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CETERIS PARIBUS LOST

ABSTRACT. Many have claimed that ceteris paribus (CP) laws are a quite legitimate feature of scientific theories, some even going so far as to claim that laws of all scientific theories currently on offer are merely CP. We argue here that one of the common props of such a thesis, that there are numerous examples of CP laws in physics, is false. Moreover, besides the absence of genuine examples from physics, we suggest that otherwise unproblematic claims are rendered untestable by the mere addition of the CP operator. Thus, "CP all Fs are Gs", when read as a straightforward statement of fact, cannot be the stuff of scientific theory. Rather, we suggest that when "*ceteris paribus*" appears in scientific works it plays a pragmatic role of pointing to more respectable claims.

1. INTRODUCTION

Philosophers fall in love with arguments. As love is blind(ing), it is no surprise that an appealing argument can seduce philosophers into believing the most outlandish things. As a case in point we could cite the topic of *ceteris paribus* (CP) laws.[1] Through a constellation of arguments, very many philosophers have managed to convince themselves not only that there is such a topic but that it is an important topic meriting a never-ending stream of articles in philosophy journals. The *innamorati* are not shy of declaring their love. Thus, for example, two recent articles, Peter Lipton's (1999) "All Else Being Equal" and Michael Morreau's (1999) "Other Things Being Equal", begin with remarkably similar declarations. Lipton: "Most laws are ceteris paribus (CP) laws" (155). Morreau: "Arguably, hedged laws are the only ones we can hope to find. Laws are commonly supposed to be truths, but interesting generalizations, without some modifier such as 'ceteris paribus' are by and large false" (163).

We are bearers of bad news: put crudely, our message is that the object of their affections does not exist. To be less crude and more specific, the following seven theses are widely endorsed in the philosophical literature:

(T1) It is legitimate for a theory of a special science (e.g., psychology, biology, economics) to posit CP laws.

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(T2) It is scientifically legitimate for a theory of fundamental physics to posit CP laws.

(T3) Some of our best current scientific theories (especially those in the special sciences of psychology, biology, economics etc.) posit CP laws.

(T4) All of the laws posited by our best current scientific theories (even those of fundamental physics) are CP laws.

(T5) There are in the world CP laws pertaining to "higher-level" phenomena such as those studied by the special sciences.

(T6) There exist CP laws.

(T7) All of our world's laws of nature are CP laws.

These theses span a great range of regions of philosophical inquiry: (T1) and (T2) concern scientific methodology; (T3) and (T4) concern the interpretation of current scientific theories; (T5)–(T7) concern metaphysics. But the arguments that have led our colleagues to these different theses are intimately intertwined. We maintain that although these intertwined arguments take note of important and interesting phenomena, they are deeply misleading, and that all of (T1)–(T7) are false.

Within the scope of this paper we cannot hope to set out in full the motivations for our sweeping claim. But we will attempt to convey some of the key considerations. In particular, we will analyze some of the mistakes that have led to the widely held notion that it is CP all the way down to fundamental physics, and at the same time we will set out our reasons for holding that laws are strict in fundamental physics (Section 2). Then we turn to our reasons for thinking that it is a bad idea to admit CP laws at all (Section 3). But given that the special sciences do not articulate strict laws, we are faced with the challenge of explaining the scientific status and the manifest achievements of these sciences. We will consider a way of meeting this challenge in Section 4.

While jilted lovers eventually recover, lovers of a chimera can rarely admit that their love had no object. Thus, we do not expect the *ceteris paribus* stream to dry up. But we do hope that once some of the confusions that have channelled this stream are recognized, it will take a different and more productive course.

2. IT'S NOT *ceteris paribus* ALL THE WAY DOWN

The claim (T4), which implies that it is CP all the way down for all physical laws, is a commonplace, as are the milder claims that some or most physical laws are CP. (For example, the opening page of Morreau's (1999) contains the assertion that, on pain of falsity, the law statements of economics, biology, and the other "non-basic sciences" must contain CP clauses. He adds: "There are reasons to think it is so in basic sciences like physics, too" (163).[2]) (T4) has twin functions in the literature. First, it lends legitimacy to the CP industry. The practitioners typically concentrate on examples drawn from the special sciences, but confidence in (T4) allows them to proceed without worry that they are focusing on some peculiar and, perhaps, undesirable feature of the special sciences. If even fundamental physics must resort to CP clauses when stating its laws, the thinking goes, then surely laws qualified by such clauses are scientifically legitimate and deserve attention from philosophers of science. Second, by rejecting the view that among all the sciences only physics is capable of discovering strict laws, (T4) seems to strike a blow against "physics chauvinism".

We hasten to insist that upholding "physics chauvinism" is no part of our project. A shortcoming of much twentieth-century philosophy of science was the assumption that physics is the paradigm science and that other sciences are scientific only insofar as they resemble physics. Once we give up this assumption, we should no longer automatically view any particular apparent difference between theories of physics and theories of other sciences as a threat to the legitimacy of the other sciences. So, given that economics, psychology etc. evidently discover no strict laws, but at best CP laws, it does not follow that we *must* say it is CP all the way down in order to avoid being physics chauvinists. Moreover, we *shouldn't* say this. The laws of representative theories from fundamental physics are not qualified by CP clauses, as we have argued in Earman and Roberts (1999) and Smith (2002). The claim that they are has been supported by a variety of moves, six of which we will review and criticize in the remainder of this section.

(i) *Appeals to examples from physics.* It is frequently alleged that actual physical theories provide examples of CP laws. But do they really? Note first that in order for a putative example of a "real" CP law to be interesting, it would have to involve a CP clause that is ineliminable. The reason why the law, as typically formulated, contains a (perhaps implicit) CP clause whose range is not made explicit, must be that the range of this clause *cannot* be made explicit. Otherwise, the CP clause is merely a function of laziness: Though we *could* eliminate the CP clause in favor of a precise, known conditional, we choose not to do so. There are two reasons why one might not be able to make explicit a more precise conditional: (1) we do not know how to state the conditions under which the qualified regularity holds; or (2) there is reason to suspect that even with the best of knowledge, these conditions could not be made explicit, because they will comprise an indefinitely large set. The first possibility is not really relevant here; a putative example of a CP law whose CP clause could not be eliminated just because we didn't know how to eliminate it would not show that physics actually discovers CP laws, only that it might. For all we know, future empirical research could reveal the conditions under which the regularity obtains. (Below we counter the most prominent arguments to the effect that we should not expect there to be any such conditions waiting to be revealed; see Subsections (v) and (vi).) This will be a case where what's needed is further scientific knowledge, rather than a philosophical analysis of the status of CP laws.

A physical law with a CP clause that is ineliminable for the second reason would be more interesting, and much of the literature is motivated by the belief that there are such laws (see, for example, Giere (1999) and Lange (1993, 2000)). However, it seems to us that there is no good reason to believe this, for the prominent alleged examples turn out upon scrutiny to be cases where the CP clause is eliminable. For instance, Lange claims that "To state the law of thermal expansion [which states that the change in length of an expanding metal bar is directly proportional to the change in temperature] . . . one would need to specify not only that no one is hammering the bar on one end, but also that the bar is not encased on four of its six sides in a rigid material that will not yield as the bar is heated, and so on" (Lange, 1993, p. 234). But this list is indefinite only if expressed in a language that purposely avoids terminology from physics. If one helps oneself to technical terms from physics, the condition is easily stated: The "law" of thermal expansion is rigorously true if there are no external boundary stresses on the bar throughout the process.[3] Other putative examples of indefinite conditions can likewise be easily stated within the language of physics. For instance, Kepler's "law" that planets travel in ellipses is only rigorously true if there is no force on the orbiting body other than the force of gravity from the dominant body and vice versa. Later we will argue that each of these examples is only problematically considered a law. So, they are not CP laws because (a) the CP clause is easily eliminable by a known condition, and (b) they are not laws anyway. So far, the alleged philosophical problem of CP laws has yet to make an appearance in the realm of fundamental physics.

(ii) *Confusing Hempel's provisos with ceteris paribus clauses.* Proponents of the claim that it is CP all the way down often refer to Hempel (1988) (see, for instance, Fodor (1991, p. 21), Giere (1999, pp. 90–91) and Morreau (1999, fn. 1)). However, a careful reading of Hempel's article reveals that his central concern is not the alleged need to save law statements from falsity by hedging them with CP clauses, but rather the problem of applying to a concrete physical system a theory of physics, the postulates of which are *assumed to express strict laws in no need of hedging*. Hempel notes that such an application typically requires the specification of the values of theoretical parameters, which are not ascertainable by direct observation. This simple point immediately raises a problem for the view that the empirical content of a theory is the set of its observational consequences; for if Hempel is right, this set will be null or very small. But as interesting as it is, this problem is far from the CP problem. One gets closer to the latter with Hempel's further observation that the applications of laws that physicists actually construct are often hedged. For example, a natural if somewhat crude application of Newton's theory of motion and his law of gravitation to the planets of our solar system involves assuming that this system is closed. One can, under this assumption, derive a differential equation of evolution type (or coupled set of them) that describes the motion of the planets given this assumption. The application will be valid provided that no other significant non-planetary masses are present and provided that no significant non-gravitational forces are acting on the planets. If the theory does not specify the allowed types of long-range non-gravitational forces – as Newton's original theory did not – then the second proviso has a kind of open-ended character reminiscent of CP clauses. But this does not amount to the conclusion that Newton's laws have implicit CP clauses. For in the first place, the condition for the validity of the application can be stated in precise and closed form: the magnitude of the non-gravitational forces must be small enough in comparison with the gravitational forces that the *theory* implies that the neglect of the non-gravitational forces does not affect the desired degree of accuracy of the predictions of planetary orbits. And in the second place, the conditions of the provisos are conditions for *the validity of the application*, not conditions *for the truth of the law statements of the theory*; if this were not so the theory could not be used to decide how small the magnitude of the non-gravitational forces has to be in order that they can be neglected. Once again the alleged problem of CP has failed to rear its head in physics.

(iii) *Confusing laws with differential equations of the evolution type.* We can offer here, however, a diagnosis of why it has *looked* to people as if there is a problem of CP laws in the vicinity. What makes it easy to miss the distinction between a *theory* consisting of a set of non-hedged laws and an *application of a theory* that might be hedged (though, again, in an easily stateable way) is that differential equations of evolution type – like the one we imagined deriving above – and their consequences are often thought of as *laws*.[4] If one takes them to be laws, one expects them to be *part* of the theory in question and, thus, it looks like the theory contains hedged laws. But differential equations of evolution type are not laws; rather, they represent Hempel's applications of a theory to a specific case. They are derived using (unhedged) laws along with non-nomic modelling assumptions that fit (often only approximately) the specific case one is modelling. Because they depend on such non-nomic assumptions, they are not laws. For example, because Kepler's "law" that planets travel in ellipses is derived from laws together with the assumption that there are only two bodies in the universe, it is not a law in spite of the normal nomenclature.[5] Lange's example of the "law" of heat expansion of metals is derived from a differential equation under the assumption that there are no boundary stresses, but that is a non-nomic boundary condition. The "law of free fall" is a consequence of a differential equation that involves the assumption that there is no resistance from the wind. That too is a non-nomic assumption, for it is not a law that there is no resistance from the wind. It seems to us that the role played by idealizations in physics is typically found here, in the derivation of differential equations, rather than within the laws themselves. The differential equations involve idealizations that need to be "hedged" in the sense described above, but this is no evidence that the laws used to derive them do.

(iv) *Early Cartwright on component forces*. In *How the Laws of Physics Lie*, Nancy Cartwright offered an argument, which still enjoys widespread influence, that special force laws like Universal Gravitation (henceforth UG) have to be merely *ceteris paribus* because they "lie" about the motion of bodies. UG, for example, supposedly misrepresents the temporal behavior of an object that is also being acted upon by, say, a Coulomb force (Cartwright, 1983, 1999; Pietroski and Rey, 1995; Giere 1999). So, it must be saved from falsehood by a (usually implicit) CP claim. But, UG *cannot* misrepresent the motion of a body, because it says nothing specific about such temporal behavior.[6] Only differential equations of evolution type – which might be derivable from UG together with other considerations – can be integrated to describe the temporal motion of a body or system of bodies. UG cannot be so integrated. Thus, it cannot misrepresent temporal motion. In reality, what we have here is a species of the confusion described in the previous section: Cartwright imagines the differential equation that leaves out the Coulomb force getting the motion wrong –

which it might – and blames that on one of the laws used in deriving the differential equation, UG. But there is more packed into this differential equation than just laws. What is really wrong with the differential equation is that it was derived under the assumption that nothing carried a net charge, a false non-nomic assumption. Neither UG nor any other law forced us to assume this. Thus, the original impetus from the (alleged) falsity of UG for thinking that the special force laws are CP is ill-founded.

Cartwright is aware of the availability of this kind of objection. Her reply is that on our view, according to which special-force laws like UG do not lie about motion because they are not about motion, there is nothing left for such laws to be about. On the face of it, such laws seem to be regularities governing component forces, but according to Cartwright, there are no such forces. Cartwright's position is not a blanket anti-realism; it is a local anti-realism about component forces (which allows that, e.g., resultant forces exist). We see no viable motivation for this local anti-realism. Successful physical theories apparently quantify over component forces, and there seems to be no natural way of "paraphrasing away" reference to such forces (as there is for, e.g., references to absolute motion in Newtonian mechanics). Cartwright (1999, p. 65) has suggested that non-total forces are not "occurrent" because they are not measurable. But in the first place, in many cases they *are* measurable (e.g. a scale measures the impressed gravitational force on an object, not the *total* force on it – the latter is approximately zero, since the scale itself gives rise to a normal force that keeps the object on it from having a total acceleration downward). And in the second place it is not clear that it follows that something is not occurrent just because it is not measurable.[7]

(v) *Cartwright's argument from Aristotelian natures and experimental method.* More recently, Cartwright has defended the view that laws, including those of fundamental physics, are not regularities in behavior, but rather ascriptions of capacities to kinds of systems. She supports this view with an argument (Cartwright, 1999, Chapter 4) to the effect that two features of scientific experimental methodology are inexplicable on the view that laws describe regularities of behavior, but can be made sense of on the assumption that they are about capacities. It is not obvious that this argument is rightly characterized as an argument that it is CP all the way down to fundamental physics. Cartwright's primary goal in this argument is not to establish that all laws are CP laws, but rather to argue against a "Humean" view that restricts the ontology of science to the behaviors of physical systems and regularities in those behaviors, and in favor of a broader ontology that includes natures and capacities. Indeed, Cartwright grants that laws entail strict regularities, though these are of the form

"Systems of kind K have capacity C", rather than the form of behavioral regularities (see her essay in this issue).

The distinction between capacities and behavior obviously plays a crucial role in this view. We are not sure exactly how this distinction should be understood. It is clear that capacities are supposed to be ontologically basic posits that have an irreducible modal or causal character that is problematic for "Humeans". But many things that are naturally regarded as having such a character, such as forces, seem to fall on the behavior side of Cartwright's distinction. For, on the standard reading of Coulomb's law, it states a putative regularity among charges, positions, and forces. Cartwright insists that the correct reading of this law is not the standard one, but rather one according to which the law attributes to charged bodies the capacity to exert a force on other charged bodies. The exertion of forces, it seems, counts as behavior, whereas the capacity to exert a force does not – so the latter but not the former is the sort of thing we should expect there to be law-like regularities about.

On what may fairly be called the standard reading of fundamental physical laws, these laws do state putative regularities among behaviors. Coulomb's law, for example, states a regularity concerning the exertion of electrostatic forces among charged bodies. So, although Cartwright does not say that laws do not entail *any* strict regularities, it is fair to say that on her view, those propositions that are standardly taken to state fundamental physical laws are not true (even if our best current physical theories are true) unless qualified by a CP clause. (For Cartwright, the best way to state the CP clause is: "so long as nothing interferes with the operation of a nomological machine.") This is the claim that we deny here.

In denying this claim, we do not mean to say that we agree with Cartwright's apparent intended target. She is concerned to refute the "Humean" view of laws according to which laws just are regularities in behavior. This rather naive view, and Cartwright's view, do not exhaust the options. One can grant that there is a lot more to being a law of nature than just being a true behavioral regularity, and even grant that what laws state is helpfully understood in terms of capacities, while maintaining that laws (and capacities) must supervene on the behaviors of physical systems. For example, one could adopt something like David Lewis's (1973) best-system analysis of laws, and allow that the Lewis-laws are usefully understood as attributions of causal capacities. Cartwright, however, seems to build a lot into her notion of capacities by denying that strict regularities in behavior can be deduced from regularities about capacities alone. Her argument is intended to show that experimental methodology cannot be made sense of without supposing that the laws scientists seek to discover are claims about capacities, where these cannot be cashed out in terms of behavioral regularities (construed broadly, so that "behaviors" include such things as the exertions of forces). This is what we will try to show she does not establish.

The first feature of experimental practice that Cartwright focuses on is generalizability. A typical experiment directly tests a low-level law concerning systems of the particular kind used in the experiment. Cartwright's example is the Stanford Gravity-Probe-B experiment. What this experiment directly tests is the low-level generalization:

> [A]ny fused-quartz gyroscope of just this kind – electromagnetically suspended, coated uniformly with a very, very thin layer of superfluid, read by a SQUID detector, housed in a cryogenic dewar, constructed just so ... and spinning deep in space – will precess at the rate predicted [by the general theory of relativity]. (Cartwright, 1999, p. 88.)

But the ultimate goal of this experiment is to test a much more general claim, an "abstract" law that is part of the content of the general theory of relativity (henceforth, GTR), namely, that relativistic coupling between the spinning of a gyroscope and spacetime curvature will result in the gyroscope's precessing at a certain rate. The problem of generalizability is that of saying why what happens in *this* experiment, which concerns a system of a very specific kind, provides evidence for the more general law, which concerns systems of other kinds as well. "What is at stake is the question, 'What must be true of the experiment if a general law of any form is to be inferred from it?' " (Cartwright, 1999, p. 87).

Cartwright's answer to this question is that in a system of the kind used in the experiment, relativistic coupling is allowed "to operate according to its nature" (ibid). All other factors, whose natures involve capacities to distort the influence of relativistic coupling on precession, have been eliminated or calculated away. But when a kind of system has, by nature, a capacity to do A, and everything else with a capacity to interfere with A has been eliminated, the system will do A. Hence, in the case at hand, if relativistic coupling really has the capacity to induce precession at a certain rate, then it will do just this in the case of the Gravity-Probe-B experiment.

But it seems that Cartwright's "Humean" opponent, who believes that laws are (or supervene on) true behavioral regularities sans CP clauses, can give a similar solution. Our background knowledge includes some well-confirmed general propositions, including (the general-relativistic analogue of) Newton's second law of motion, the laws relating torque to precession, and various special-force laws. We hypothesize that there is some law relating relativistic coupling to precession. By designing the experiment in such a way that the torque-components contributed by all other known factors (such as nearby charged or massive objects, frictional properties of the material used, etc.) that are nomically related to special forces and thence to torques are very close to zero, we can make it reasonable to assume that if any precession takes place, it will all be due to relativistic coupling. Hence, we can test the prediction made by any putative law relating such coupling to precession. This procedure requires that we take for granted a great deal of background knowledge – for example, that the laws just mentioned obtain, that there really is some law relating relativistic coupling to precession, and that there aren't any other factors nomically related to precession that we have neglected to take into account. It would be fair to demand independent empirical support for each of these presumptions. However, Cartwright's solution is subject to a precisely analogous difficulty. On her analysis, the experiment presumes that the various factors relevant to the experiment really do have the capacities we take them to have; that relativistic coupling really does have some stable capacity for producing precession; that there are no capacities operative in the experimental situation at hand that have not been taken into account. So it is hard to see where Cartwright's view has an advantage over the view that laws are true regularities.[8]

Some of Cartwright's comments suggest that she thinks that unless laws were about capacities, induction wouldn't be justified. Inductive inference from a particular case to the more general case, she argues, requires that we know that the systems involved in the observed situation have capacities that remain constant and that they carry with them from one situation to another (Cartwright, 1999, p. 90; see also Cartwright, 1989, pp. 145, 157–158, 163). Otherwise, we aren't entitled to believe that there is any connection between what happens in one situation and what happens in others. But if what we need to do is justify induction, then positing capacities and natures won't help. Recall Hume's argument that assuming secret powers in nature is no help in solving the problem of induction; the problem just becomes that of explaining how we can know that the same sensible qualities are always tied to the same secret powers (Hume, 1748/1993, p. 24).

Cartwright, of course, doesn't believe what Hume seems to presume: that inductive inference in science works from the ground up. She has a bootstrapping view, according to which we always already have a store of background beliefs about natures and capacities that we can rely on in order to test new hypotheses (Cartwright, 1999, p. 98). But if the trick can be done by background beliefs about capacities, then why can't it be done by background beliefs about regularities? Why can't we justify generalizing from one experimental situation to others on the basis of background beliefs to the effect that nature is governed by laws, that these laws entail strict behavioral regularities that are true throughout spacetime, and that approximations to some of the regularities are already known?

The second prong of Cartwright's argument concerns the design of controlled experiments. In designing the Gravity-Probe-B experiment, the experimentalists had to take account of all the different kinds of factors that could influence the precession of the gyroscopes, and control each one. How did they know which factors needed to be controlled? Cartwright's answer is that they knew that everything with a capacity to influence the precession needed to be controlled. She alleges that if laws were regularities rather than ascriptions of capacities, then there would be no way for scientists to know what to control.

Why is this? Why couldn't the scientists take into account all the laws relating other factors to precession, and infer that everything related to precession by one of these laws needs to be controlled? Cartwright considers this response, and rejects it. She argues that her opponent faces a dilemma. Either the laws to be consulted are all high-level, abstract laws, or they include low-level laws about particular kinds of concrete situations. If the former, then the laws won't supply enough information to address the experimental problem at hand. This is correct, of course. If you want to know which factors need controlling in a particular experiment, it's not enough to know the most general laws of, say, electromagnetism: you also need to know the low-level laws concerning such things as the magnetization properties of the materials used in the experiment.

So the second horn of the dilemma is the one Cartwright's opponent should grasp. Where is that horn's sting? According to Cartwright, the difficulty is simply that the low-level laws are too complicated: "In low-level, highly concrete generalizations, the factors are too intertwined to teach us what will and what will not be relevant in the new design" (p. 95; see also pp. 91-92). What is puzzling about this answer is that it doesn't have anything to do with the "Humean" idea that laws are strict regularities. According to Cartwright, it is just impossible to tell which factors need to be controlled for, and which factors don't, simply because the factors are so intertwined. But that is just as true for Cartwright as it is for her opponent. In fact, the difficulty posed here is even greater for Cartwright. For according to her opponent, the contribution to the effect made by each separate factor is governed by a strict regularity, the ways in which those factors combine to produce the effect is governed by a strict regularity, and there are no other factors "peculiar to the individual case" that in principle escape the net of theory. According to Cartwright, all three of these claims are false.[9] Their falsity can only make it more difficult to determine what needs controlling and how to control it. In conclusion, the two prongs of Cartwright's argument show that there are many difficulties that experimentalists must face, but they do not show that adopting her view of laws makes these difficulties any more tractable.

(vi) *The world as a messy place.* There is one argument left for the view that it is CP all the way down: "The world is an extremely complicated place. Therefore, we just have no good reason to believe that there are any non-trivial contingent regularities that are strictly true throughout space and time". The premise of this argument is undeniably true. But it is very hard to see how to evaluate the inference from the premise to the conclusion.

Strictly speaking, this argument supports (T7), rather than (T4). As an argument for the claim that our best current theories feature only CP laws (a claim made by Cartwright (1983, 1999), Pietroski and Rey (1995), Morreau (1999)) it is impotent. Moreover, considered as an argument for (T7), it strikes us as at best an expression of despair. We will argue below that there are no CP laws, so if (T7) is true, then physical theorizing as such is a doomed enterprise. And so it might be, for all any of us know. But, in the absence of any convincing reason to think that the inference from the premise of the above argument to its conclusion is a valid one, we see no reason to surrender to despair.

3. THE TROUBLE WITH CP-LAWS

There are two important objections to the claim that CP laws play an indispensable role in science. The first is that there seems to be no acceptable account of their semantics; the second is that there seems to be no acceptable account of how they can be tested.

The first objection is the weaker one in our view, and here we only touch on it briefly. It seems that there could be no informative account of the truth-conditions of CP law-statements that did not render them vacuous. One way to see the problem is to note that we could specify the conditions under which such a statement is true if and only if we could specify the conditions under which it is false, but that is exactly what we cannot do with a CP law-statement. For such a statement will be violated exactly when the regularity contained in it is violated *and* "other things are equal", i.e. there is no "interference". But we cannot specify the conditions under which the second conjunct obtains; otherwise the CP clause is simply an eliminable abbreviation and what we have is not a genuine CP law-statement. Nonetheless, many philosophers have tried to supply truth conditions for CP law-statements (e.g., Fodor (1991), Hausman (1992)), or at least conditions for their "non-vacuity" (Pietroski and Rey, 1995).

For specific criticism of these proposals, see Earman and Roberts (1999), Schurz (2001, 2002), and Woodward (2002).

This point is not fatal to CP laws, however. Perhaps it is unreasonable to demand truth conditions for CP law-statements. This could be because the concept of a CP law is a primitive concept, which is meaningful even though it cannot be defined in more basic terms. Or it could be because an assertability semantics or conceptual-role semantics, rather than a truth-conditional semantics, is appropriate for CP law-statements. Furthermore, one might well deny that it is necessary to have an acceptable philosophical account of the semantics for a given type of statement before granting that that type of statement plays an important role in science. And it is hard to deny that there are examples of statements qualified by CP clauses that seem to be perfectly meaningful.[10]

But the second problem with CP laws, their untestability, is decisive in our view. In order for a hypothesis to be testable, it must lead us to some prediction. The prediction may be statistical in character, and in general it will depend on a set of auxiliary hypotheses. Even when these important qualifications have been added, CP law statements still fail to make any testable predictions. Consider the putative law that *CP*, all Fs are Gs. The information that x is an F, together with any auxiliary hypotheses you like, fails to entail that x is a G, or even to entail that with probability p, x is a G. For, even given this information, other things could fail to be equal, and we are not even given a way of estimating the probability that they so fail. Two qualifications have to be made. First, our claim is true only if the auxiliary hypotheses don't entail the prediction all by themselves, in which case the CP law is inessential to the prediction and doesn't get tested by a check of that prediction. Second, our claim is true only if none of the auxiliary hypotheses is the hypothesis that "other things are equal", or "there are no interferences". What if the auxiliaries *do* include the claim that other things are equal? Then either this auxiliary can be stated in a form that allows us to check whether it is true, or it can't. If it can, then the original CP law can be turned into a strict law by substituting the testable auxiliary for the CP clause. If it can't, then the prediction relies on an auxiliary hypothesis that cannot be tested itself. But it is generally, and rightly, presumed that auxiliary hypotheses must be testable in principle if they are to be used in an honest test. Hence, we can't rely on a putative CP law to make any predictions about what will be observed, or about the probability that something will be observed. If we can't do that, then it seems that we can't subject the putative CP law to any kind of empirical test.

A number of philosophers have argued that, in spite of these difficulties, CP laws can be empirically confirmed and are confirmed regularly in the special sciences (e.g., Hausman (1992) and Kincaid (1996)). Here we will not consider in detail what any single author has written about this, but will consider a couple of the most common ideas and explain why we find them unsatisfying. (More detailed criticisms of specific authors can be found in Earman and Roberts (1999).)

One common view is that we can confirm the putative law that CP, all Fs are Gs by finding evidence that in a large and interesting population, F and G are highly positively statistically correlated. Such evidence would indeed, ex hypothesi, confirm the (precise!) hypothesis that in that large and interesting population, F and G stand in a certain statistical relation. But that is not a CP law. Why confirmation of this precise claim should be taken as evidence for the truth of the amorphous claim that "CP, all Fs are Gs", from which nothing precise follows about what we should observe, has never been adequately explained. Perhaps, under certain circumstances, confirmation of this statistical claim can also lend confirmation to the stronger claim that in some broader class of populations, F and G are positively statistically correlated. That would be interesting, but again, that would not be a CP law.

It has also been suggested that we can confirm the hypothesis that CP, all Fs are Gs if we find an independent, non-ad-hoc way to explain away every apparent counter-instance, that is, every F that is not a G.[11] 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 (e.g., in terms of its chemical structure and the way it interacts with the human nervous system); these explanations are not ad hoc, but can be supported by a variety of kinds of evidence; but none of this constitutes evidence for the hypothesis that it is a law that CP, white substances are safe for human consumption. It might be complained that whiteness is not a real property, and so is unfit to appear in a law of nature. But clearly, examples like this one are easily multiplied. Substitute "compounds containing hydrogen" for "white substances", and the example works just as well.

Perhaps the testing of a putative CP law requires both explanations of apparent counter-instances, and evidence that in some large and interesting population, F and G are highly correlated. Here again, such evidence would confirm a certain set of hypotheses – (a) a hypothesis concerning the statistical relations between F and G in a certain population, and (b) one or more hypotheses explaining why many Fs are not Gs. Such hypotheses could constitute valuable empirical information. But again, they would not be CP laws. Would anything of interest be added if, in drawing our conclusions, we didn't stop with announcing the confirmation of (a) and (b), but went on to add, "And what's more: CP, Fs are Gs"? We don't see what. More importantly, we don't see what could justify or motivate this addendum. Certainly, all the evidence is accounted for by (a) and (b) alone. Further, it seems that any counterfactuals licensed by the alleged CP law could be supported by (a) and (b) as well.[12] It might be argued that the CP law provides a *good explanation* for why (a) and (b) should be true, and is thus a warranted conclusion. But it would be a supposedly explanatory hypothesis which implies no predictions over and above the evidence it supposedly explains – neither testable predictions about what we may observe, nor even predictions about unobservable features of the world.[13] The addendum would thus seem to be both empirically and theoretically otiose.

4. WHY THERE DON'T HAVE TO BE CP LAWS IN THE SPECIAL SCIENCES

It is frequently argued that many of the special sciences have managed to articulate and confirm laws of nature that can only be interpreted as CP laws. So, if one wants to deny that there are any CP laws, then one is going to have to deny the manifest achievements of the special sciences.

This argument is over-hasty. Of course it is true that many of the special sciences have made impressive achievements in describing, predicting and explaining phenomena. And it is also true that most of the apparent laws one finds in the special sciences have exceptions, and cannot be rewritten in a finite form in which they are logically contingent and exceptionless. But all that follows from this is that doing justice to the special sciences requires recognizing an important and legitimate job that can be played by CP *law-statements*. It needn't follow that there must exist propositions or facts which it is the job of CP law-statements to state. There are plenty of important things that indicative-mood sentences can do other than state propositions or facts.

To see how CP law-statements could do important work even if there are no CP laws, consider the example:

(S) CP, smoking causes cancer.

If an oncologist claims that (S) is a law, then, we maintain, there is no proposition that she could be expressing (except for the vacuous proposition that if someone smokes, their smoking will cause them to get cancer, unless it doesn't), and even if there were, we wouldn't be able to test it. But we could come to know what she was getting at if she could say why she thought it was (S), rather than its contrary ("CP, smoking prevents cancer") that is a law. There are many things she could say about this. She could tell us that the probability of having lung cancer given that you smoke is higher than the probability of having it given that you don't. She might say that laboratory tests have shown that when certain compounds found in tobacco smoke are introduced into normal cells, they become cancerous at a higher rate than normal. And so on. The more such information the oncologist gives us, the more light dawns. Let "(I)" stand for this body of helpful information provided by the oncologist. None of the information in (I) is a CP law. It all consists of unambiguous, contingent empirical information that we know how to test using the techniques studied by standard confirmation theory.

Many philosophers of science would take (S) to be a hypothesis which is confirmed by (I). An alternative is to view the relation between (S) and (I) not as the relation between hypothesis and evidence, but rather that between an elliptical and imprecise expression of a large and unwieldy body of information, and a longer but more precise statement of that same body. On this view, the information adduced by the oncologist is indeed her reason for saying, "It is a law that, other things being equal, smoking causes cancer", but it is (part of) her *pragmatic* reason for producing a CP law-statement, rather than her *epistemic* reason for believing in the existence of a CP law.

There are other options as well. One could give a non-cognitivist account of CP law-statements, according to which the speech act of uttering such a statement is a way of expressing something which is not thereby asserted. For example, to say, "CP, all Fs are Gs" might be a way of simultaneously: (i) asserting that a great deal of precise empirical information has been gathered of the sort adduced by the oncologist in the above example; and (ii) expressing (but not asserting) that the speaker is committed to a research program that aims to explain all or most Gs in terms of Fs (together, perhaps, with other factors). The proposal is not that the CP law-statement is equivalent to the conjunction of (i) and (ii); if it were, then it would follow that if (ii) were false, then so would be the CP law-statement – but surely, anyone who asserts a CP law-statement does not mean to be asserting a proposition whose truth depends on her own commitments. Rather, the proposal is that a token utterance of a CP law-statement is a speech act in which (i) is asserted, and the information that (i) is true is pragmatically conveyed – just as a token utterance of "It is raining" pragmatically conveys the information that the speaker believes that it is raining, without asserting it. One advantage of this proposal would be that it would allow our oncologist to come to reject (S) even while continuing to maintain (I) – as she might do if she came to believe that the correlations noted in (I) were true but explicable in terms of a common cause that screens off the influence of smoking on cancer.

No doubt, there are other possible avenues for developing a pragmatist or non-cognitivist account of CP law-statements, without admitting the existence of any such proposition or fact as a CP law.[14] Pursuing some such strategy would have a number of important advantages. First of all, the strategy upholds the overwhelming appearance that the very idea of "CP laws" is either confused or vacuous, and does not (disastrously) require them to be empirically confirmable. Further, it recognizes a sense in which CP law-statements can be useful and important, and a sense in which they stand for important scientific achievements. It makes understanding the significance of any given CP law-statement a matter of knowing something about the details of current work in the science from which it comes, rather than a matter of doing philosophical analysis, or logic-chopping, on the "CP" clause. Most importantly, its availability shows that those who deny the existence of CP laws need not denigrate the achievements of the special sciences. Here we won't defend any particular version of this general strategy, which can be characterized by the slogan, "CP law-statements without CP laws". But it seems to be a hopeful strategy, whereas we have argued that the view that nature contains CP laws and science can discover them is hopeless.

5. CONCLUSION

Those enamored of CP laws typically assume that (1) the special sciences are incapable of establishing strict laws. They further assume that (2) to count as a genuine science, a discipline must be able to provide scientific predictions and scientific explanations of the phenomena in their respective domains. They also assume that (3) scientific predictions and explanations must be based on laws. And finally they assume that (4) the special sciences are sciences. They conclude that there must be CP laws and that the special sciences are capable of establishing them. Since this train of reasoning is valid and since we reject the conclusion, we must reject at least one of the four assumptions. We do not wish to question the fourth assumption. Earman and Roberts (1999) not only accepted the first assumption but gave an argument for it. Elliott Sober (private communication) has convinced us that our argument is vulnerable. Nevertheless, we continue to think that something in the neighborhood of the first assumption is true for most of the special sciences, at least insofar as they resist explicit reduction to physics. Of the remaining assumptions, our hunch is that the third is the most vulnerable and that it is worth devoting some effort to developing non-law based accounts of explanation and prediction.[15] The second assumption also deserves some critical examination, but an outright rejection of this assumption would bring into question the standard ways of trying to demarcate the genuine from the pseudo-sciences.

In sum, the way ahead is not clear. But what is clear is that our rejection of the entrenched views on CP laws has important ramifications for the philosophy of the special sciences. In particular, it points to a kind of pluralism. There is an important difference between fundamental physics and the special sciences with regard to the laws they discover and the forms of explanation they can produce. This, however, is no threat to the legitimacy of the special sciences.

NOTES

[1] In what follows we will use both "CP law statement" and "hedged law statement" to mean a statement of the form "CP (i.e. all other things being equal) Φ" where Φ is a strict law statement, i.e. a non-hedged proposition asserting a lawlike generalization of either universal or statistical form. A CP law is a law supposedly expressed by a CP law statement. The distinction between CP law statements and CP laws will become important in Section 4.

[2] The claim is typically allowed to stand unchallenged. To our knowledge, the only sustained attacks are found in Earman and Roberts (1999) and Smith (2002).

[3] Of course, it will be close to true even when there are some external stresses – as there usually are – but they are small. Deviations from the "law" for small stresses should be easily calculable and are not essentially problematic.

[4] A differential equation of evolution type is a differential equation with time as the independent variable, describing the evolution in the physical magnitudes of a given system over a given stretch of time.

[5] It is also not a law for the reason that the ellipses only result from the differential equation for some initial conditions, and initial conditions are generally taken to be non-nomic.

[6] This is obvious in that UG does not even involve time as a variable at all.

[7] Lange (this issue) suggests a plausible reason for denying the reality of the component electrostatic forces apparently governed by Coulomb's law. As Lange notes, the standard argument for the reality of the electric field does not carry over to the reality of the components of this field contributed by individual charged bodies. However, carrying over the standard argument for the reality of the electric field is not the only way to argue for the reality of component electrostatic forces. For example, in some cases, such forces can be measured; a torsion balance can be used to measure an electrostatic force by measuring the torsion force needed to counterbalance it.

[8] Of course, the "Humean" line just sketched assumes that component forces, such as the electrostatic force exerted on one body by another, exist, and that special-force laws are true regularities concerning such forces. Cartwright famously rejects these assumptions.

For example, she dismisses the very idea of force due to charge as “no concept for an empiricist”. We find this a peculiar argument in this context. In the same chapter, Cartwright acknowledges that she herself has “made the empiricist turn” (Cartwright, 1999, p. 81), yet she is happy to allow the existence of *capacities* possessed by systems *due to their natures*, which persist *even in conditions where they do not manifest themselves in observable behavior*. “We no longer expect that the natures that are fundamental for physics will exhibit themselves directly in the regular or typical behavior of observable phenomena” (ibid). If natures, together with the capacities they ground, persisting in objects even when they are not exhibited in observable behavior, are acceptable to an empiricist, then we wonder what is so bad about forces due to charges, which do not typically manifest themselves as the whole resultant force responsible for observable motion, but which can be measured in certain controlled situations.

[9] Concerning the third, see Cartwright (1989, p. 207).

[10] For example, Lange’s example concerning officers in the British navy, discussed in his paper in this volume.

[11] This is the proposal of Pietroski and Rey (1995). Lange (this volume) suggests that CP laws can be testable because we can find genuine counterexamples to them, by finding counter-instances that are clearly not covered by the CP clause and can only be excused in an ad hoc way. It seems to us that our response to Pietroski and Rey applies to Lange’s proposal as well, though it would take more work to show this in detail.

[12] Schurz (2001) argues that in a deterministic world such explanations are always forthcoming.

[13] Consider the counterfactual: “If the water in this cup had been drawn from the Atlantic Ocean, then it would be salty”. One can support this counterfactual by pointing out that (a) almost all samples of water drawn from the Atlantic are salty, and (b) almost all such samples that *aren’t* salty aren’t salty because they have been subjected to a purifying process (which we know that the water in this cup has not been). (If this water *has* been subjected to such a process, then the counterfactual is presumably false.) There is no need to back up the counterfactual by alleging that it is a law of nature that, CP, water drawn from the Atlantic is salty. It is plausible that counterfactuals supported by alleged CP laws could also be supported in ways similar to the one just illustrated.

[14] Lange (2000) articulates and defends a sophisticated theory according to which laws are not regularities at all, but rather encode rules of inference that belong to “our best inductive strategies”. One of the advantages Lange claims for his account is that it handles CP laws very naturally, since a rule of inference might fail to be truth-preserving in all cases yet still from part of a very good strategy for studying the world. Two of us (Earman and Roberts, 1999, pp. 449–451) have criticized Lange on this issue. However, we now think that Lange’s account might be exactly right for the case of *CP* laws. It is a “non-cognitivist” account of laws in the sense that it takes laws to express normative features of our scientific practice rather than to describe the (natural, non-normative) features of the world. We disagree with Lange, however, on the topic of the laws of fundamental physics.

[15] Here we applaud the efforts of Woodward (2000, 2002).

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