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CETERIS PARIBUS, THERE IS NO PROBLEM OF PROVISOS

ABSTRACT. Much of the literature on *ceteris paribus* laws is based on a misguided egalitarianism about the sciences. For example, it is commonly held that the special sciences are riddled with *ceteris paribus* laws; from this many commentators conclude that if the special sciences are not to be accorded a second class status, it must be *ceteris paribus* all the way down to fundamental physics. We argue that the (purported) laws of fundamental physics are *not* hedged by *ceteris paribus* clauses and provisos. Furthermore, we show that not only is there no persuasive analysis of the truth conditions for *ceteris paribus* laws, there is not even an acceptable account of how they are to be saved from triviality or how they are to be melded with standard scientific methodology. Our way out of this unsatisfactory situation to reject the widespread notion that the achievements and the scientific status of the special sciences must be understood in terms of *ceteris paribus* laws.

1. INTRODUCTION

It is often maintained that certain putative laws of nature are not strictly true unless qualified by a proviso to the effect that nothing else interferes, where what would count as an interference cannot be stated explicitly.[1] For example, consider the "law" that when the demand for a product increases while supply remains constant, the price of that product will increase. Stated thus baldly, the generalization is too strong, for there are numerous possible situations in which it would fail to obtain, such as cases of mass irrational behavior, widespread ignorance of the demand on the part of vendors, natural disasters that interfere with the normal working of the market, etc. It seems that the most we can say is that when demand increases while supply remains constant, price will increase, *unless something interferes*, i.e. "so long as other things are equal". But in this case, there seems to be little hope of finitely characterizing the class of events that would count as an interference. So it seems that our "law" is stuck with an irredeemably vague clause, something that one might have thought has no place in the statement of a law of nature. Such clauses are generally called *provisos* or *ceteris paribus clauses* in a growing literature on the topic.

The recent literature on provisos and *ceteris paribus* clauses is in agreement that such qualifications to putative laws of nature pose an important

Synthese **118:** 439–478, 1999.

and unresolved problem.[2] There the agreement ends. Disagreement reigns with regard to the scope of the problem, its implications for the concept of a law of nature and for the status of the disciplines that employ *ceteris paribus* constructions, and the reign even extends to the very formulation of the problem. While views on the problem have proliferated rapidly, we think that little real progress has been made toward its resolution, and this seems to us an indication that the problem has been ill-conceived; indeed, we contend that it is in need not so much of a solution as a dissolution. Since our position is bound to be controversial, we will proceed towards it by a careful plod through various attempts to deal with provisos and *ceteris paribus* laws.

In Section 2 we briefly review Lange's (1993a) attempt to state the problem of provisos as a dilemma whose horns offer either falsity or triviality. In Section 3 we review Hempel's (1988) analysis, which supposedly inspired Lange's dilemma. We argue that Hempel has been widely misunderstood; in particular, he did *not* give aid and comfort to those who claim that it is provisos all the way down to fundamental physics. At the same time, however, we argue in Section 4 that Hempel's insight (together with other plausible premises) entails that the special sciences, insofar as they remain autonomous disciplines, cannot formulate strict laws of their own. This conclusion is widely endorsed by many commentators who further conclude that the special sciences must employ *ceteris paribus* laws. They are thus obliged to confront Lange's dilemma. In Sections 5–10 we review various responses to this dilemma, all of which are found wanting. In Section 11 we argue that it is a mistake to try to provide truth conditions for *ceteris paribus* laws. When various confused and illegitimate senses of "*ceteris paribus*" are peeled away, the valid core of what is left of the problem of provisos and *ceteris paribus* clauses is a scientific, not a philosophical problem. In Section 12, we consider the nature of hypothesis-testing in the special sciences in the light of the preceding arguments. Concluding remarks are offered in Section 13.

2. AN ATTEMPT TO STATE THE PROBLEM OF PROVISOS

Lange (1993a) attempts to give a compact statement of the problem of provisos in the form of a dilemma which is attributed to Hempel (1988).

> For many a claim that we commonly accept as a law statement, either that claim states a relation that does not obtain, and so is false, or is shorthand for some claim that states no relation at all, and so is empty [because of the open-ended and ill-defined proviso (' . . . provided that other relevant factors are absent') needed to protect the claim against counterexamples]. (235)

Applied to the "law" with which we began, the problem is clear: The statement that price *always* increases when demand rises while supply remains constant is very probably false, so we face the first horn of Lange's dilemma. In order to make the statement true, we might add the clauses, "so long as no natural disasters interfere with the market", "so long as there is no sudden outbreak of irrationality", etc. But it is clear that this won't help, because the number of interfering factors that have to be excluded is indefinitely large, and there seems to be little hope of summing them all up in a finite formulation. So we might just add the clause, "so long as nothing interferes", but then the "law" threatens to become a triviality, asserting merely that in the circumstances described, price will increase unless it doesn't. Thus, we land on the second horn of Lange's dilemma.

For the moment, let us take the dilemma at face value and ask what its scope is. It is a commonplace that the discourse in the social sciences is riddled with provisos. But Lange doesn't intend his dilemma to apply only to the social sciences; indeed, he argues that it applies equally to the hard sciences, even physics. (One of Lange's examples: the law of thermal expansion, which says that when a metal bar is heated the expansion is proportional to the temperature change, requires a proviso to ward off counterexamples such as a bar that is heated but does not expand because someone is hammering on the ends.)[3] Kincaid (1996) concurs with Lange:

> *Ceteris paribus* clauses surely do plague the social sciences. That, however, does not separate them from the natural sciences, for *ceteris paribus* clauses are endemic even in our best physics.[4] (64)

This sentiment is fairly widespread in the literature (see, for example, Carrier (1998)).[5]

To get a feel for how difficult the problem is, let's do a little initial testing of the horns of the dilemma. Can we seize the first horn and maintain that genuine law statements must be true without exception or provisos and, thus, that scientists err when they attach the honorific "law" at the same time they attach a proviso or *ceteris paribus* clause? Lange thinks that the price for this move is too high. We must do justice to actual scientific practice, where proviso-ridden claims play the role of laws in that they are used to give explanations and to support counterfactuals. Should we then seize the second horn? This alternative appears even more unattractive since it seems to amount to endorsing claims of we-know-not-what.

Thus, if the advertisements are to be believed, we are faced with a pervasive problem that admits of no easy solution. We believe that there is indeed an interesting set of problems connected with provisos and *ceteris paribus* clauses. But we do not believe that anyone has succeeded in correctly diagnosing the problems. As a first step towards a diagnosis it is

crucial to be clear about what the problem of provisos is *not*. Towards this end, we will review the article by Hempel that inspired Lange's dilemma. When we do that it will become clear that Hempel's problem is not Lange's dilemma.

3. WHAT HEMPEL'S PROBLEM OF PROVISOS IS NOT

Hempel's (1988) discussion is couched in terms of the old fashioned "received view" of scientific theories, according to which a theory T is identified with a set of sentences which may be thought of as formulating the putative laws of the domain of the theory, as well as a set of "correspondence rules" relating terms in the observational vocabulary with the theoretical vocabulary. But Hempel gives this old view a new twist. Instead of bifurcating the non-logical vocabulary of the theory T into the observational and theoretical parts, he speaks of the antecedently understood terms (V_A) and the theoretical terms (V_C) first introduced with the theory T. (So, for example, physics might have arrived at a stage where 'electron' belongs to V_A while 'quark' belongs to V_C.) In this setting the Duhem–Quine problem amounts to the following. The idea of a hypothetico-deductive (HD) test of a theoretical hypothesis H which makes essential use of V_C, is to make predictions by deriving from H consequences stated purely in terms of V_A , and then to submit these predictions to the judgment of observation and experiment. But typically the derivation requires the help of auxiliary assumptions, with the (alleged) upshot that statements in the theory cannot be individually confirmed or disconfirmed but rather face the tribunal of experience as a corporate body.[6]

Hempel claims to have discovered a new twist to the Duhem-Quine problem: "The argument from provisos leads . . . to the stronger conclusion that even a comprehensive system of hypotheses or theoretical principles will not entail any V_A sentences because the requisite deduction is subject to provisos" (1988, 25). Hempel's claim is that typically a theory T of the advanced sciences will not have *any* logically contingent consequence S whose non-logical vocabulary belongs entirely to V_A. What we can hope to derive from T are consequences of the form $P \rightarrow S$, where again S is a logically contingent sentence whose non-logical vocabulary belongs entirely to V_A and P is a "proviso" that requires the use of V_C. If this is correct, then the instrumentalist conception of scientific theories, which views theories merely as handy devices for generating V_A predictions, is in deep trouble, a point that had already been stressed by Wilfrid Sellars, (1963, 1991), for reasons similar to Hempel's.[7]

Can we express Hempel's insight without using the suspect assumption of a bifurcation of the vocabulary of theory? Following the now fashionable semantic view of theories, let us think of a theory as a family of models, and let us call those features of a model that represent phenomena that are observable independently of that theory the *empirical substructure* of that model (more or less following van Fraassen (1980)). In this setting Hempel's insight, restricted to fundamental physics, amounts to the following:

(1) For a typical theory T of fundamental physics, there are no logically contingent conditions on the empirical substructures of the models that hold across all models; but there are logically contingent conditions on empirical substructures that hold across all models in which some proviso P is true, where P places constraints on features of models other than their empirical substructures.

It should now be clear that Hempel's provisos are not Lange's provisos. Like Hempel, Lange argues for a strengthening of the Duhem–Quine problem, but his strengthening is not Hempel's. According to Lange, the auxiliary hypotheses needed to derive empirical predictions from the theory must include an indefinitely large number of presumptions, which cannot all be made explicit at once.[8] By contrast, Hempel does not suggest that it is impossible to state all of the required auxiliary hypotheses. Here it is helpful to consider Hempel's discussion of the use of Newtonian mechanics and gravitational theory to make predictions for the motions of planets of our solar system by neglecting non-gravitational forces and extra-solar system gravitational forces as well. (Assume for purposes of illustration that 'force' and 'mass' belong to V_C while 'position' belongs to V_A.)

> [T]he envisioned application of the theory ... presupposes a proviso to the effect that the constituent bodies of the system are subject to no forces other than their mutual gravitational attraction. This proviso precludes not only gravitational forces that might be exerted by bodies outside the solar system but also any electric, magnetic, frictional, or other forces to which the bodies of the system might be subject. The absence of such forces is not, of course, vouchsafed by the principles of Newton's theory, and it is for this reason that the proviso is needed. (23)

Here the proviso can be made fully explicit in a finite form. For Hempel, the important moral has nothing to do with the length of the list of the necessary auxiliary hypotheses, but rather with the fact that these hypotheses must include conditions that cannot be stated without use of the special vocabulary of the theory (here 'force').[9]

More importantly, it should also be clear that Hempel's provisos are not provisos in the proper sense. By a *proviso proper* we mean a qualification without which a putative law would not be a law, not because it lacks modal force but for the more fundamental reason that it would be false unless qualified. (Recall Lange's thermal expansion example where the putative law is simply false if taken at face value without qualification.) Hempel's provisos are not provisos proper but are simply conditions of application of a theory which is intended to state lawlike generalizations that hold *without* qualification. Indeed, Hempel makes it explicit that his provisos are clauses that must be attached to *applications of a theory* rather than to law-statements,[10] in contrast to what we are calling provisos proper, which are clearly Lange's topic.[11]

This point is underscored by the fact that Hempel does entertain a doubt about whether all the provisos needed in his celestial mechanics example can be stated, but he quickly dismisses this doubt:

> The proviso must . . . imply the absence, in the case at hand, of electric, magnetic, and frictional forces; of radiation pressure; and of any telekinetic, angelic, or diabolic influences.
>
> One may well wonder whether this proviso can be expressed in the language of celestial mechanics at all, or even in the combined languages of mechanics and other physical theories . . .
>
> It might seem, therefore, that the formulation of the proviso transcends the conceptual resources of the theory whose deductive applicability it is to secure. That, however, is not the case in the example at hand. For in Newton's second law, $f = ma$, "f" stands for the *total* force impressed on the body; and our proviso can therefore be expressed by asserting that the total force acting on each of the two bodies equals the gravitational force exerted upon it by the other body; and the latter force is determined by the law of gravitation. (Hempel 1988, 30)

Lange claims that Hempel denies that the needed proviso can be given genuine content by the theory itself, because the proviso must rule out all "other relevant factors", and no theory contains a complete list of all relevant factors (1993a, 235). But as is clear from the quoted passage, in the celestial mechanics case, Hempel takes the second law to refer to the *total* impressed force on a body, and to imply that the total impressed force (together with the mass) determines the acceleration. So a proviso to the effect that all non-negligible forces have been taken into account does imply the absence of any other relevant factors. Of course, the theory at issue may not be *true* – there may *really be* other relevant factors besides those mentioned by the theory – but what is at issue here is only whether, *given the theory*, an indefinitely large and thus unstatable host of provisos is necessary.

We fully endorse Hempel's insight. But his example and its mode of presentation are unfortunate in two respects. First, it uses an idealization

(no forces acting other than gravitational forces) and/or an approximation (the total resultant forces on the planets are given to good approximation by the gravitational force component). Approximations and idealizations are widely used in physics, and their usage raises a host of important methodological issues. But Hempel's key point is independent of these issues. In the case at hand, it is in principle possible to do without any idealization or approximation: there is nothing to prevent the introduction of an explicit postulate into the theory which specifies the kinds of forces that occur in nature, and there is nothing in principle that prevents the exact specification of the values of each of these forces acting on the planets of the solar system (this specification would, of course, be a proviso, *in Hempel's sense*). Even so Hempel's key point stands: the said specification requires essential use of the V_C vocabulary. Hence, with or without idealizations and approximations, the theory by itself, without conditions of application stated in the V_C vocabulary, cannot be expected to yield non-trivial predictions stated purely in the V_A vocabulary.

Hempel's presentation appears to have misled Giere (1988), who argues that the semantic view of theories solves Hempel's problem of provisos. Giere's solution proceeds in two steps. First, take the problem of provisos to be about the role of idealizations and approximations; in particular, take it to be about (say) the role of the idealization in which there are only two bodies, the sun and the earth, acting on each other by Newton's $1/r^2$ law of gravitation. Second, claim that the problem is solved by taking Newton's laws of motion and gravitation to apply without proviso not to the messy world but to the tidy model in which there are only two bodies in the universe. But as we have urged, Hempel's key point does not concern idealizations and approximations. And even if we concentrate on the attempt to apply Newtonian theory via the indicated idealization/approximation, the semantic view hardly solves the application problem, for questions immediately arise as to the justification for using the model in question and as to how far it can be trusted to yield accurate predictions about the actual motion of the earth. Nothing in the semantic view of theories *per se* can answer these questions.[12]

The second respect in which Hempel's presentation is potentially misleading is that it has led some commentators to think that it is provisos all the way down to fundamental physics. Thus, Fodor has written that "considerations recently raised by C. G. Hempel make it seem plausible that there are *no* strict laws of nature ....." (1991, 21). But to repeat, the putative laws at issue in Hempel's example – Newton's second law of motion and his law of gravitation – are intended as strict laws which require no proper provisos. The notion that it is provisos all the way down

to fundamental physics can be motivated by the view that the world is a messy place and that we ought not to expect to find precise, general, exceptionless laws sans proper provisos. For all we know the world could be such a messy place. Our claim is only that – contra Lange (1993), Kincaid (1996), Cartwright (1983), and Pietroski and Rey (1995) – typical theories from fundamental physics are such that *if* there they were true, there would be precise proviso-free laws. For example, Einstein's gravitational field law asserts – without equivocation, qualification, proviso, *ceteris paribus* clause – that the Ricci curvature tensor of spacetime is proportional to the total stress-energy tensor for matter-energy; the relativistic version of Maxwell's laws of electromagnetism for charge-free flat spacetime asserts – without qualification or proviso – that the curl of the **E** field is proportional to the partial time derivative of the **B** field, etc. We also claim that the history of physics and the current practice of physics reveal that it is the goal of physicists to find such strict, proviso free laws. Obviously we cannot rehearse that history here, but we believe that a fair reading of it shows that when exceptions are found to the candidates for fundamental physical laws, and when the theorists become convinced that the exceptions cannot be accommodated by explicitly formulated conditions in the language of the theory, the search is on for new candidates.

We hasten to add that what we are describing applies only to a part of physics itself and certainly not to all of physics, much less to the majority of the sciences. Indeed, we will argue below that Hempel's insight plus some other plausible assumptions make it unlikely that exceptionless laws can be formulated in phenomenological physics much less in the special sciences.[13] For us, the irony is that although Hempel's problem is not the problem of provisos (proper), his insight shows that in the broad range of cases the problem of provisos cannot be escaped. But at the same time we think it important to take a stand against the now fashionable revisionism which holds that even the most fundamental laws of physics must be qualified by provisos or *ceteris paribus* clauses.[14]

4. HEMPEL'S INSIGHT AND THE NON-FUNDAMENTAL SCIENCES

We will now argue that if Hempel's insight is correct, then it is highly plausible that phenomenological physics, as well as the special sciences, will not be able to discover any general laws that hold without exception. Hence, if these sciences are to propose any laws at all, then these will apparently have to be *ceteris paribus* laws. (This conclusion may be thought to be obviously true. Nonetheless, we think it is interesting and worthwhile

to see how its plausibility is grounded by Hempel's insight, which, we have argued, is *not* the insight that it is *ceteris paribus* all the way down.)

Phenomenological physics and the special sciences take as their subject matter entities, properties, and processes that can be observed independently of any particular theory of fundamental physics. Thus, the pronouncements of these sciences will impose conditions only on the empirical substructures of the models of any theory of fundamental physics, in the sense described above. It then follows from Hempel's insight (HI) that any generalization that these sciences discover will not be true across all models of any of our fundamental physical theories. Their truth will not be guaranteed by the laws of fundamental physics, and in that sense they will be physically contingent. Thus, if we presume that phenomenological laws or special science laws must be reducible to or supervenient upon the fundamental laws of physics, and if Hempel's insight is correct, then there are no such laws. Of course, one might well reject the presumption. Even if there are no true generalizations of, say, economics that are guaranteed to hold by the fundamental laws of physics, there might still be true law-like generalizations about economic phenomena that have the right to be dubbed economic laws, perhaps because of the role they play in economic explanations.[15] Despite this objection, our conclusion, which we take to be a rather unsurprising corollary of Hempel's insight, enjoys widespread acceptance. In the remainder of this section, we want to illustrate how the corollary of Hempel's insight has been discovered and rediscovered, typically accompanied by great fanfare.

Consider, for example, John Beatty's (1995) evolutionary contingency thesis, according to which there are no "distinctively biological" generalizations that qualify as laws. Beatty admits that there are generalizations which apply to biological systems and which would seem to count as good candidates for laws by the usual criteria of nomicity. But he holds that they invariably fail to count as distinctively biological laws because insofar as they pass muster as laws they turn out to be wholly or largely generalizations of physics or chemistry. Beatty provides an elaborate analysis to support his thesis. We find his argument quite convincing, but, given Hempel's insight, the upshot is no surprise.[16]

Consider next Schiffer's (1991) example concerning folk psychology.

(2) If a person wants something, she'll take steps to get it.

Obviously this generalization will not stand without qualification since, for example, the person might have a stronger desire whose realization is incompatible with the realization of the first. Can all the needed qualifications be stated in the vocabulary of folk psychology or intentional

psychology? It seems unlikely, for as Schiffer notes, there will probably be many nomologically possible micro-physical conditions which defeat (2) but which do not correspond to anything recognizable as an intentional psychological state.

Cartwright (1995) has discovered the corollary in economics, although she comes at it from an entirely different perspective. Her slogan is that capacities are primary and regularities are secondary.

> Fixed patterns of association among measurable quantities are a consequence of the repeated operation of factors that have stable capacities (factors of this kind are sometimes called 'mechanisms') arranged in the "right" way in the "right kind" of stable environment. The image is that of a machine with set components that must be assembled and shielded and set running before any regular associations between input and output can be expected. In the case of economics we can summarize this way: *regularities are a consequence of the repeated successful operation of a socio-economic machine*. (277–278)

Adopting Cartwright's perspective we can ask: Can the conditions that capture the appropriate "shielding" of the economic machine be characterized purely in terms of economic variables (the money stock, the rate of deposits, etc.)? Cartwright thinks not (see her example on pp. 281–282). We agree. But we do not find this a surprising or profound discovery. Furthermore, adopting Hempel's insight allows us to embrace Cartwright's conclusion about special-scientific laws, while having a decent explanation of why this conclusion is true, without having to appeal to Cartwright's metaphysics of irreducible capacities (a topic to which we will return in Section 7).

5. ATTEMPTS TO COPE WITH *CETERIS PARIBUS* LAWS

It is generally conceded that there are no strict laws of the special sciences. A not uncommon response is that there must be *ceteris paribus* laws. Anyone who wishes to deny this response and maintain that there are no laws at all in the special sciences must cope with Pietroski and Rey's (1995) dilemma:

> [I]f one insists that the special sciences don't state laws, one must either (a) explain away the illusion that explanations like those just mentioned [using Darwin's law of fitness, Boyle's law, the law of supply and demand, etc.] avert to laws, explaining, moreover, how the special sciences can provide good explanations without having any laws to avert to, or (b) deny the immensely plausible claim that, a least sometimes, the special sciences sometimes provide good explanations. (85)

We will eventually confront this dilemma. But our initial strategy is to follow the main-line reaction in the literature to the effect that both horns

are too barbed for safe engagement and that a way must be found to cope with *ceteris paribus* laws.

The ways of coping are so varied that they defy neat classification. But for present purposes we will consider them in three categories. The first and most ambitious response is to provide truth conditions for *ceteris paribus* laws, various versions of which will be discussed in Sections 7–10. A second and less ambitious response is to decline to provide truth conditions for *ceteris paribus* laws but nevertheless to show how they escape the charge of vacuity. Pietroski and Rey's (1995) version of this strategy will be taken up in Section 6. A third strategy, not unrelated to the second, is to show how *ceteris paribus* laws can be integrated with standard scientific methodology. We will briefly review two versions of this response, due to Lange (1993a) and Kincaid (1990, 1996) in the present section.

Lange's proposal is that a proviso or *ceteris paribus* clause averts to a store of implicit knowledge that is possessed by the practitioners of the science at issue and that cannot, in principle, be made explicit all at once. Lange appeals here to the Wittgensteinian point that "to require that a rule be intelligible in the absence of implicit background understanding of how to apply it is not a reasonable criterion of completeness because no rule can satisfy it" (1993a, 241). We need a rule for applying the rule, and if it is demanded that this rule be made explicit, then we still need a further rule for applying that rule, and so on. "In the same way, a law-statement specifies a determinate relation only by exploiting implicit background understanding of what it would take for nature to obey this law" (ibid). The "rule" supplied by the thermal-expansion law is appropriately applied in some cases, not in others. In order to understand this "rule", one must know how to tell which case is at hand; for example, one must know that one ought not to apply the rule when the bar is being hammered forcefully at both ends. It isn't fair to require that all such instructions concerning when to apply the rule be made explicit, because this cannot be done for any rule. So the fact that a law is understood to have exceptions, not all of which can be made explicit, does not mean that the law is false or empty.

Let us grant for the sake of argument that the general Wittgensteinian point about rule-following is correct. The "rules" governing the use of words such as "game" or "plus" or "expansion" cannot be made fully explicit, because there is a regress of rules. You cannot explain to someone how to use these words properly unless they already have a large background knowledge of other linguistic rules that cannot all be made explicit. Still, Lange's view about laws does not immediately follow, because it is not clear that a law-statement is a "rule" in the same sense as these linguistic "rules". We take it that Newton's second law of motion is a uni-

versally quantified statement concerning the mathematical relation among the quantities mass, force and acceleration. To explain the rules implicit in the use of the terms "mass", etc., one would have to appeal to a store of background knowledge. But Newton's second law appears to be a statement that uses these terms, rather than an attempt to explicate the rules governing their use. We cannot specify these rules in a way that is "complete" in the sense that it presupposes no implicit knowledge of other rules. But it does not follow that we cannot make a statement (such as a law-statement) that leaves no escape-clauses. This is just because we can make a statement, and make it completely (in the sense that no unstated or vaguely specified exception-clauses are needed to make it true), without stating completely all the linguistic rules that govern the terms used in the statement. For example, we can completely state the proposition that a particular apple is red, in a way that doesn't allow for exceptions and escape clauses ("What I said wasn't false, because I didn't mean that the apple was red even if someone had painted it green!"), even if we cannot specify completely all the rules that you need to be able to follow in order to understand this proposition. A law-statement has a different logical form than the statement that a particular apple is red, but it still seems to be a *statement*, so it seems that the point applies to it as well.

Perhaps this appearance is deceptive, though. Lange makes a case for the novel[17] view that, despite appearances, law-statements really are *rules*, namely rules for drawing inferences, and that as such, they are affected by the familiar regress-of-rules argument. A large part of this case is constituted by his argument that otherwise, the problem of provisos brings us to grief.[18] In response to this argument, we note first that the view that laws are rules of inference rather than statements of fact is quite counterintuitive.[19] So it seems that a rather powerful case is required to support it. But as will emerge in the rest of this paper, we don't find the threat posed by the problem of provisos to be a very strong motive, since we deny that all laws of nature are afflicted by it (see Section 3), and we think that where it does seem to pose a problem, less revisionary moves are available (see Sections 11 and 12 below).

Before moving on, we want to register one more worry we have about Lange's solution. We take it that one of the important differences between science and pseudo-science is that scientists are expected to be capable of making their presuppositions explicit, bringing them into the light of day so that they can be tested. While we do not think that there is any bright red line between science and pseudo-science, we do take it as symptomatic of the pseudo-scientific status of astrology, for example, that claims such as "*Ceteris paribus*, birth time determines personality characteristics"

are defended against apparent counterevidence by the assertion that only the qualified practitioners of astrology have the tacit knowledge of when the supposed linkages do and do not hold. Lange shares this view, but we are unsatisfied by his account of the difference between science and pseudo-science: "What is noteworthy about science is that this background understanding is genuine background *understanding*. In general, all researchers identify the same testable claims as those to which one would become committed by adding a given lawlike hypothesis to a certain store of background beliefs. Because they agree on how to apply the hypothesis, it is subject to honest test" (Lange 1993a, 241–2). But it seems to us that an unspoken (and unspeakable) agreement among scientists about how to test a hypothesis does not yet guarantee that the tests are honest. Could not the scientific community as a whole capriciously and tacitly change what counts as an "interfering factor" in order to accommodate the new data as they come in (as the psychoanalytic community does, according to some critics of psychoanalysis)? This danger can be ruled out if we can say, in advance of testing, what the content of a law is, without recourse to vague escape clauses. Otherwise, we confess that we don't see how to rule the danger out. The fact (if it is a fact) that, afterwards, the scientific community generally forms a consensus about whether a rule was correctly applied, does not seem to do it. And if the danger cannot somehow be ruled out, then a proviso-ridden law-statement still threatens to become either false or trivial. So we hope that a different response to the problem of provisos is available.

We turn now to Kincaid (1990; 1996, Ch. 3) who provides an illuminating discussion of how *ceteris paribus* laws in the social sciences can explain and how they can be confirmed. In the end, however, we are left unsatisfied. Kincaid suggests that *ceteris paribus* laws are be able to explain because they can pick out tendencies construed as partial causes in a causal network. When a *ceteris paribus* law takes the form, "cp: all As are Bs", there is some plausibility to the notion that it functions to indicate that A is a partial cause of B.[20] But we fail to see how this notion applies to a law of the form, "cp: ϕ" where ϕ states a quantitative functional relation. And even in cases where the tendency or partial cause notion applies, we fail to see how it underwrites explanation. For as Kincaid himself acknowledges, a tendency may be present without being dominant; and unless the tendency is dominant the actual pattern of events need not be even approximately like the pattern that would obtain if the tendency in question were the only or the dominant factor present. Thus, if what one wants explained is the *actual* pattern, how does citing a tendency – which for all one know may or may not be dominant and, thus, by itself may or

may not produce something like the actually observed pattern – serve to explain *this* pattern?

Kincaid's nine suggestions for how to confirm *ceteris paribus* laws are too complicated to summarize here. But suffice it to say that while we find much good sense in these suggestions, we are not convinced that the important problem posed by *ceteris paribus* clauses, namely the problem of their apparent lack of determinate content, has been adequately dealt with. Two of his suggestions are that (i) one can sometimes show that in some narrow range of cases the *ceteris paribus* conditions are satisfied, and (ii) one can sometimes provide inductive evidence for a *ceteris paribus* law by showing that as conditions approach those required by the *ceteris paribus* clause, the law becomes more predictively accurate. But we do not understand how to implement these suggestions unless the *ceteris paribus* conditions are known or capable of being made explicit, in which case they can be incorporated into the law and the *ceteris paribus* qualification removed. The trouble with genuine *ceteris paribus* claims is precisely that the all-things-equal clause stands for we-know-not-what and, thus, that no definite claim is in the offing. To Kincaid's suggestion that one can provide evidence that there exists some mechanism connecting the variables in the purported *ceteris paribus* law, we reply that the problem here is just a junior version of the senior problem: unless "There exists a mechanism such that . . . " can be reduced to a definite, *non-ceteris paribus* claim, the notion of evidence pro and con loses its grip.

Our discussion of Kincaid in this section may leave the impression that we object to his account of hypothesis-testing and explanation in the social sciences as such. But this is not the case; our objection is only to Kincaid's claim that *ceteris paribus* laws can play a legitimate role in scientific testing and explanation, and we think this claim can be separated out from other important claims he makes. This should become clear when we return to the issue of hypothesis-testing in Section 12, below.

6. TRYING TO SAVE *CETERIS PARIBUS* LAWS FROM VACUITY

Pietroski and Rey (1995) attempt to show how *ceteris paribus* laws can be nonvacuous, without being so ambitious as to attempt to give truth conditions for such laws. They explicitly take for granted the legitimacy of a notion of scientific explanation, and a two-place relation among facts *x explains y*. They introduce the notion of *explanatory independence* $I(x, y)$ among facts as follows: $I(x, y)$ iff there exists a fact z such that x explains z, z is not a logical or analytic consequence of y, and z does not depend

causally on the occurrence of y. They consider *ceteris paribus* laws of the form:

$$\text{cp: } [(x)(Fx \rightarrow (\exists y)Gy)]$$

They then propose a sufficient condition for a statement of this form to be nonvacuously true.[21] Informally, their proposal amounts to the following:

cp: $[(x)(Fx \rightarrow (]\exists yGy)]$ is nonvacuously true if each of the following three conditions obtains:

(i) F and G are properties that can appear in legitimate law-statements (e.g., they are not grue-like, and perhaps they must make no essential reference to particular places, times, or objects);

(ii) For every x such that Fx, there exists a y such that either Gy, or else there exists a fact Hw distinct from Fx such that $I([Hw], [\sim Gy])$ and $[Hw]$, either alone or in conjunction with $[(x)(Fx \rightarrow (\exists z)Gz)]$, explains $[\sim Gy]$. (Intuitively: For every case that fails to conform to the law, there is some fact that explains this failure (either alone or in conjunction with the law itself), where this fact does some explanatory work independent of explaining this failure. This is to rule out viciously ad hoc appeals to dubious "facts" to explain away every failure of one's favorite theory.)

(iii) There is at least one concrete case of an x, such that Fx, and a y, where either Fx together with the law explains Gy, or there is some independent explanation of $\sim Gy$ as per clause (ii).[22]

The promising idea behind this account is that a *ceteris paribus* law can be nontrivially true, even if we don't cash out explicitly which "other things" need to be kept "equal", if for every occurrence of the antecedent of the law, either the consequent holds, or there is some independent interfering factor that can explain why the consequent doesn't hold. The requirement in clause (ii) that $I([Hw], [\sim Gy])$ is intended to require that the interfering factor $[Hw]$ is indeed independent, and is not simply an ad hoc "fact" cooked up to explain a particular failure of the law.

Unfortunately, conditions (i) - (iii) are not sufficient for the nonvacuous truth of the *ceteris paribus* law. To see why, let "Fx" stand for "x is spherical", and let "Gy" stand for $y = x$ and y is electrically conductive". Now, it is highly plausible that for any body that is not electrically conductive, there is some fact about it – namely its molecular structure – that explains its non-conductivity, and that this fact also explains other facts that are logically and causally independent of its non-conductivity – e.g., some of its thermodynamic properties. Thus, clauses (ii) and (iii) appear to be easily satisfied. If Pietroski and Rey's proposal were correct, then it would follow that *ceteris paribus*, all spherical bodies conduct electricity. More generally, whenever any object's failure to exhibit property G can be explained by anything independent of whether the object exhibits property F, then Pietroski and Rey's proposal implies that *ceteris paribus*, anything

with property F also has property G. Surely this trivializes the proposal, so that it does not, after all, provide a sufficient condition for the nonvacuous truth of a *ceteris paribus* law.

The general moral of this observation seems to be that it is not enough simply to require, as Pietroski and Rey do, that when cp: $(A \rightarrow B)$, any case of A accompanied by $\sim B$ must be such that there is an independent explanation of $\sim B$. This is because this requirement does not guarantee that A is in any way relevant to B, which surely must be the case if cp: $(A \rightarrow B)$ is a law of nature. Perhaps Pietroski and Rey's proposal could be modified to remedy this defect. But we do not see how to do this other than by requiring that the antecedent of the law be relevant to its consequent, in a previously understood sense of "relevant". It is not clear to us that the relevant sense of "relevant" would not depend on a previously understood concept of a *ceteris paribus* law, rendering the account circular. Of course, one could simply take the required notion of relevance as a primitive, but this strikes us as a very unattractive move, since we take it that the kind of relevance in question is something we understand by way of our notion of a law.

7. PROVIDING TRUTH CONDITIONS: CARTWRIGHT'S ACCOUNT

We have considered attempts to cope with *ceteris paribus* laws by means of explaining how they can play a legitimate role in scientific practice, and by showing how they can be non-vacuous, and have found these attempts wanting. Now we turn to more ambitious projects, which seek to come to terms with *ceteris paribus* laws by specifying their content or truth conditions. In this section we deal with a proposal developed in many writings by Cartwright.

Cartwright (1989, 1995, 1997) argues that the law-statements formulated by the sciences, if construed as statements of regularities in the course of events, are not true without qualification. If construed as generalizations about how empirical phenomena unfold, they must be construed as true only *ceteris paribus*. However, she holds that this way of putting the matter obscures the true role of laws in science, because she claims that law-statements (and in particular, purported statements of fundamental laws) should not be interpreted as statements of regularities or generalizations about the course of events. Rather, they are attributions of capacities and tendencies[23] to various kinds of systems; in a nutshell, "cp: $(x)(Fx \rightarrow Gx)$" is true just in case all Fs have a capacity or tendency to be G, so that they will be G in (the rare) cases where there are no other capacities or tendencies acting on them. Furthermore, she argues that such attributions

do not entail any strict regularities about how empirical phenomena unfold. So statements that purport to assert lawlike regularities in the observable course of events can be true only if qualified by a *ceteris paribus* clause. This is, she claims, equally true of physics and the social sciences.[24] We will object to her argument that laws, in her sense, do not imply any regularities that hold without *ceteris paribus* qualification, and to her claim that attributions of capacities that do not imply any such regularities can be empirically confirmed and play an important role in empirical science.

For Cartwright, a typical law says that systems of kind *A* have a certain capacity *C*, and such a claim does not entail any regularities concerning the behavior of *A*s, because the way in which any particular *A* will behave depends on what other capacities it has, what capacities are possessed by the systems with which it interacts, and the ways in which all these capacities interact and interfere with one another.[25] For example, if we accept as a law the proposition that a magnet has a capacity to attract steel, nothing follows about what will happen if we place a magnet near a steel paper clip; what will happen will depend on what other factors are in play. The most that we can infer from our law is that the paper clip will be drawn to the magnet unless some other capacity interferes with the attractive capacity of the magnet in such a way as to prevent this.

But all that this argument shows is that whatever regularities we can infer from the law will have to be stated in a vocabulary that includes terms referring to other capacities. This point is very similar to Hempel's insight, which tells us that the laws of a theory will not imply any regularities that can be stated without using the vocabulary of the theory, which will include the vocabulary we need to discuss the capacities of the magnet and perhaps other capacities as well. (Indeed, Cartwright notes a strong parallel between her argument and an argument due to Sellars, which is the same argument that we have already noted is mirrored by Hempel's (1988) argument.[26]) This kind of consideration does not show that, if we avail ourselves of a rich theoretical vocabulary that allows us to refer to capacities and other theoretical items, we will still be unable to state laws that imply strict regularities governing the course of events.[27]

However, Cartwright thinks that we will not be able to state such laws, even if we allow ourselves to refer to capacities. She asserts that any real natural system will be subject to the influences of a set of capacities that cannot, in principle, be covered by any of our scientific theories, or even by all of our theories put together.[28] If we attempt to formulate a regularity that will allow us accurately to predict the behavior of a given system of a given kind in a certain set of circumstances, we can begin by enumerating all of the capacities that, according to our theories, a system of this kind

possesses, as well as the capacities that, according to our theories, are possessed by the other systems with which this system interacts, and all of the laws we have on hand that concern these capacities and how they interact with one another. This process might go as follows: We start by writing down a regularity that describes how the system would behave if only one of its capacities were in operation; then, one by one, we make the corrections to this regularity that are called for by the other capacities in play and the laws concerning these capacities; but in the end, our corrected regularity will not be corrected enough, because there will always be further capacities (or perhaps other interactions among capacities), specific to the given context, that are not and in principle cannot be covered by any theory.[29]

We do not know how to begin to assess Cartwright's claim about context-specific factors that in principle elude theoretical treatment; it appears to be a very strong metaphysical thesis concerning the disorderliness of the world (perhaps motivated by her Anglocentric theological view that "God has the untidy mind of the English"[30]). But consider what would follow if it were true: none of our theories, and not even all of our theories taken together, would suffice to make a reliable prediction of any course of observable events. In fact, it appears that any course of events would be compatible with any set of laws (understood in Cartwright's sense, as attributions of capacities and tendencies), for any deviation from what one might have expected given those laws could be explained away as the result of context-specific factors not captured by the net of theory. Given this, it is difficult to see how laws, as Cartwright understands them, can be used for making predictions or giving explanations, and it is far from clear how hypotheses about such laws could be confirmed. (Cartwright appeals to Glymourian bootstrap methodology as a way of showing how claims about capacities can be confirmed: we confirm new hypotheses about capacities relative to a background theory that already includes other claims about capacities. But it is hard to see how this solves the present problem; if the background theory consists of claims about capacities that entail no observable regularities, and the hypothesis to be tested is also such a claim, then how can any testable consequences be derived from the background theory together with the hypothesis?)

The arguments we have criticized have a place in a complicated and subtle view of the way that science works, and we have little doubt that Cartwright can produce an interesting response to our objections, but we do not see how a satisfactory response could go. So we tentatively conclude that the arguments of Cartwright just discussed provide neither a good reason for thinking that the laws of nature do not entail any strict regularit-

ies concerning the course of events, nor a satisfying way of understanding how "laws" that do not entail such regularities can play a role in science.

8. PROVIDING TRUTH CONDITIONS: SILVERBERG'S ANALYSIS

A recurring complaint against *ceteris paribus* claims is that they have "no clear meaning" (Hutchison 1938, 1965). Silverberg (1996) takes this complaint to be equivalent to the charge that *ceteris paribus* claims are semantically defective, and he seeks to rebut the charge by providing a possible-worlds semantics for them. His account makes use of David Lewis's (1973) notion of a relation, defined over possible worlds, of comparative similarity to the actual world. Whereas this relation is rarely specified explicitly, Lewis contends that in any given context, such a relation is implicitly in use. Silverberg further proposes that we distinguish between worlds that are appropriately idealized and those that are not, where what counts as an appropriately idealized world is fixed by pragmatic factors depending on the context of scientific practice in which a *ceteris paribus* law is stated. On Silverberg's account, it is a law that cp: (if *A* then *B*) just in case *B* is true in all possible *A*-worlds that are appropriately ideal and that are otherwise most similar to the actual world.[31]

Silverberg's analysis is targeted at *ceteris paribus* claims that involve idealizations. This covers many examples of *ceteris paribus* laws from economics, where appeal is made to perfectly rational agents, perfect information, perfect markets etc. But not all of the cp laws of the special sciences are of this sort, and in general the problem of *ceteris paribus* qualifications is distinct from the problem of idealizations. Often the idealization can be stated in a precise closed form (e.g., the ideal gas law assumes that the gas molecules have no volume and interact only by contact). Here the problem is not in saying precisely what is involved in the idealization but in relating it to the real world which is not ideal. By contrast, many cp laws claim to be about unidealized real world situations but make indefinite claims about these situations. This leads to our second qualm about Silverberg's analysis. When Hutchison charges that *ceteris paribus* claims have "no clear meaning", his complaint is not that there is no respectable semantics for such claims but rather that they are pseudo-claims because they make no definite assertions and, thus, cannot be used in science to make predictions and cannot be confirmed or disconfirmed by the usual methods of scientific inquiry. Providing a possible-worlds semantics for them of Silverberg's type may provide us with a way of understanding their truth conditions, and hence how they can make a definite

claim. But it does not yet show us how the definite claims they make can be tested empirically, so it seems that it doesn't show how they can play any role in empirical science.

9. PROVIDING TRUTH CONDITIONS: FODOR'S ANALYSIS

Fodor (1991) develops an analysis of the truth conditions of *ceteris paribus* laws that presupposes the notion of a law simpliciter. We think that his proposal, combined with a helpful suggestion by Silverberg (1996), is promising, despite objections raised against it by both Silverberg (1996) and Mott (1992). Fodor considers putative laws of the form, cp: $(A \rightarrow B)$, where A is a functional state, e.g., a psychological state, which can be realized by a number of non-functional states that may be called its *realizers*. If R is a realizer of A, then in Fodor's terminology, if a condition C is such that an occurrence of R together with C is nomologically sufficient for B, but C alone is not, then C is a *completer* for R and the law cp: $(A \rightarrow B)$. Fodor considers the proposal that cp: $(A \rightarrow B)$ is true if and only if every realizer R of A has a completer for R and cp: $(A \rightarrow B)$. But he rejects this because of an argument of Schiffer (1991) to the effect that for any psychological state A and any typical, plausible psychological law cp: $(A \rightarrow B)$, there are likely to be realizers of A for which there are no completers. So Fodor instead proposes that:

(3) cp: $(A \rightarrow B)$ is true if (and perhaps only if): Every realizer R of A either has a completer C for cp: $(A \rightarrow B)$, or else it has a completer for sufficiently many other laws with A in their antecedents.

Against proposal (3), Mott (1992) has argued that it trivializes the notion of a *ceteris paribus* law. For example, consider the ludicrous statement that *ceteris paribus*, if a person is thirsty, then she will eat salt. It seems plausible that many (and probably all) neurophysiological realizers of the state of being thirsty will lack completers for this law. However, each of these realizers will presumably have completers for a great many other psychological laws involving thirst. Hence, this ridiculous non-law will achieve the status of a *ceteris paribus* law by piggybacking on all of the legitimate laws.

Silverberg proposes an amendment to Fodor's (3) designed to get around this objection. The amended proposal is as follows:

(4) cp: ($A \rightarrow B$) is true if (and perhaps only if): Every realizer R of A is such that either R has a completer C for cp: ($A \rightarrow B$), or else both: (i) R has a completer for sufficiently many other laws with A in their antecedents, and (ii) sufficiently many other realizers of A do have completers for cp: ($A \rightarrow B$).

This seems to evade Mott's objection. In the case of the putative law that *ceteris paribus*, thirsty people eat salt, very few (if any) realizers of the antecedent will have completers, so the putative law will not be able to piggyback its way to lawhood.

Silverberg, however, is not satisfied with (4) because he objects to the use of the phrase "sufficiently many", which he suggests is just as vague as the phrase "*ceteris paribus*", undermining the usefulness of (4). While we share this qualm, we also think that Fodor's analysis points to an important feature of *ceteris paribus* laws. But before tying to spell this out, we will review one more attempt to provide truth conditions.

10. PROVIDING TRUTH CONDITIONS: HAUSMAN'S ANALYSIS

Hausman (1991) maintains that "*ceteris paribus*" has an invariant meaning – namely, other things being equal – whereas the property or proposition it picks out varies with the context. He proposes that "cp: (Every F is a G)" is true in context X just in case X picks out a property C such that "Everything that is both F and C is a G" is true. Further, "cp: (Every F is a G)" expresses a law just in case the cp clause determines a property C in the given context such that "Everything that is both F and C is a G" is a law in the strict sense.[32]

The first worry we have about this proposal is that it is at once too strong and too weak. In Fodor's terminology, Hausman's C is a completer.[33] Schiffer's (1991) and Fodor's (1991) point, which strikes us as correct, is that for the *ceteris paribus* laws of psychology one cannot expect that there will be an appropriate completer C that covers every physically possible case; hence Hausman's account is too strong. But even if in the case of some particular law there is a completer for every physically possible situation, this is not very helpful from the point of view of psychology if C obtains only rarely among the intended applications of the (would be) psychological law; hence Hausman's account appears to be too weak, since it seems to require an additional clause to the effect that C obtains commonly among the intended applications.

Our second worry is more general. It concerns the question of how the context determines the content of the *ceteris paribus* clause. Hausman has little to say about this matter, but the vagueness of the proposal is not sufficient to hide the following problem. Hausman's analysis is intended to apply mainly to economics, and what he has in mind for the content of the *ceteris paribus* clause in, for example, cp: (if the price of a good falls, the demand for it will rise) in standard economic contexts is something like the condition that the prices and the tastes for other goods remain the same. But given Hempel's insight and given that the laws of economics (if there are any) supervene on the laws of physics, it is wholly implausible that the content of the *ceteris paribus* clause can be specified in purely economic terms so as to yield a strict law, as required by Hausman's analysis. And it is beyond plausibility that the economic context will pick out the fundamental physical properties needed to underwrite a strict law.

While we find Hausman's analysis of the truth conditions of *ceteris paribus* laws wanting, we find considerable merit in his discussion of the factors, such as reliability, that make it reasonable to believe that a *ceteris paribus* generalization expresses a law.[34] We will make use of these merits in our own proposal.

11. *CETERIS PARIBUS* LAWS: THE PROBLEM THAT ISN'T AND THE PROBLEM THAT IS

Contemporary philosophy of science has a healthy naturalistic tenor: the job of philosophy is not to issue normative dicta to scientists but to reconstruct and clarify what actually goes on in science. It seems to follow that, to the extent that *ceteris paribus* claims are employed in some special science, it behooves the philosopher to provide an analysis on which these claims play a respectable role in that science. While sharing the general naturalistic orientation, we think that the attitude is misapplied in the present case. In a nutshell, our position is that there is no distinctively philosophical problem about *ceteris paribus*, but there is a scientific problem: what is needed is not finer logic chopping but better science. Since this position is bound to be controversial, we will approach it indirectly through some logic chopping of our own.

Using insights gathered from the failures of the analyses reviewed above, we will propose a set of truth conditions for *ceteris paribus* laws. Let ϕ be a lawlike generalization of X, where X is some special science; that is, the non-logical vocabulary of ϕ is appropriate to X, and ϕ satisfies the usual criteria for lawlikeness. We seek conditions for "cp: ϕ" to be a 'law of X', where the scare quotes indicate that "cp: ϕ" may not be a strict

law but that, nevertheless, it has features that imply that it functions like a law in X. Our task falls into two parts: (A) specifying the conditions for "cp: ϕ" to be true for X, and (B), specifying what additional conditions are needed for "cp: ϕ" to be a 'law of X'. We begin with the second subtask.

Our idea is that the additional condition needed for ϕ to be a 'law of X' is for ϕ to have for X an analogue of the feature that allows us to identify something as a strict law. This is an obvious sticking point since the philosophical literature is badly at odds on how to analyze the notion of a strict law of nature. Purely for purposes of illustration, suppose that David Lewis's (1973) "best system" analysis is correct: a strict law is an axiom or a theorem of the deductive system (deductively closed and axiomatizable set of true sentences) that achieves the best balance between strength and simplicity.[35] Assuming that this is right, then for "cp: ϕ" to be a 'law of X' we want it to be the case that there are other lawlike generalizations $\phi'\phi''$, ..., of X such that "cp: (ϕ & ϕ' & ϕ''' & ...)" is true for X and such that if all these ϕs were true and if the world were completely described in the vocabulary of X, then the ϕs would form the axioms of the best deductive system. If some other account of lawhood is preferred, then the story here will have to be different, but we maintain that whatever distinguishes *ceteris paribus* laws from merely contingent *ceteris paribus* generalizations is just whatever distinguishes strict laws from contingent strict generalizations.

The first subtask turns out to be more difficult since, we contend, there is no univocal sense of "cp: ϕ". The ragged character of the philosophical literature on this topic is explained in part by the fact that it tries to treat under one umbrella several different usages. For example, there is the *lazy sense* of *ceteris paribus*, as in Lange's example of "cp: (if a metal bar is heated uniformly, its expansion is directly proportional to the difference in temperature before and after heating)". We contend that when physicists assert the heat expansion law they are implicitly assuming that there are no external stresses acting on the bar. If so, this assumption can be explicitly incorporated into the generalization, obviating the need for a *ceteris paribus* qualification.[36] We assume subsequently that the *ceteris paribus* qualification is not being used in this lazy sense. We also exclude the *improper sense*, as in "cp: (any two oppositely charged particles attract each other with a force inversely proportional to the square of the distance between them"). We have maintained that "*ceteris paribus*" is out of place here because what physicists intend to assert is the unqualified strict law that two oppositely charged particles *always* exert an electrical force on one another of the form indicated.[37] We also think that it is improper to classify Boyle's law as a *ceteris paribus* law. The obvious things to say here

are that it not a law because it is false; that it is false because it is based on unrealistic idealizations (e.g., that the gas molecules have no volume); but that, nevertheless, for some gases and some pressure-temperature ranges the idealization provides an approximation that is good enough for most applications. Nothing is gained or clarified by slapping on an "others things equal" clause.

As for the proper and non-lazy senses, the literature reveals two main strands that give rise to a weak and a strong sense of "*ceteris paribus*." We begin with the weaker one, which can be described informally as follows: "cp: ϕ" is a truth of the science X just in case there is an important class of cases in which ϕ is true, and systematic violations of ϕ cannot be produced, at least not using the techniques appropriate to X. More formally, we define this weak sense of "*ceteris paribus*" in two clauses. (i) There should be no condition ψ which can be stated in the language of X, which may not occur "naturally" but which can be realized using the techniques of X, and which defeats ϕ. By "defeats ϕ" we mean that when ψ obtains, ϕ is not even approximately true for X. Here we are following Mott (1992) who has emphasized that if violations of ϕ can be systematically produced, then "cp: ϕ" is not a law in any interesting sense; indeed, "cp: ϕ" is not even true. Of course, if such a factor ψ is discovered, then ϕ could be modified to $\hat{\phi} = \sim \psi \rightarrow \phi$. But this could cause trouble if the best-system analysis is accepted, since the move from a system that entails ϕ to one that entails $\hat{\phi}$ but not ϕ may represent a loss of both strength and simplicity and so $\hat{\phi}$ may not qualify as a 'law of X' on this analysis (and of course, it is possible that this move will cause problems given other analyses of lawhood as well). As for approximate truth we wish to say only that while there is no good analysis of approximate truth in general, in typical concrete cases of quantitative relations, scientists have no trouble in making precise what it means for such a relation to be approximately true.[38] The qualification "for X" in "approximately true for X" is added to emphasize that judgments of approximate truth are to be made relative to the kinds of measurements and empirical data available in X (e.g., in phenomenological physics, the data concerns macroscopically discernible states and changes of state). We are not committed to drawing unbreachable boundaries between sciences in terms of vocabulary and techniques; indeed, we think that it is conceivable for just about any science to incorporate vocabulary and techniques from other sciences. But we are committed to the view that a *ceteris paribus* law involves implicit reference to boundaries drawn in this way and, thus, that what counts as a *ceteris paribus* law for X changes as these boundaries shift.[39] Further, in the spirit of Hausman (1991), we require that (ii) there be conditions θ such that when θ obtains, ϕ is true or approximately true

for X. Since we are not dealing with strict laws, we cannot argue, as we did in Section 4, that Hempel's insight implies that θ cannot be stated in the vocabulary of the special science X. Nevertheless, we think that if ϕ is not only a 'law for X' but is in some appropriate sense near a strict law, then the conclusion will continue to hold.

This *weak sense* of "cp: ϕ", captured by the conjunction of (i) and (ii), is compatible with θ's obtaining only rarely in the intended applications of ϕ in X. If in addition a failure of θ to obtain defeats ϕ, then the weak sense of "cp: ϕ" has the unappetizing feature that "cp: ϕ" can be true for X even though ϕ gives (by the standards) of X, completely unreliable predictions for its intended applications. Even if θ obtains often in the intended applications of ϕ, there is little to comfort the practitioners of X if they are not in a position to determine when θ obtains and when it doesn't. And assuming the implication of Hempel's insight extends to ϕ, such a determination will involve theorizing about entities and processes that may be regarded as beyond the ken of X. If so, "cp: ϕ" degenerates into a kind of *wannabe* sense: here "*ceteris paribus*" is an implicit admission that X has not achieved reliable generalizations and, perhaps, also as an expression of a pious hope that a reliable generalization is to be found in the neighborhood.

Such reflections motivate the move to the *strong sense* of "cp: ϕ", which adds a third clause inspired by Fodor (1991) and Silverberg (1992): (iii) the condition θ of (ii) obtains in "most" of the intended applications of ϕ in X. The conjunction of clauses (i)–(iii) appears to be a good statement of what is generally intended by "*ceteris paribus*" in most of the cases considered by the philosophers we have been discussing: a *ceteris paribus* law (in the strong sense) is a generalization that plays some of the roles of laws in the science at issue, and that is not strictly true but that, nevertheless, is approximately true in most of its intended applications; extraordinary situations (e.g., for the case of psychological *ceteris paribus* laws, severe neurophysiological malfunction) may render the generalization false, but such situations are not among the intended applications, and in most of the intended applications, the generalization is reliable. But this strong sense of "*ceteris paribus*" obviously only has a determinate content when we have a reasonably clear sense not only of what "approximately true" (as used in (ii)) means, but also of what is meant by "most of the intended applications". We now wish to illustrate how determinate senses can be supplied to clauses (ii) and (iii) and how our analysis of the strong sense of "*ceteris paribus*" can be clearly satisfied.

The laws of classical phenomenological thermodynamics appear to satisfy conditions (i)–(iii). One can be confident in making pronouncements

about clauses (ii) and (iii) because of the reduction of thermodynamics to statistical mechanics.[40] There are dynamically possible microtrajectories that produce permanently anti-thermodynamical behavior, but it is thought that the set of such trajectories is ignorable in the sense of being "measure zero" in the phase space of the system. It is also consistent with basic physical laws that fluctuations eventuate in temporary anti-thermodynamical behavior, but it is extremely unlikely that fluctuations will produce violations of the second law of thermodynamics, say, in the form decreases in entropy in a closed system, which can be detected on the macroscopic scale. Violations of the second law are detectable with the aid of a low-power microscope – for example, in Brownian motion. This led to the worry that small fluctuations could somehow be exploited by clever devices so as to systematically produce macroscopic violations of the second law in the form of a perpetual motion machine of the second kind that would output macroscopically usable work. However, the long history of failed attempts along these lines, plus some theoretical considerations, strongly indicate that clause (i) is safe.[41] Furthermore, it is clear that, by the account sketched above, we are justified not only in saying that the laws of phenomenological thermodynamics are *ceteris paribus* true in the strong sense, but also that they are *ceteris paribus laws*: the historical evidence clearly indicates that the laws of thermodynamics do function as laws of phenomenological physics. Further, in the 19th century when the laws of thermodynamics were thought to be strictly true and when many physicists were dubious of atomism and thought that the world could be fully described in phenomenological terms, the honorific "laws" was bestowed on these generalizations.

Note the important role played here by the availability of a micro-reduction: the reduction of thermodynamics to statistical mechanics is what makes it possible to give a clear sense to the crucial phrase, "most of the intended applications". We think that this will probably turn out to be true in general. Consider again the Fodor-Silverberg proposal (4). In order to make it clear that we have a "*ceteris paribus* law" in the sense of this analysis, we would need to have at least a sketch of a micro-reduction of psychology to neuroscience, so that we can make clear what the relevant "realizers" and "completers" are. If a sketch of such a reduction is supplied, then it might well turn out that there emerges a natural measure over the micro-states that play the role of realizers, so that a determinate sense can be given to the phrase "sufficiently many" as it occurs in the analysis. But the importance of reductionism is a side light to our main point, which is that in order for clauses (ii) and (iii) to make any determinate claim

about the world, a determinate sense must be given to the slippery notion of "most of the intended applications".

But there is obviously something strange about our illustration: scientists generally do not attach a *ceteris paribus* clause to the laws of thermodynamics.[42] Furthermore, in cases where *ceteris paribus* clauses are typically attached (e.g., in psychology and economics), it seems impossible to satisfy clauses (ii) and (iii) with any perfectly determinate sense of "most of the intended applications". This sets up the moral we wish to draw. We claim that (1) our strong sense of "*ceteris paribus*" captures the intuitive notion that philosophers usually have in mind when discussing *ceteris paribus* laws, and (2) this sense can be made precise, in a way that makes it clear what claim about the world is made by a *ceteris paribus* law so that it is clear how such a law can be confirmed and support predictions, only if a determinate sense can be given to "most of the intended applications", but (3) the clear cases where this demand can be satisfied are cases where the phrase "*ceteris paribus*" is not used by scientists and where its use feels out of place.

There *is* a clear sense to be given to the notion of a "near-law", i.e. a generalization that is not a strict law, but that deserves to be called a "near-law" because it is, in a precise sense, true or approximately true in almost all intended applications, because it plays the role of laws in giving explanations, supporting counterfactuals etc., and because it is clear that it makes definite claims about the world and can be confirmed or disconfirmed empirically. But, we claim, the most clear paradigms of such laws (viz. the laws of phenomenological thermodynamics) are not thought of as *ceteris paribus* laws, and statements that are thought of as *ceteris paribus* laws do not answer to this clear sense of a "near-law". Our conclusion is that the use of a *ceteris paribus* clause is a flag indicating that this kind of near-law has not been found, but that some vague but perhaps more-or-less useful generalization has been found, perhaps with the hope that a clear case of a near-law is "in the neighborhood". But since clauses (ii) and (iii) cannot be seen to be true in any determinate sense of "most of the intended applications", such generalizations do not make definite claims about the world, and so, we maintain, it is hard to see how they can be empirically confirmed or disconfirmed and what role they can play in making scientific predictions and giving scientific explanations.

In the light of this, we wish to make the following suggestion. "*Ceteris paribus* laws" are not what many philosophers have taken them to be, that is, they are not elements of typical scientific theories that play the same kinds of roles in the practice of science that less problematic statements such as strict laws or near-laws (in the sense just defined) play. Rather, a

"*ceteris paribus* law" is an element of a "work in progress", an embryonic theory on its way to being developed to the point where it makes definite claims about the world. It has been found that in a vaguely defined set of circumstances, a given generalization has appeared to be mostly right or mostly reliable, and there is a hunch that somewhere in the neighborhood is a genuine, well-defined generalization, for which the search is on. But nothing more precise than this can be said, yet. To revive a now-unfashionable notion, "*ceteris paribus* laws" belong to the context of discovery rather than the context of justification. And while we do not adhere to the old logical empiricist dictum that philosophy is to restrict itself to considering the context of justification, we do submit that the philosophical analysis that is called for in the case of a "work-in-progress" theory is probably quite different from that called for in the case of a "finished" theory that already makes definite claims about the world and so is a candidate for empirical confirmation or disconfirmation. In particular, whereas we see the need for a philosophical analysis of the truth conditions of strict law-statements and near-law-statements, and a philosophical account about how such statements can be confirmed, we do not see any such needs in the case of "*ceteris paribus* laws". This is because we maintain that such "laws" are only vague statements that partly constitute embryonic theories; they are not put forward as true (except perhaps in the vague and Pickwickian sense that something in their neighborhood is probably true) and there appears to be little interest in an account of what the world would have to be like for them to be true, or to be "*ceteris paribus*" true (whatever that might mean). While there are probably many interesting things to say about the relation between "*ceteris paribus* laws" and the evidence available in the sciences that sport them, we claim that this relation must be quite different from that studied in confirmation theory, where hypotheses that make definite claims about the world are tested against empirical data. Likewise, whatever explanatory power such "laws" have is probably quite different from that of precisely formulated law-statements.[43]

If laws are needed for some purpose, then we maintain that only laws will do, and if "*ceteris paribus* laws" are the only things on offer, then what is needed is better science, and no amount of logical analysis on the part of philosophers will render the "*ceteris paribus* laws" capable of doing the job of laws. Perhaps there are purposes for which laws are not needed, and "*ceteris paribus* laws" will serve,[44] but since we maintain that "*ceteris paribus* laws" are inherently vague and without definite truth conditions, we think it follows that in any such situations, a true account of the world is not needed. If there are such situations, then perhaps they are situations where the theories needed are best given an instrumentalist construal. But

we suspect that the main interest of "*ceteris paribus* laws" is that they are (hopefully) stations on the way to a better theory with strict generalizations (or at any rate, statements with precise contents). The challenge for philosophers of science here is to understand this process; it is tempting but dangerous to mistake the way-station for the destination, and to attempt to analyze "*ceteris paribus* laws" in a way that minimizes or obscures their differences from strict laws.

12. THE ILLUSION OF THE IMPORTANCE OF *CETERIS PARIBUS* LAWS FOR THE SPECIAL SCIENCES

We fear that our remarks in the previous section may seem to invite charges of "physics chauvinism". To say that if laws are needed, then "*ceteris paribus* laws" will not do, and if only "*ceteris paribus* laws" are in hand then we need better science, looks at first glance to be a negative judgment about the special sciences as compared with fundamental physics. In this section we shall try to dispel this appearance.

Toward this end, let us return to Kincaid's (1996) discussion of nine methods of confirming *ceteris paribus* laws. Again, we think that these methods embody much good scientific sense; but we wish to consider the question of what, exactly, is the content of the claims that are tested by such methods. One answer is that these methods test claims about actual correlations among variables across various populations. For example:

(5) In population H, P is positively statistically correlated with S across all sub-populations that are homogeneous with respect to the variables $V_1, \ldots, V_n$.

(5) does not suffer from the vagueness of "*ceteris paribus* laws" (so long as the variables P, S and $V_1 \ldots V_n$ are defined precisely enough). It asserts a certain precisely defined statistical relation among well-defined variables. Most of the methods described by Kincaid seem to be good methods for testing claims like (5). Recall that one of Kincaid's methods involves looking to cases in which "the *ceteris paribus* conditions are satisfied".[45] As we noted above, this requires that we be able to *tell* when these are satisfied. This suggests that the *ceteris paribus* clause is not a vague and open-ended "escape clause". The clauses Kincaid has in mind might well be taken to be such clauses as, "so long as variable V_i has been controlled for". In this case, the method Kincaid describes is essentially that of controlling for relevant variables, which is certainly a reasonable practice if one is concerned to test a claim of the form (5). Similar remarks apply to some of Kincaid's other methods.

Kincaid argues that his methods are exemplified in some good work in the social sciences. In particular, he provides an illuminating discussion of Paige's (1975) work on agrarian political activity.[46] If we let H stand for the population of humans between 1948 and 1970, P stand for degree of political activity among cultivators, S stand for economic relations among non-cultivators and cultivators in agrarian societies, and the V_i's stand for such factors as the proximity of progressive urban political parties, then (5) closely approximates the results of Paige's statistical analysis, which was carried out using many of Kincaid's methods. This at least suggests the idea that what really gets tested by these methods tends to be propositions about statistical distributions conditional on controlled variables, such as (5). Of course, much research in the special sciences produces results of just this form.

Kincaid argues forcefully that the results of careful applications of the methods he lists, such as Paige's work, can have great explanatory value. We agree, and in particular we think that this can be seen to be the case if we suppose that the results of such research typically take the form of (5). According to Kincaid, Paige's work allows us to infer much about the (partial) causes of particular political events, and that such information has explanatory import. We don't wish to get into the debates about the notion of causality here, but Kincaid's point seems to be very plausible. Moreover, as many philosophers have argued, there seems to be no compelling reason to suppose that in order to shed light on the causes of individual events, it is necessary to cite any general laws.[47] So (5) could be useful for providing causal explanations even if it doesn't state or imply a law.

Furthermore, even for someone wary of claims about causality, there is still some reason to think that propositions like (5) might have explanatory virtues. For example, (5) seems to provide exactly the kind of information that would be crucial to constructing a statistical explanation for the degree of political activity in a particular agrarian community, on Salmon's S-R model[48] or some similar alternative. Again, this way of understanding the explanatory import of (5) needn't involve any claim about laws, since the S-R model does not specifically require laws in the explanans.

So it seems quite plausible that a claim like (5), which can be tested using Kincaid's methods, can have explanatory import, even if there is no particular nomological claim licensed by (5). But another way of understanding the explanatory import of (5) has it that (5) permits us to infer the existence of a law of nature, and that this law has explanatory value precisely because it can figure in D-N or covering-law explanations. This line of thought goes hand-in-hand with a second way of answering the question of exactly what claim is tested by Kincaid's nine methods. The

second answer, of course, is that what is tested is a putative law. Apparently, the law in question is not a strict law – this would involve the claim that in all other (nomologically possible) populations of the same kind, the same variables would be positively correlated, when the same other variables are controlled for. As is a familiar point by now, the systems studied by the special sciences tend to be too dependent upon contingency and circumstance for there to be such strict laws. So the putative law must be a putative *ceteris paribus* law. The idea here is essentially that if methods like those described by Kincaid can establish a statistical claim like (5), then from this we may infer:

(6) *Ceteris paribus*, P and S covary.

The "*ceteris paribus*" clause here covers significant variations in the variables $V_1 \ldots V_n$, but also other possible interfering factors, not all of which may be explicitly formulated.

On this line of thought, we are forced to face the first horn of the dilemma of Pietroski and Rey (see Section 5). That is to say, if we suppose that this is in fact the way to understand what is tested by Kincaid's methods, and if we suppose that the explanatory power of the results of these methods is to be understood in terms of the ability of a law of nature to figure in covering-law explanations, and if we insist that the legitimacy and explanatory import of the special sciences must be respected, then it follows that we must understand *ceteris paribus* laws as genuine laws. But as we have argued, the prospects here are pretty bleak. It does not follow that Kincaid's methods are not legitimate and scientific, nor does it follow that the results of these methods lack explanatory value. All that follows is that *one* way of understanding these methods and the value of their results leads to a dead end.

We have considered two ways of understanding the role played by Kincaid's methods in the practice of science. On the first way, these methods are useful ways of confirming claims like (5), and (5) has explanatory value, either because it sheds light on particular causal histories, or because it provides statistical data useful for providing statistical explanations, or both. On the second way, Kincaid's methods can be used to confirm (5), but this is not the inferential stopping point: (5) is used as a premise for an ampliative inference to (6), and the conclusion of this inference can be used to give covering-law explanations of, among other things, (5) itself. We object to this second line of thought, not because we have epistemological doubts about the rationality of the supposed ampliative inference, but because we think that the supposed conclusion of this supposed inference is empty.[49] Thus, we favor the first way.[50]

Let us return to our slogan, "If laws are needed, then only laws will do, and '*ceteris paribus* laws' will not". The point of this slogan is not that the special sciences cannot be scientifically legitimate. Rather, the point is that when only "*ceteris paribus* laws" are on offer, then whatever scientific purposes are being fulfilled (and, as Kincaid's discussion of Paige illustrates, important scientific purposes can be fulfilled in such cases) do not require laws. Hence, there is no need to try to rescue the special sciences by finding a way to minimize the differences between "*ceteris paribus* laws" and laws. To do so is to try to stuff all good science into the pigeon hole modeled on fundamental physics, which, we have argued, does articulate strict laws of nature. This can only obscure what is important about the special sciences, as well as what is important about fundamental physics.

We agree with Kincaid that his nine methods are reasonable ones for scientists to employ. We find it plausible that the results of such methods may have explanatory value. So we think that we agree with the main point that Kincaid argues for in the passages we have considered – namely, that social science can be legitimate science. What we object to is one gloss that Kincaid sometimes puts on his position: that the social sciences articulate *ceteris paribus* laws, and that such laws play a legitimate role in scientific practice.[51] (We should note that this gloss doesn't seem to be essential to the main thrust of Kincaid's argument, and at some places he distances himself from it.[52]) The gloss strikes us as unfortunate, because it gives aid and comfort to philosophical projects for explicating "*ceteris paribus* laws" in a way that minimizes their glaring difference from strict laws of nature. As we have explained, we find such projects hopeless, misguided, and irrelevant to understanding what really goes on in science.

13. CONCLUSION

We have argued that it isn't *ceteris paribus* all the way down – *ceteris paribus* stops at the level of fundamental physics. Furthermore, given Hempel's insight, if all regularities in the world supervene on the regularities that can be studied by physics, then there can be no strict laws of a distinctively biological, psychological, or economic kind; that is, there can be no strict laws formulated purely within the vocabulary of the special science in question. The supervenience claim may be challenged, but most writers on the topic seem to be in agreement with the conclusion that there are no strict laws of the special sciences. It follows that if there are laws of the special sciences – and many commentators assume that there must be – then there must be *ceteris paribus* laws. But there is no persuasive analysis of the truth conditions of such laws; nor is there any persuasive

account of how they are saved from vacuity; and, most distressing of all, there is no persuasive account of how they meld with standard scientific methodology, how, for example, they can be confirmed or disconfirmed. In sum, a royal mess.

Our rhetorical strategy for finding a way out of this mess was to start out to answer the question "What is a *ceteris paribus* law?", to provide an answer by giving truth conditions, then to see what is wrong with the answer, and, finally, to draw the moral that the question is not a good one. There is a well-defined sense in which a generalization can fail to be strictly true and yet be a "near-law"; we have shown how the "laws" of classical phenomenological thermodynamics answer to such a sense. Where a micro-reduction is possible, it is hopeful that such a clear sense of "near law" can be satisfied. But what is crucially important is that there be precise senses that can be given to "approximately true" and "most of the intended applications". In most cases where *ceteris paribus* clauses are actually used, this is not the case, and in our example of a situation where this is the case, *ceteris paribus* clauses are not used. Our positive proposal is that when the requisite precise senses cannot be defined, "*ceteris paribus* laws" are the vague claims that they appear to be, and that their widespread use can be explained by the fact that they are elements of "work-in-progress theories".When they are put forward by a science, this is an indication that science is still in the process of elaborating a theory that makes definite claims about the world; philosophers should let the scientists get on with their work and try to understand this process, rather than attempting to analyze "*ceteris paribus* laws" in a way that hides their shortcomings and obscures the road that lies ahead for science.

As we hope we have made clear, we think there is much more to the special sciences than just articulating such "work-in-progress" theories. Nonetheless, articulating such embryonic theories might be a real feature of scientific practice, in both the special sciences and physics, and we suspect that it is. So "*ceteris paribus* laws" might have a place in an adequate understanding of science, although we think it must be quite different from that typically ascribed to them. For they might be elements of embryonic theories. As such, they are not yet ready to be confirmed or disconfirmed, and it is not clear that they can have real explanatory import in their current stage of development. Thus, they do not stand in need of the same kind of explication as do the propositions of fully-formed theories, such as laws, conditional probability statements, and the like. And it would be a mistake to try to analyze them in such a way as to obscure or minimize the ways in which they differ from the latter.

Much work on the topic of provisos and *ceteris paribus* laws has been motivated by a concern to defend the special sciences. The concern often derives from the following line of reasoning: "These sciences do not state strict laws, so they must state *ceteris paribus* laws; the scientific status of these sciences is not to be impugned, so we must find a way of showing that *ceteris paribus* laws are not really that different from the laws of fundamental physics". We remain "physics chauvinists" in the limited sense that we *do* think there is a crucial difference here. It is not "*ceteris paribus* all the way down" – *ceteris paribus* stops at the level of fundamental physics. But we are *not* physics chauvinists in a more important sense, for we deny that the mark of a good science is its similarity to fundamental physics. The concept of a law of nature seems to us to be an important one for understanding what physics is up to, but it is a misguided egalitarianism that insists that what goes for physics goes for all the sciences. The special sciences need not be in the business of stating laws of nature at all, and this blocks the inference from the legitimacy of these sciences to the legitimacy of *ceteris paribus* laws. For us, it is ironic that an effort to justify the special sciences takes the form of trying to force them into a straitjacket modeled on physics. We think this effort should be resisted, since it damages both our understanding of the special sciences and our understanding of the concept of a law of nature.

NOTES

[1] This claim is made by Cartwright (1983, 1989, 1995, 1997), Fodor (1991), Giere (1988), Hausman (1992), Kincaid (1990, 1996), and many of the other works cited below – but *not*, we will argue (against prevailing opinion), by Hempel (1988).

[2] The list of references given below is by no means complete, but it does provide the reader with representative sample of the recent work on this problem. For a history of the origins and use of "*ceteris paribus*", see Persky (1990).

[3] Lange (1993a, 233). Later we will question the efficacy of this example.

[4] Here Kincaid is using '*ceteris paribus* clause' to mean the same thing that Lange means by 'proviso'. The two are often used interchangeably in the literature, and generally we will follow this practice. However, as will be explained below, Hempel uses 'proviso' in a different sense.

[5] Lakatos (1970) also held that theories of physics contain *ceteris paribus* clauses. But his sense of *ceteris paribus* seems closer to Hempel's sense of proviso discussed below in Section 3 than to the standard sense of *ceteris paribus*.

[6] This holism results from the notion that the HD method is all there is to inductive reasoning, a very dubious notion indeed. However, we do not wish to inveigh against holism here.

[7] See especially chapter 4 ("The Language of Theories") of Sellars (1963, 1991).

[8] Thus, Lange (1993a, 240): "the number of provisos is 'indefinitely large', which makes it impossible to offer them all as premises".

[9] Lange's own solution to the problem of provisos shares this feature of Hempel's account. As will become clear below, Lange holds that the content of a scientific theory typically cannot be explicated in terms intelligible prior to the introduction of the theory. So it isn't that Lange completely misses Hempel's point; it's just that he takes the main upshot of Hempel's argument to be something other than what we take it to be, and what Hempel says it is.

[10] In particular, see Hempel (1988, 26): "Note that a proviso as here understood is not a clause that can be attached to a theory as a whole and vouchsafe its deductive potency . . . Rather, a proviso has to be conceived as a clause that pertains to some particular application of a given theory . . . "

[11] To be fair to Lange, it should be noted that he acknowledges that Hempel does not explicitly present the dilemma discussed above. But he claims that "this dilemma certainly stands behind [Hempel's] discussion" (1993a, 238). He argues that "[i]t must be because he believes that a 'law-statement' without provisos would be false, that Hempel defines provisos as 'essential"' (ibid). But as we have pointed out, Hempel does not think that without a proviso, a law would be false. In the Newtonian celestial mechanics example, Hempel takes the laws to be true as they stand. He argues that the proviso that there are no non-negligible forces other than mutual gravitational attraction is essential because it is necessary for *this particular application of the laws*, not because it is essential to the truth of the laws. It might be replied that Hempel must take the laws in this example to include such clauses as "so long as no unaccounted-for forces are acting", but the work done by this clause is already done by the reference, in Newton's second law of motion, to the *total* impressed force (see below).

[12] This is not to say that the semantic view of theories does not have advantages over the statement view of theories, but our opinion (for which we will not argue here) is that the virtues of the semantic view have been greatly exaggerated.

[13] By "phenomenological physics" we mean those branches of physics that aim to state correlations among more or less observable macroscopic phenomena; hence, Lange's example of the law of thermal expansion belongs to phenomenological physics. By the "special sciences" we mean to include all sciences other than physics; but we have in mind particularly biology, psychology, and economics.

[14] Towards this end we have to take on what would be Cartwright's (1983) objection to our analysis of Hempel's example, and in particular, to our claim that Newton's law of gravitation stands or falls without proviso or *ceteris paribus* qualification. There are two ways to construe this law. One is to take it as asserting that "If there are no non-gravitational forces acting, then any two massive bodies exert a force on one another directly proportional to the product of their masses and inversely proportional to the square of the distance between them". The other is to take it as asserting that "(Regardless of what other forces may be acting) any two massive bodies exert a gravitational force on one another that is directly proportional to the product of their masses and inversely proportional to the square of the distance between them". Cartwright grants that the first reading states (what was taken to be) a true law; but she notes, quite correctly, that this law is irrelevant to real world situations where typically other forces are present. The second reading (which we favor) produces a more useful law statement but one that according to Cartwright lacks facticity because component forces are unreal. We first observe that even if correct, Cartwright's view is not very damaging to our thesis because there are plenty of other examples that

are not subject to her peculiar form of anti-realism about component forces (e.g. Einstein's general theory of relativity which treats gravity in terms of spacetime curvature rather than in terms of force). Furthermore, we do not understand how anything short of a blanket anti-realism can motivate the notion that the gravitational force component of a total impressed force is unreal. To be sure, it is not implausible to say that an arbitrary decomposition of the total resultant force may yield components to which we may not want to assign any direct ontological significance. But modern physical theory from Newton onward gives two reasons to take certain component forces as having real ontological significance: first, the theory gives an account of how the component force arises from the distribution of sources (masses for the gravitational force, charges for the electrical force, etc.); and it promotes a form of explanation in which the total resultant force is obtained as a vector sum of the component forces that are due to sources.

[15] But since these generalizations are, by hypothesis, true, the problems of *ceteris paribus* and provisos would not arise. As a matter of fact, however, there seem to be very few strictly true generalizations which can be stated purely in the vocabulary of a special science and which are lawlike and play in that special science the sorts of roles that would make it plausible to dub them laws of the this special science.

[16] Admittedly there is much more to Beatty's thesis, which we cannot do justice to here. In particular, he shows that there are various senses in which the 'laws of biology' are deeply contingent.

[17] Though not unprecedented – see Musgrave (1980) and the references therein.

[18] This isn't Lange's entire case – he provides other arguments in his (1993b) and (1995). Here we cannot do justice to all of Lange's views and arguments concerning laws. Our concern here is not to criticize his positive account of laws, but only to object to his treatment of the problem of provisos in his (1993a).

[19] See Musgrave (1980) for arguments against the view of laws as rules.

[20] Here and subsequently, "cp: P" will be used to express the proposition that P holds under a *ceteris paribus* clause.

[21] Actually, Pietroski and Rey say that they are giving a condition for the *nonvacuity* of a *ceteris paribus* law. But it is clear that their sufficient condition for nonvacuity will not be satisfied by putative *ceteris paribus* laws that are nonvacuous but *false*. Hence, we construe their proposal as a proposed sufficient condition for such a law to be both true and nonvacuous.

[22] Pietroski and Rey are non-committal about this third clause; its purpose is to rule out laws that are empty in the sense of having neither any instances nor any exceptions. They allow for the possibility that this clause should be dropped. We have corrected what we take to be a typo in clause (ii). In the published version, this clause ends with the phrase "explains $[-Gz]$". But this "z" is a variable unbound by any quantifier. So we read "y" for "z".

[23] Cartwright draws a distinction between capacities and tendencies, but this distinction will not matter for our discussion; see her (1989, 226).

[24] Cartwright (1995, 293): "Economics and physics equally employ *ceteris paribus* laws, and that is a matter of the systems they study, not a deficiency in what they say about them".

[25] See Cartwright (1989) pp. 158–70.

[26] Cartwright (1989, 162–3). For Sellars's argument, see his (1963, 95–97 and 118–23).

[27] Sellars uses his argument to establish that theoretical laws typically do not have any contingent consequences that can be stated purely within the "observation framework". It does

not follow from this that there are no strict lawlike regularities governing observable events; indeed, Sellars is concerned to argue that one important function of theories positing unobservables is that they allow us to formulate strictly true lawlike generalizations where before we could not (see the references in the last note). It seems to us that Cartwright's parallel argument may well establish that attributions of capacities do not imply any strict regularities that can be stated in a vocabulary without the resources to refer to capacities; Cartwright concludes, however, that there are no strict lawlike regularities in nature at all, not even ones that can be only be stated in a richer vocabulary that mentions capacities. This is the conclusion we are taking issue with.

[28] Thus, she writes, in her (1989, 206–207): "The abstract law [i.e., the fundamental law ascribing capacities] is one which subtracts all but the features of interest. To get back to the concrete laws that constitute its phenomenal content, i.e., whatever observable regularities the abstract law gives rise to in concrete situations], the omitted factors must be added in again. But where do these omitted factors come from? ... given a theory, the factors come *from a list*. But the list provided by a given theory, or even by all of our theories put together, will never go far enough. There will always be further factors to consider which are peculiar to the individual case".

[29] The sketch just provided is an informal account of what Cartwright describes as the process of concretization in her (1989, 202–6). The problem of the factors peculiar to the given context is what she calls the "problem of material abstraction" on p. 207; it is introduced by the passage quoted in the preceding note.

[30] Cartwright (1983, 19).

[31] Silverberg's analysis is directed at cp claims of the form cp: $(A \rightarrow B)$ rather than cp: $((x)(Ax \rightarrow Bx))$ although the latter is more relevant to cp laws. Presumably he would say that the latter is true iff $(x)(Ax \rightarrow Bx)$ is true in all possible worlds that are appropriately ideal and are otherwise most similar to the actual world.

[32] This analysis has been influential in the philosophy of economics; see, for example, Rosenberg (1992).

[33] This needs to be qualified: As it stands, Hausman's proposal is trivial, since there always exists a condition C that will do the job, namely G itself. But G would not count as completer in Fodor's sense, because a completer must not by itself be a sufficient condition for the consequent of the law. So by calling C a completer, we are implicitly building in a correction that Hausman's analysis needs anyway.

[34] See Hausman (1991, 139–142).

[35] For criticisms of Lewis' account, see van Fraassen (1989) and Carroll (1994). We are optimistic that these criticisms can be met, but one of us (J.R.) thinks that Lewis' account fails for other reasons.

[36] At any rate, this treatment is more in keeping with Hempel's (1988) discussion (see especially the discussion of Newtonian celestial mechanics on p. 23) than is Lange's discussion.

[37] That this is not an idle point, see Hausman (1991, 135 n.13), who claims the opposite.

[38] For instance, a dynamical theory will be deemed approximately true if the trajectories predicted by the theory track actual trajectories sufficiently closely. We are in complete agreement with Peter Smith (1998) that for dynamical theories, Popper's notion of verisimilitude, which gauges nearness to the truth in terms of the amount of exact truths that are captured, is badly off the mark. A dynamical theory can be approximately true in the above sense even if *all* of its assertions about trajectories are strictly false, and it can

be nearer the truth than some other theory which makes many more strictly true statements about trajectories but which fails to track the actual trajectories as closely.

[39] For example, suppose for sake of argument that "*ceteris paribus*, if demand increases, then the price increases" is a cp law of economics. Now suppose that the boundaries of the sciences shift so that economics makes use of the vocabulary and experimental techniques of micro-physics. It seems plausible that this "law" will no longer be a law of economics since economics will now be able to study many kinds of micro-physical states that might well defeat the "law".

[40] There are unresolved technical and conceptual problems in the reduction of thermodynamics to statistical mechanics (see Sklar (1993)), but these problems do not affect the present discussion.

[41] See Earman and Norton (1998) for an account of this matter.

[42] Carrier (1998) calls the second law of thermodynamics a *ceteris paribus* law because it is "afflicted with exceptions" (p. 221). But when this generalization was formulated in the 19th century, it was believed to hold without exception. And when exceptions were discovered in the 20th century, scientists typically reacted not by redubbing it a *ceteris paribus* law but by adding (implicit) scare quotes to the honorific "law" and noting that it is only an approximately true generalization whose limited reliability is to be explained by statistical mechanics.

[43] A helpful analogy is provided by Faraday's striking and imaginative statements about lines of force. Such statements pointed the way to the laws of electrodynamics, but they were not such laws themselves, and it may be appropriate to think of them as part of a "work-in-progress" theory. "What are the truth conditions of these statements? What is their precise content? How can we make sense of the way they are confirmed, and the role they play in making predictions and giving explanations?" What would be the point of asking questions like these? But surely there are more interesting questions in the neighborhood, e.g., "What role did Faraday's claims about lines of force play in the development of Maxwell's theory of electromagnetic fields?"

[44] Another possible case, of course, is one in which some legitimate scientific purpose requires neither laws nor '"*ceteris paribus* laws". See Section 12.

[45] Kincaid (1996), p. 67.

[46] Kincaid (1996), pp. 70–80.

[47] Anscombe (1971); Cartwright (1983).

[48] Salmon (1970).

[49] What we think is empty is not the notion of a law as such, but the notion of a *ceteris paribus* law.

[50] Of course, someone could say that the conclusion of the kind of investigation Kincaid describes is a statement of the form (6), but that such statements are just convenient shorthand for statements of the form (5). If this is done, then nothing is amiss, but we would say that "*ceteris paribus*" is being used in the *lazy* sense described in Section 11, rather than in the philosophically interesting sense that has been the focus of most of the literature on this topic.

[51] Kincaid (1996), p. 63ff.

[52] For example, on p. 97 of his (1996), Kincaid writes: "[I]n the end the real explanatory work results from picking out the particular causes at work. Generalizations help in that process and result from it, but they are really derivative of the specific causal facts. Thus though I used causal laws as a wedge into this chapter, the key factor in explanation, I would suggest, is not the laws but the causes. . . . Laws . . . are likely to pick out only very

partial causes. They will be confirmed and will explain only to the extent that we are sure they apply – and that is done best by filling in their *ceteris paribus* clauses, frequently on a case-by-case basis". Thus, Kincaid shares our skepticism that the explanatory fruits of his nine methods are covering-law explanations. He also seems to share our skepticism that "*ceteris paribus* laws" (which are clearly the only kind of laws he is concerned with here) have explanatory import and are capable of being confirmed as long as their *ceteris paribus* clauses are left vague.

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