, we can assume that capacity claims of the form "A has the capacity to Φ" imply that if nothing

Suppose we wish to test $F = ma$ in its unconditional form. We set up a number of different kinds of situations to which, using our bridge principles, we would naturally assign some specific force function. For instance we arrange two bodies of charges q_1, q_2 and masses m_1, m_2 a known distance r apart so that we can assign the force function $Gm_1m_2/r^2 + \varepsilon_0 q_1 q_2/r^2$ directed between them. We ensure as best we can that the situation does not explicitly meet any of the other descriptions to which we know how to assign force functions. Then we also ensure as best we can that there is nothing else about the situation that might be assignable a force function—there is no significant wind, no trucks rumbling by, no bright lights, ... Finally we look to see if the motions that occur in all these situations match those predicted from the equation.

Suppose instead that we wish to test'If nothing interferes with the operation of the force, $F = ma$.' Everything in the description of what we do will be identical to the previous description except for five words. We substitute for the sentence 'Then we also ensure ...' a new one: 'Then we also ensure as best we can that there is nothing else about the situation that might *interfere with the force's operation.*' And what we actually do to ensure this is the same in both cases.

'But,' one may ask, 'how do we know what to eliminate in the second case?' I think the question is more appropriately 'How do we know what to eliminate in the first case?' We do not look for features that figure in our bridge principles as we did in setting up the basic part of the experiment. Of course people who believe in the unconditional form of the law will assume that the features we are looking for are exactly the other things in the situation that can be assigned force functions. But that does not supply them with a method for picking these features out.

Here's what I think happens. We have seen a vast number of cases of forces at work to which we have tried to fit Newton's second law and over time we have established very strong rules of thumb about what can make trouble for it. That is, we have learned from a lot of experience what kinds of things might *interfere* with the operation of the force. That's what we control for.

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interferes A will Φ; and probabilistic ascriptions "A has capacity of strength r to Φ", to imply roughly that if nothing interferes the probability that A will Φ is r. But, as I have argued in Cartwright (1999) capacity ascriptions can say a lot more as well.

Earman, Roberts and Smith might think my testing strategy lets in too much. They consider what might seem an analogous reading of *ceteris paribus* laws:

> It has also been suggested that we can confirm the hypothesis that *CP*, all F's are G's if we find an independent, non-*ad-hoc* way to explain away every apparent counterinstance... But this could hardly be sufficient. Many substances that are safe for human consumption are white; for every substance that is white and is not safe for human consumption, there presumably exists some explanation of its dangerousness... but none of this constitutes evidence that *CP*, white substances are safe for human consumption.[21]

The reading that Earman, Roberts and Smith offer for *ceteris paribus* laws is an excellent attempt at the logical positivist program of substituting acceptable formal-mode concepts for dicey material-mode ones. For example: X *causes* Y becomes X *explains* Y, which in turn becomes Y *can be derived from* X *given the claims of our theory.* Here we have analogously: X *interferes with* $(x)(Fx \rightarrow Gx)$ becomes X *explains why a, which is F, is not G*, which presumably in turn becomes *Ga can be derived from X given the claims of our theory (and perhaps Fa as well).*

The rendering is a good try, but it does not work, as we can see from Earman, Roberts and Smith's own example. It does not work for very much the same reason that the analogous formal-mode rendering of causation does not work. You can't get causality and its associated family of concepts out of laws unless the laws themselves are causal laws, not just laws of association, in which case the program of replacement fails anyway.

Moreover, the program is misguided to begin with. There is nothing unacceptable about the concepts of *causation* and *interference.* They are well understood, claims about them are testable and, as G.E.M. Anscombe argues,[22] some causal relations are directly observable; or, more guardedly, causal concepts do not systematically fare worse in any of these respects than other concepts.

[21] Earman *et al.* forthcoming

[22] in her (1971)

We have, I maintain, ample reason to think that there is as much a fact of the matter about whether it is a causal law that forces cause motions as there is about whether it is a law that $F = ma$; that there is as much a fact of the matter about singular causal claims as there is about other relational claims; and as much a fact of the matter about whether X interferes with some process as about whether the process itself obtains.

The drawback to *interference* is not that there is something wrong with it ontologically; it is rather that we often have epistemic problems. First, we often cannot tell just by looking that X is interfering with Φ, though even this is not always the case. (Sometimes, for example, it is easier to tell that you are, interfering with someone's work than to tell that they are working. My friends Anne and Sandy, sit at their computers typing just as I do now *for fun.*) Usually to make judgements about interference, we need to have a lot of specialized knowledge and a lot of experience; you can't just tell by looking.

Second, it seems that in most cases there are no systematic rules linking X *interferes with* Φ to other descriptions in some special vocabulary that we prefer epistemically (unless the vocabulary is itself heavily laden with concepts that already imply facts about causality, such as *pushing*, *attracting*, *shielding*...). We almost never have 'special interference laws' to tell us in, say the language of masses, charges, distances and times, when something interferes with something else in the way that we have special force laws to tell us when a particular force function obtains.

We should note though that the absence of'special interference laws' is not so epistemically damaging as many suggest. The special force laws do tell us when a particular force function obtains, but only for very specific descriptions—the descriptions that appear in our bridge principles. For other descriptions that may be applied far more immediately and be far more epistemically accessible, such as *a truck passing by* or *the press of the wind*, we are just as much on our own without the help of a system of rules as we are in deciding if we can label the truck passing by as an *interference.*

There are four facts I would like to underline:

1. The lack of systematic rules does not mean that we cannot have knowledge about whether a certain kind of occurrence constitutes an interference. Galileo after all knew to use smooth planes for his rolling-ball experiments because he knew he should eliminate the interference of

friction with the pull of the earth. Similarly he knew to drop small compact masses and not feathers from the Leaning Tower. And that was long before he could have had any idea whether friction or the wind exerted a *force* in the technical Newtonian sense.

2. The fact that we cannot identify what counts as interference with respect to a claim Φ does not mean that we cannot test whether Φ is true or not. Consider *Aspirins relieve headaches, if nothing interferes.* We regularly test claims like this in randomized treatment/control experiments.

3. Nor does it mean that is it is too easy to dismiss disconfirmations.[23] When the predicted result fails to transpire, one can always *say* that something interfered. But saying does not make it true. And as epistemologists are always reminding us, saying, even when it is true, does not constitute knowledge, or even reasonable belief. We need a good reason for claiming that something is an interference. When we do not have any idea whether a nominated factor is an interference or not, then we equally have no idea how to classify the case. Our intended test is no test at all.

4. It follows that one needs a great deal of information about what might and might not interfere with a process before we can carry out serious tests on the process and that in turn means that we need already to have a great deal of information about the process itself. That just means that science is difficult, as we already knew, and that it is hard to get started in a vacuum of knowledge.

5 Capacities and Induction

In *Nature's Capacities and their Measurement*[24] I offer a number of defences of capacities:

[23]For a recent example of this kind of claim specifically in the context of the capacity laws I defend see Winsberg *et al.* (2000).

[24]Cartwright (1989)

a) Once we have rejected Hume's associationist view of concept formation, there is no good argument against the family of concepts connected with causes and capacities.

b) Strengths of capacities[25] are measurable, just as is the strength of the electromagnetic field vectors or the energy of a system.[26]

c) We commonly use the analytic method in science. We perform an experiment in'ideal' conditions, I, to uncover the 'natural' effect E of some quantity, Q. We then suppose that Q will in some sense 'tend' or 'try' to produce the same effect in other very different kinds of circumstances. (What I mean by 'in some sense' is that the rules for calculating what happens when a number of factors with different 'natural' effects operate together will differ according to subject matter. Recall the examples of such rules in section 2.[27]) This procedure is not justified by the regularity law we establish in the experiment, namely 'In I, $Q \rightarrow E$'; to adopt the procedure is to commit oneself to the claim'Q has the capacity to E'.

In The *Dappled World*[28] I add another. With the use of capacity language we can provide a criterion for when induction is reliable. This, I maintain, cannot be done with the use of OK properties and strict regularities alone. Earman, Roberts and Smith object to this claim. This is not surprising because they also reject one of its major premises.

Imagine we set up a very complex and delicate design, D, an ideal experiment, to observe the precession of a gyroscope in order to test relativistic predictions about the effects of space-time curvature on precession. The result is R. To the extent that we believe our design is a good one and that we

[25] This includes their presence or absence.

[26] We cannot, of course, tell by the measurement itself that what we are measuring is a real capacity, anymore than we can tell by the procedures for measuring the electric field strength that what we are measuring is a real quantity. In both cases that requires a lot of theory.

[27] In Cartwright (1999) I argued that nature might not have always provided such rules. Even in that case there is a cash value to knowledge about the capacity. The associated effects are more likely to occur when a feature with the appropriate capacity is present than when no such feature obtains. (Consider using a magnet to pull a pin from between the floorboards. It is a good idea to try the magnet even should there be no fixed rules for what happens to the pin in just exactly that combination of circumstances.)

[28] Cartwright (1999)

have implemented it properly, we expect that that result should be repeatable in just that experimental set-up, *i.e.* we believe that $D \rightarrow R$ is a strict regularity.

What does it mean that'our design is a good one'? That is, what criteria must D satisfy if R, which occurs on one occasion of D, is to occur whenever D occurs? The crude answer is that D must control for all factors relevant to R. I read this as'D is an arrangement in which the capacity of the space-time coupling to produce precession R operates unimpeded.' Those who do not like capacities will try other ways to explain relevance. I imagine they look to strict regularities and consider two levels at which they might look. First, at a concrete level. Look through all the strict regularities involving very concrete features that have R as a consequent. All and only factors that occur in the antecedents of these are relevant and should be controlled for. My objection to this strategy is not that the list is too long but rather that it will not provide the information we need. Almost *anything* can appear in one of these laws, depending on the arrangement of the other factors; we could design our experiment in indefinitely many ways and still expect the result R. Any feature that was essential to any of these designs gets counted as relevant. Moreover, the long list of regularity laws with R in the consequent will not fix *how* a relevant factor should be controlled for. That will depend on the actual design, D.[29] So lists of strict regularities at a very concrete level cannot provide a criterion that D must satisfy if its results are to be repeatable.

A more plausible proposal is to look at a more abstract level, as Earman, Roberts and Smith propose. The abstract formula for precession is

$$Precession: d(n_n^r)/dt = \Gamma^r n_s/\omega_s$$

This formula suggests that an adequate criterion is, 'Eliminate all sources of torque (Γ) except that arising from the space-time coupling as well as all sources of variation in the gyroscope's moment of inertia (I) and in its spin angular velocity (ω)'. Let us concentrate on the torque, as Earman, Roberts and Smith do. They suggest that we couple the formula for precession with 'the laws relating force to precession and various special force laws' to fix

[29] In my (1988) and (1989) I give examples where two different laws employ the same factor in different ways.

what D must be like. What is wrong with that from my point of view? Two things—the first familiar from section 2. and the second from section 3.

1. The special force laws are not strict regularities among OK features.[30]

2. As with Newton's second law, we do not have sufficient evidence to ensure that the precession law can be read as a strict regularity. (A more cautious rendering includes a condition: 'If nothing that cannot be described as a torque (or a variation in I or ω) interferes, then...')

In their discussion in this volume Earman, Roberts and Smith deny 1), as we have seen. I suspect they would also deny 2). I have explained why I disagree with them. But if we grant either of these assumptions, we see that the job cannot be done with strict regularities alone. We need capacities. The generalization '$D \rightarrow R$' is reliable because D is a kind of situation in which a stable capacity (the capacity of space-time curvature to affect precession) operates without interference.[31]

[30] This is the assumption that figured in the arguments of my (1999), p. 95, where I concluded, "The regularity theorist is thus faced with a dilemma. In low-level highly concrete generalizations, the factors are too intertwined to teach us what will and what will not be relevant in a new design. That job is properly done in physics using more abstract characterizations. The trouble is that once we have climbed up into this abstract level of law, we have no device within a pure regularity account to climb back down again". The device we need includes the special force laws, which, I maintain, can not be rendered as statements of regularity.

[31] In my (1999) I dubbed situations like this 'nomological machines'. This is to highlight both the need to eliminate interference, which I have stressed in this discussion, and the need to have the right kind of internal structure (one for which there are rules about how the contributions of the parts combine), which I do not discuss here.

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