## Ceteris Paribus-Laws: A Naturalistic Account

forthcoming in *International Studies in the Philosophy of Science*

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### Abstract

An otherwise law-like generalisation hedged by a *ceteris paribus* (CP) clause qualifies as a law of nature, if the CP clause can be substituted with a set of conditions derived from the multivariate regression model used to interpret the empirical data in support of the generalisation. Three studies in human biology that use regression analysis are surveyed, showing that standard objections to cashing out CP clauses in this way—based on alleged vagueness, vacuity, or lack of testability—do not apply. CP laws also cannot be said to be simply false due to the indefinitely many conditions not explicitly stated in their associated model: scientific CP clauses imply that these are, given the evidence, not nomically relevant.

### 1. Introduction

Empirical universal propositions explicitly or implicitly hedged by a *ceteris paribus* (CP) clause, or similar qualifications such as 'in the absence of disturbing forces', are a staple of many sciences. Yet, hedges of this type are notorious among philosophers of science for allegedly rendering vague, vacuous, untestable, spurious, or just plain false, every statement they are attached to. There is widespread scepticism in the profession about the suitability of generalisations prefixed by a CP clause for the role traditionally attributed to statements expressing laws of nature. Some hold that since the statements of the social sciences, in particular, are riddled with CP qualifications, these disciplines cannot pretend to discover genuine laws (e.g. Earman, Roberts and Smith 2002); others take the view that the clause must be attached to all of our scientific generalisations and that therefore no science—physical or otherwise—should be expected to produce 'laws' in the traditional sense of exceptionless empirical regularities, in the first place (e.g. Giere 1999; Cartwright 2002).

I shall defend the view here that CP clauses are not the methodological, epistemological, and metaphysical quagmire that philosophers have made them out to be. Taking a claim by three studies surveyed in the 2004 U.S. Surgeon General's Report,

> 'in utero exposure to maternal smoking adversely affects pulmonary function in infants',

as a plausible candidate for an empirically supported CP law, I argue that in some scientific contexts at least, CP clauses can be interpreted as straightforwardly referring to the known interfering factors controlled for in the multivariate regression model used to interpret the evidence in support of the generalisation they hedge (Sec. 2). On this broadly naturalistic approach, a hedging clause is an abbreviation for a specification provided by an associated data model of the range of circumstances under which the relevant law holds (the 'completer' of the law). Standard objections in the literature to completer-accounts of CP laws are that such a completer must, of necessity, remain vague or indeterminate; that this vagueness or indeterminacy makes CP laws vacuous; and that they are untestable, and hence unscientific. I show that none of these charges are true of the CP generalisations derived from the regression models used in the medical studies surveyed in the Surgeon General's report (Sec. 3).

Further (Sec. 4), I address the widespread view that the in-principle-impossibility of eliminating a law's CP clause by fully specifying the conditions under which the law holds means that all CP laws are strictly speaking false. I note, firstly, that there are plausible examples in the physical as well as non-physical sciences of cases where we appear to be able to do just that. Secondly, and more importantly, scientific practice does not justify the requirement that we should (always) be able to do so: scientists do not think of a model that selectively includes but a small number of causal factors as a necessarily *false* representation of its target system; nor do they consider a model to be descriptively or otherwise inadequate on the grounds that it "leaves out" indefinitely many potential interfering factors. I argue that this is because to describe a finite set of conditions $\{X_1 \ldots X_n\}$ in a model or the completer of a CP hypothesis associated with the model as relevant to the truth or falsity of the hypothesis, is to *imply* that an indefinitely large set of further conditions $\{X_{n+1}, X_{n+2}, \ldots\}$ is negligible, or nomologically irrelevant to that hypothesis, given the evidence. Properly understood, a (scientific) CP clause functions as a bisection of the set of all conditions according to their relevance, given the evidence, for the truth of the law-like hypothesis to which it is attached; its semantic content is part *explicit* and *finite*, part *implicit* and *indefinite*.

I close (Sec. 5) by considering the objection that according to Earman, Roberts and Smith's classification of hedged generalisations, my theory is not really a theory of CP laws; and with the observation that the present account does not address all relevant problems, in particular, that the question of the relation between higher- and lower-level CP laws remains as moot on this account as it is on others.

## 2. The dangers of smoking when pregnant, *ceteris paribus*

Early 20th-century advertisements for cigarettes and tobacco often peddled imaginary health benefits of smoking, such as relief from asthma. Unreasonable in hindsight, they remind us that there was a time when there was scant scientific evidence for the detrimental effects of tobacco. Around mid-century the first studies began to suggest a correlation between smoking and chronic bronchitis, and cancers of the lung and larynx; today, clear evidence for the harm caused by smoking has extended to almost all organs of the body (USDHHS 2004, 3, 25). In what follows I shall take a look, *pars pro toto,* at a small subset of this wealth of data: studies of the link between maternal smoking during pregnancy and impaired lung function in early infancy.

To identify potential in utero effects of maternal smoking, researchers often conduct pulmonary function tests in newborn infants, in particular, measurements of the time to peak tidal expiratory flow (tPTEF) as a proportion of overall expiratory time (tE): the amount of time it takes until a newborn's respiratory tract reaches its maximal expiratory flow divided by the amount of time needed to complete its expiratory cycle provides an index to detect airflow obstruction, a lower ratio generally indicating reduced pulmonary function. Stick and Burton 1996, Lødrup et al. 1997, and Hoo et al. 1998, carried out neonatal examinations in Australia, Norway, and the UK, respectively; each detected a statistically significant link between maternal smoking during pregnancy and decrements in tPTEF/tE and. Overall, tPTEF/tE was estimated to decline -0.0021 per unit increase in cigarettes per day (USDHHS 2004, 471).

Results of this type have led U.S. health authorities to conclude that '[t]he evidence is sufficient to infer a causal relationship between maternal smoking during pregnancy and a reduction of lung function in infants', and that '[a]lthough the biologic basis for impaired infant lung function from maternal smoking during pregnancy is not yet fully understood, the causal link provides yet another strong rationale for smoking cessation during pregnancy' (USDHHS 2004, 469.). Philosophers will note that a *causal* link is imputed here in the absence of knowledge of a causal mechanism: in support, the 2004 U.S. Surgeon General's Report cites Bradford Hill's criteria for causal inference in epidemiology (Hill 1965), where 'plausibility'—i.e. knowledge of a plausible mechanism linking cause and effect—is only one of a total of nine benchmarks (see USDHHS 2004, 21). In fact, the Report endorses, with a brief tip of the hat to Hume and David Lewis, a counterfactual notion of 'cause' that does not rely on mechanism: 'something is a cause of a given outcome if, when the same person is observed with and without a purported cause and without changing any other characteristic, a different outcome would be observed' (USDHHS 2004, 19).

The Report's authors point out that in practice it is of course not possible to observe the same human being twice under conditions strictly identical except for smoking status. We are thus limited to trying to infer what the (counterfactual) outcome of a change in smoking status would have been by studying the health of two groups of people who are on average very similar, and differ only in terms of their smoking exposure. The expectation is that the observed differential in outcomes can be used to represent what would have occurred in the non-smoking group if it had been subject to smoke, and vice versa (USDHHS 2004, 19). Since to ask a randomly selected group of people to smoke over a prolonged period of time for purposes of comparison with a non-smoking group would be highly unethical—especially when the cohorts are pregnant mothers—a large part of the evidence on smoking and disease inevitably remains non-experimental: studies comparing observed disease risk in cohorts of actual smokers and nonsmokers.

Any two such groups will on average differ in more respects than just one, and the causal effects of these differences can combine with the effect of smoking. Potential confounders must hence be "controlled for", i.e. they must be distinguished from the primary causal factor we are interested in, and their effects separately measured and compensated for. A commonly used mathematical tool for this purpose is multivariate linear regression analysis—a modern extension of C.F. Gauss' classical linear regression model with just one independent variable, $Y_i = \beta_0 + \beta_1 X_i + e_i$ (originally developed for the calculation of planetary orbits), to models with arbitrarily many independent variables, $Y_i = \beta_0 + \beta_1 X_i + \beta_2 X_{i+1} + \ldots \beta_p X_p + e_i$. Although multivariate linear regression uses slightly more complex matrix algebra to generate estimates of parameters $\beta_1 \ldots \beta_p$, it is still based on Gauss' basic idea that the best fit of a linear model to the data is obtained by minimising the squared differences between the observed values of the dependent variable, and the predicted values: the ordinary least squares (OLS) method. (There also are non-linear regression models that use an iterative procedure). Once the parameters of the multivariate regression model have been fully estimated, we are in a position to calculate our conditional expectation of the value of $Y$ given the estimated value of all independent variables, $E(Y|X_i, X_{i+1} \ldots X_p)$, as well as the difference between $E(Y|X_i, X_{i+1} \ldots X_p)$ and $E(Y|X_i + 1, X_{i+1} \ldots X_p)$. In other words, a fully estimated multivariate model allows us to probe the relationship between $Y_i$ and $X_i$ by varying the value of $X_i$ (in the equation) while leaving unchanged (in the equation) the value of all other variables—thereby "statistically holding them constant", even though it may not be the case that $X_{i+1} \ldots X_p$ are constant in the real world (see e.g. Clogg and Haritou 1997).

The rationale of multivariate regression modeling is thus precisely that it provides a means to estimate the covariation of a pair of causal factors measured by independent variables in abstraction of the potential confounding effects of all other factors specified in the model, by 'controlling for' the latter in this manner. The experimental sciences do this by attempting, not always successfully, to hold potential confounding variables fixed physically. (Imagine testing the effect of baking time on a cake by varying baking time while keeping constant the ingredients, as well as temperature, humidity, air pressure, etc.). But for many practitioners in the social and health sciences the expression 'controlling for *X*' has become practically synonymous with 'including *X* in a regression model': the latter lets us do "in the mathematics" what may be impossible or unethical to do in the real world.

Stick and Burton proceeded as follows: they first identified a list of 13 independent variables based on background knowledge of these factors' associations with growth, development, and respiratory illness (Stick and Burton 1996, 1061).[1] Using single variable linear regression, they then determined that eight among these had no significant effects on tPTEF/tE, whereas five variables turned out to be significant individual predictors of a lower tPTEF/tE index: age, respiratory rate, history of asthma, hypertension, and mother's smoking habit. Then, using a multivariate regression model that included these five, Stick and Burton judged age and respiratory rate to be confounders, because lower values of tPTEF/tE were associated with maternal smoking, asthma, and hypertension independently of the effects of respiratory rate

[1] These were: infant age, sex, infant weight, infant length, respiratory rate, mode of delivery, ethnic background, socio-economic group, maternal smoking history, smoking habits of household members other than the mother, family history of asthma (in a first-degree relative), hypertension before or during pregnancy, mother participated in the intensive or the usual care arm of the relevant cohort study (Stick and Burton 1996, 1061).

and age (Stick and Burton 1996, 1061). They ultimately identified a significant dose-response effect of maternal smoking on tPTEF/tE, as infants of mothers who smoked more than ten cigarettes daily had the lowest mean tPTEF/tE, and infants of non-smoking mothers the highest mean tPTEF/tE (Stick and Burton 1996, 1062).

Lødrup et al. similarly found a change in tPTEF/tE of -0.0021 per daily cigarette (Lødrup et al. 1997, 1776). Their regression model controlled for age, sex, birth weight, length, maternal education, family income, parental allergies, maternal active or passive smoking. Hoo et al. 1998, finally, followed a procedure similar to Stick and Burton, first identifying a set of covariates known to be associated with either maternal smoking or tPTEF/tE, and then homing in on sex, ethnic group, social class, and maternal smoking for their regression analysis. They determined that the tPTEF/tE index in prematurely born infants exposed to tobacco in utero was significantly lower than that of those who were not. (See Figure 1 for a graph summarising the results of the three studies). Despite differences with respect to absolute values as well as variation in terms of how confounding factors were categorised and measured (e.g. 'social class' vs. 'family income' or 'maternal education'), the studies agree quite remarkably in their trend lines. There is undoubtedly a dose-response effect—Bradford-Hill's causal criterion nº 5—of maternal smoking on infant pulmonary function, with tPTEF/tE decreasing as smoking increases (Figure 1). Each study duly concludes that smoking during pregnancy 'adversely affects' the respiratory health of the child.

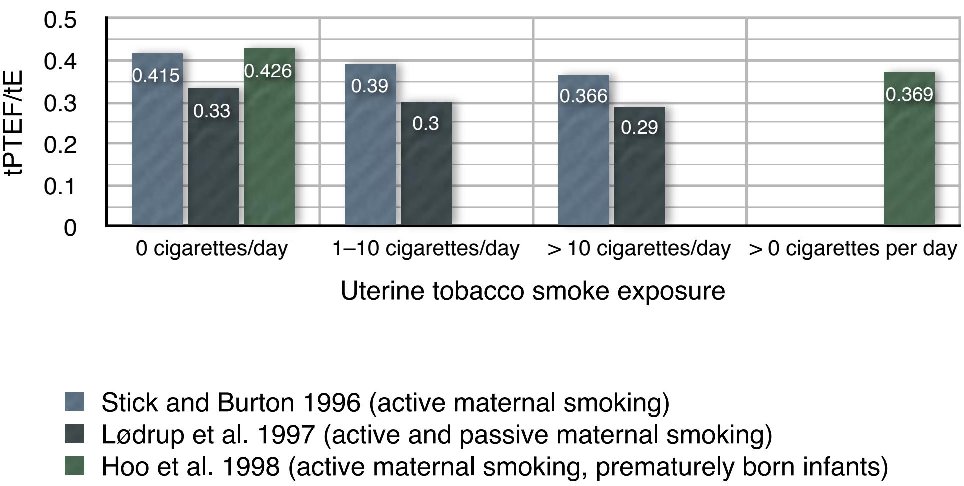


Figure 1. Reduction of time to peak tidal expiratory flow as a proportion of total expiratory time (tPTEF/tE) according to in utero exposure to cigarette smoking

Notice that the models that were used to interpret the data, *qua* multivariate regression models, all express and test the same underlying thought: that there might exist a link, possibly *causal*, between maternal smoking and a specific health outcome, which however cannot be isolated and quantified without previously screening off the potential impact of other factors. In other words, the epistemic rationale for data modeling in this context is that the causal effect, if any, of smoking on infant pulmonary function is likely to be swamped by the potentially much larger effects of age, size, sex (in one study), asthma, allergies, hypertension, and so on. To detect causality we must control for these variables to determine the truth of the Surgeon General's causal counterfactual, i.e. whether 'when the same person is observed with and without [maternal smoking] and without changing any other characteristic, a different [pulmonary function] would be observed'.

Typical uses of the CP concept are made with an identical epistemic rationale. *Ceteris paribus* literally means 'other things being equal', and just like multivariate regression the concept is deployed in contexts where the relevant 'other things' are expected to interfere with or swamp the effect we are interested in (as e.g. in '[*ceteris paribus*] changes in GDP growth depend linearly on changes in the unemployment rate', or '[in the absence of interfering factors] allele frequencies in randomly mating populations remain constant', etc.). In fact, in economics textbooks the parameter estimates of multivariate regression models are often referred to as having a 'partial effect, or *ceteris paribus*, interpretation'—in other words, as allowing us to draw '*ceteris paribus* conclusions' about the degree to which the value of any individual $X_i$, $X_{i+1}$, $X_{i+2}$ etc., independently affects the value of $Y_i$, despite the fact that in reality these factors might be correlated not just with $Y_i$, but also with each other (see e.g. Wooldridge 2009, 73, 80).[2] This suggests that an appropriate way to characterise the evidential import of the regression models used to interpret the results of the pulmonary function tests in the above studies is that they support an inference to a corresponding CP generalisation about maternal smoking and its effects on respiratory health in infants:

i) *ceteris paribus,* the mean tPTEF/tE ratio of newborn infants whose mothers actively smoked >10 cigarettes per day when pregnant, decreases by

[2] No multicollinearity is a key modeling assumption in multivariate linear regression (other important assumptions are uncorrelated errors, homoskedasticity, and no model specification error). In practice, two ore more independent variables are often correlated with each other, which can threaten to render the model's coefficient estimates unstable. But this is not always the case, and tests for robustness are available and are frequently used.

0.049 (Stick and Burton 1996)

ii) *ceteris paribus,* the mean tPTEF/tE ratio of newborn infants whose mothers actively or passively smoked >10 cigarettes per day when pregnant, decreases by 0.04 (Lødrup et al. 1997)

iii) *ceteris paribus,* the mean tPTEF/tE ratio of prematurely born infants whose mothers smoked >0 cigarettes per day when pregnant, decreases by 0.057 (Hoo et al. 1998)

These generalisations can be interpreted in a causal way, since they support conditional predictions of the form 'if the proviso holds, and the smoking variable is intervened on (i.e. if the mother increases or decreases her daily exposure to smoke), then we will see a corresponding change in the value of the tPTEF/tE variable' (cf. Woodward 1997, 2003). Indeed, the three studies adopt explicitly *causal* language in their conclusions, despite the fact that their evidence is purely statistical. Regression models, like all structural equation models, "say" that a unit increase in $X$ would result in $\beta$ unit increases of $Y$, and it is traditionally accepted that given the standard assumptions (see footnote nº2), they "explain" the variation in the dependent variable by the variation in the independent variable (see e.g. Haavelmo 1944). If the association further satisfies Hill's criteria of causation, then that explanation is generally accepted to be *causal*. The objective of this article is not to contribute to the causation vs. covariation debate, however. It is, rather, to argue that: in actual scientific practice as well as in public health policy, important inferences from covariation to causation are frequently made on the basis of multiple regression analysis; that inferences such as (i)-(iii), despite having being made from statistical data, do *not* assert a merely probabilistic relationship, but can be interpreted as causal; and that a theory of CP laws based on multiple regression methodology is available, and can solve common philosophical problems associated with CP laws.

Thus, the use made by Stick and Burton 1996, Lødrup et al. 1997, and Hoo et al. 1998 of multivariate regression analysis suggests that the meaning of the CP clause hedging *some* types of empirical generalisation in human biology at least—see *infra* for more on this qualification—can be understood in terms of the evidence that supports it. More specifically, since each of the CP clauses in (i)-(iii) can straightforwardly be taken to refer to the known interfering factors that have been controlled for in the multivariate model used to interpret the data in support of the generalisation they hedge, CP clauses of this type are endowed with a determinate and specific content by virtue of their association with a multivariate regression model.[3]

### 3. A naturalistic account of CP laws

The above analysis suggests a general account of one class of putative CP laws. Let 'CP (F → G)' denote a prima facie law-like CP generalisation.[4] If the empirical data supporting this generalisation has been interpreted using a multivariate regression model that identifies and controls for factors with the potential to interfere with the truth of the generalisation, then the model ipso facto gives content to the CP clause hedging it: *cetera* are not *pares* just in case one or several of the confounding variables controlled for by the model do not in fact have the specified value (or range of values), in which case an observation of an F that is not G would not falsify 'CP (F → G)'. In other words, law-like CP generalisations determine their conditions of breakdown with maximum specificity: their CP clauses function as an abbreviation for a precise specification of the range of circumstances under which the generalisation to which they are attached is expected to hold, and, by the same token, of those under which it is expected to be false. Generally, then, an empirical law-like generalisation will qualify as the expression of a genuine law of nature if its CP clause can be substituted with a set of conditions, $C_F$, that "complete" it and associate it with a strict law, as follows:

$$\text{CP (F} \rightarrow \text{G) if (F \& } C_F) \rightarrow \text{G.}$$

In order for the 'completer' $C_F$ to play this role, three conditions must be satisfied:

[3] I thus disagree with Hausman, who claims that, generally, scientific context can at best *vaguely* determine what the "other things" are and what it is for them to be "equal" (Hausman 1989, 309-10); cf. also Hausman 1992)—quite on the contrary, I take Stick and Burton 1996, Lødrup et al. 1997, and Hoo et al. 1998 to have *precisely* determined what qualifies as such (for more on this, see Sections 4 and 5 *infra*.) I also take a different approach than Earman and Roberts 1999 and Schurz 2001b, 2002, who propose that putative CP laws in the special sciences are a species of non-strict *statistical* generalisation (see also *infra*).

[4] I shall not be concerned here with the general definition of 'law-likeness', or the logical form of law-statements. I take the term 'law-like' to apply, roughly, to any true generalisation that is universal, supported by empirical evidence, testable, and explanatory, and that has the form '(F → G)' (where the quantifier is dropped, schematic letters 'F' and 'G' denote whatever the correct relata of laws of nature are—events, states, properties, capacities, etc.—and '→' stands for a connective that expresses the type of nomological necessity, if any, appropriate for law-statements). My focus will be exclusively on the specific contribution of the CP clause to law-like statements (cf. Fodor 1991, 22; Lange 2002, 407).

(a) ‘(F & $C_F$)’ → G’ is true.
(b) neither F nor $C_F$ is nomically sufficient by itself for G.
(c) the content of ‘$C_F$’ is determined by an appropriate multivariate regression model applied to the empirical evidence in support of ‘CP (F → G)’.

(a) follows from the assumption that CP (F → G) is law-like (see footnote nº4). (b) calls for parsimony: if either of the conjuncts composing the antecedent of a given law is sufficient for the consequent, then the other conjunct will be nomologically idle, and should be left out of the statement of the law (Fodor 1991, 23). (c) finally, expresses the thought developed in the previous section: the specification, $C_F$, of the conditions under which a CP law holds can be derived from an associated regression model of the scientific evidence in its support. (c) thus gives expression to the naturalistic premise that, as Earman and Roberts put it, ‘[w]hen various confused and illegitimate senses of “ceteris paribus” are peeled away, the valid core of […] the problem of provisos and *ceteris paribus* clauses is a scientific, not a philosophical problem’ (Earman and Roberts 1999, 440). As we shall see presently, this approach helps to debunk an important set of philosophical objections against ‘completer-accounts’[5] of CP laws: CP laws are neither vague nor vacuous, untestable or unscientific.

Philosophers sceptical about theories that analyse ‘*ceteris paribus* (F → G)’ as ‘(F & $C_F$) → G’ typically focus their objections on the difficulty of fully describing $C_F$—i.e. on the difficulty of identifying all factors with the potential to interfere with the truth of the generalisation, and of explicitly excluding these factors in our formulation of the antecedent of the law. A common observation is that given that in many cases the list of relevant factors is likely to be indefinitely long, no (humanly conceived) model could hope to account for them all. The following conclusions typically are drawn from this: (1) the precise content of $C_F$ and hence the meaning of ‘*ceteris paribus*’ must in many contexts be left partially indeterminate, open-ended, or vague (e.g. Giere 1988; Hausman 1989); (2) this, in turn, threatens to render the distinction between spurious and genuine hypotheses moot, and CP generalisations in general vacuous (e.g. Mott 1992; Woodward 2000, 2003); (3) the vagueness of the hedge makes putative CP laws untestable, and hence unscientific (Earman, Roberts and Smith 2002) .

[5] Completer-accounts of CP laws have in one form or another been proposed by Hausman 1989, 1992; Fodor 1991; Pietroski and Rey 1995; and, perhaps, Hempel 1988.

Let's begin with the charge of vagueness (1). There may indeed be a 'confused and illegitimate sense of "*ceteris paribus*"' out there on which the charge is true: '*ceteris paribus*, it'll fly' (Fodor 1991, 22) or 'if a person wants something, then, all other things being equal, she'll take steps to get it' (Schiffer 1991, 2) come to mind. But these putative folk-psychological uses of the expression—if anyone ever uses it in this way—are not what I have been concerned with here; nor have I been concerned with other non-scientific contexts in which it might indeed be impossible to give precise content to a CP clause. I have argued that this is possible if the clause can be associated with a specific scientific methodology, namely, multivariate regression analysis of scientifically collected empirical data. Section 2 provides prototypical scientific examples which suggest that *some* evidential inferences at least can be interpreted in terms of CP laws as defined above, which are not vague.

(2) Vacuity. To illustrate how the problem arises, philosophers like to use examples of self-evidently spurious CP generalisations such as '*ceteris paribus*, all dogs have six legs' (Hausman 1989, 309), 'CP, all spherical bodies conduct electricity' (Earman and Roberts 1999, 453), or 'CP, all human beings with normal neurophysiological equipment speak English with a southern U. S. accent' (Woodward 2000, 249).[6] The argument is that completer-accounts of CP generalisations are wont to misclassify these as genuine laws of nature, because no account that does not fully specify $C_F$ by explicitly identifying all factors liable to interfere with the stipulated regularity can rule out a potentially infinite number of "non-standard" interpretations of the CP clause on which the generalisation comes out true. There are, after all, innumerable far-fetched but conceivable conditions under which the antecedent of each of the above generalisations would indeed be followed by the respective consequent. Since a precise specification of $C_F$ is presumed impossible for any CP ($F \rightarrow G$), spurious "completers" can be multiplied at will, which renders CP clauses vacuously true and CP laws unsuitable for science.[7]

The vacuity charge evidently does not stick against our present approach. The statement '*ceteris paribus,* maternal smoking of >10 cigarettes per day reduces the mean tPTEF/tE ratio in newborns by 0.049' is hardly spurious or vacuous, if the CP clause is interpreted as referring to the finite set of interfering factors controlled for in

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[6] Further examples of spurious "CP laws" in the literature are: 'CP, if you're thirsty you will eat salt' (Mott 1992, 340); 'CP, all charged objects accelerate at 10 $m/s^2$' (Woodward 2000, 310); 'all compounds containing hydrogen are safe for human consumption' (Earman, Roberts and Smith 2002, 294), and 'CP, nuts consumption is lethal' (Kowalenko 2011, 448).

[7] The counterexamples work as expected against Fodor 1991, Hausman 1992, and Pietroski and Rey 1995; see Mott 1992, Woodward 2000, 250ff, and Schurz 2001a, respectively.

multivariate regression models applied to relevant pulmonary function test data. On the contrary, in this form the generalisation has precise empirical content and as such provides the basis (with similar evidence) for U.S. Department of Health guidance recommending smoking cessation during pregnancy. *None* of the spurious counter-examples in the literature can be associated in the same way with a bona fide scientific model and data.

Take, for instance, Woodward's 'CP, all human beings with normal neurophysiological equipment speak English with a southern U. S. accent'. Only a scientifically absurd model of the relevant evidence could prompt a researcher to accept this as a genuine CP law candidate: assume that 'neurophysiological normality' and 'southern U.S. accent' are measurable quantities; were we to randomly collect worldwide cross-sectional linguistic data measuring (degree of) *southern U. S. accent* (*Y*), together with psychometric data measuring (degree of) *normality of neurophysiological equipment* (*X*), and then regress *Y* on *X*, we would likely find that in this data set change in *X* is not correlated with change in *Y;* being more or less neurologically normal is not correlated with being more or less linguistically southern U.S.

To save Woodward's "law" from falsification by the data, a researcher would need to switch to a multivariate regression model and introduce "confounders" whose putative interference would explain the lack of correlation of *Y* and *X*. (I shall not venture a guess what such confounders might plausibly be in this example). Yet, in normal scientific practice putative confounders are not "controlled for" in this manner without good epistemic justification for our belief in their *existence* and *relevance,* provided by evidence and/or theoretical background knowledge. For example, in the three case studies the confounders that were controlled for had independently been observed to have an effect on pulmonary function; but even when no such evidence is available and researchers act on a hunch, the latter is usually informed by background knowledge regarding plausible causal mechanisms (see also footnote nº15 infra).

In Woodward's "counterexample", this is not the case. We can reject any model of human neurophysiological normality that features the capacity to 'speak with a southern U.S. accent', because the correlation between these variables that obtains in some geographical areas of the U.S. would in most random worldwide samples be swamped by correlations between normality and numerous other accents; and we would have no theoretical reason to chalk up these swamping effects to confounders because currently we have no explanation of how a phenotype such as *accent of type 'Southern U.S'* could (even if 'accents' were linguistically real) confer a selective advantage on an organism, and similarly no evidence for adaptive pressures that could have led to the evolution of a gene that codes for expression of such a phenotype.

(The fact that people pronounce their idiolect differently according to regions looks rather like a case of exaptation). We therefore have no good scientific reason to adopt a model of the extant linguistic data that would licence an inference to 'CP, all human beings with normal neurophysiological equipment speak English with a southern U. S. accent'. The same points apply, *mutatis mutandis,* to all the other putative counter-examples to completer-accounts in the literature.[8]

(3) Testability. If CP laws are not said to be vague or vacuous, then they are claimed to be *untestable*, and hence *unscientific*. This charge, too, appears off target given our present approach: if a law of the form 'CP (F → G)' means '(F & $C_F$ → G)', where '$C_F$' denotes a definite and finite rather than an indefinite set of conditions, then the expression can be tested using standard controlled experiment methodology: take two samples from a population that is similar with respect to the factors that have been controlled for in the underlying model, and expose one of them to F, while conditions $C_F$ are satisfied in both the experimental sample and the control. If there is a statistically significant deviation in the exposed sample from the value of G predicted by the model that cannot be observed in the control, then F would not seem to have the predicted effect and we must check our data and/or improve our model and the associated CP law.

In our non-experimental case, observing, say, a randomly selected group of pregnant women who smoke >10 cigarettes per day while law (ii)'s $C_F$ conditions are satisfied, would lead us to expect to see a decrease—compared to a control group of randomly selected nonsmoking pregnant women—by approximately 0.04 of the mean tPTEF/tE ratio of their newborns, taking into account measurement error. But we would *not* consider (ii) refuted by a different outcome if that outcome were observed while $C_F$ did not obtain, e.g. if there was a difference in birth weight between the exposed group and the control: birth weight, after all, is a real confounder independently known to have an effect on pulmonary function. Multivariate models that include the regressor 'birth weight' "compensate" for the effects of this variable by specifying precisely how much the tPTEF/tE ratio should be expected to vary if birth weight var-

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[8] This does not mean that the naturalistic account of CP laws equates 'CP (F → G) with 'most F are G' or even with 'normally, F → G' (if 'normally' is read in a *statistical* frequency sense). It can easily account for the concept of *biological* normality or typicality, which is independent of statistical frequency: it may be the case, for example, that '*ceteris paribus*, sea turtles have long life spans' or '*ceteris paribus*, lions have manes' are genuine CP laws of biology, despite the fact that the vast majority of sea turtles do not survive the first hour of their lives, and that many lions do not have manes—so long as the multivariate model applied to the observational evidence in support of these generalisations controls for the appropriate interferers, such as e.g. predation, environmental factors, etc., supported by independent evidence or background knowledge (see also Kowalenko 2011: 451-52).

ied by a given amount. Hence, if the content of $C_F$ is given by the factors controlled for in a *bona fide* scientific model used to interpret the evidence in support of F → G, as condition (c) stipulates, then 'CP (F → G)' can be considered testable, and scientifically legitimate.

I take it to be inadvisable for philosophers to attempt to define further strictures on the "scientificity" of a CP law candidate beyond a fairly modest condition of this type. The naturalistic approach I advocate emphasises the need for empirical evidence and sound scientific reasoning to justify the acceptance of a CP generalisation as a law. It shows that contrary to popular philosophical lore, there are contexts in which regression analysis of empirical data can provide sufficient epistemic justification for CP hypotheses that are neither vague nor indeterminate. By the same token, however, it cannot be this account's prerogative to yield, for any given CP generalisation, a 'yes/no' answer to the question whether it is scientifically legitimate: the relevant answer constantly evolves with scientific progress and—especially at the cutting edge of research—is often indeterminate. Moreover, since there are no unanimously agreed necessary and sufficient criteria for the validity of a given data model, or successful formal algorithms for the correct application of background knowledge to model specification, it is always a more or less open scientific question whether the data modelling required by condition (c) satisfies the requisite standards of adequacy or appropriateness in a given case. In other words, it is always a more or less open scientific, not a philosophical, question whether a CP law candidate is *bona fide* "scientific". Naturalistic accounts of CP laws reflect this reality (cf. Kincaid 1996, 70), and suggest that the revisionist motivation of much previous literature in this field is misguided.

## 4. But aren't CP laws *false*?

Marc Lange notes the following difficulty for CP laws, which has since become known as 'Lange's dilemma': many CP claims that purport to state a law of nature seem to either state a relation that does not obtain, and hence are *false*; or they are shorthand for a claim that states no relation at all, and so are *empty* (Lange 1993, 235; he attributes the dilemma to Hempel 1988). Vacuity, I have shown, presents no problem for a naturalistic account. Yet, *both* horns of Lange's dilemma need to be wrong for the present approach to work, given that it conceives of CP laws as strict empirical generalisations, and that according to condition (a), these must be '*true*'. There is a longstanding and influential view in the literature that there simply are no such generalisations.[9] According to the early Cartwright who has done much to popularise the view, for example, there are no exceptionless quantitative laws in physics: even our best-known and most entrenched law-candidates fail to 'cover the phenomena'. When modified with a CP clause, they may be true, but fail to cover anything but a very narrow range of phenomena—cases where "conditions are right", i.e. where the CP clause is satisfied. Yet, since the latter often refers to *ideal* circumstances that do not obtain in reality, conditions almost never are right, Cartwright famously argued, and the laws of physics as normally stated "lie" (Cartwright 1980b, 159-60; and Cartwright 1983). If this is the case for physics, we may fear even worse for the other disciplines.

Moreover, according to Ronald Giere we must give up even the modest hope that occasionally at least, we can state an empirical generalisation such as (i)-(iii) with a CP clause that refers to actual conditions under which the generalisation does "cover the phenomena": 'the number of provisos implicit in *any* law is indefinitely large', he claims, (Giere 1988, 40; my emphasis; see also Morreau 1999; Smith 2002), which implies that it is humanly impossible to ensure that any given law is descriptively adequate across all empirical contexts by stating *all* relevant CP conditions. Giere concludes that '[M]ost purported laws of nature seem clearly to be false (Giere 1999, 90). The popularity of views of this type—together with the demise of 20th-century logical empiricism—has created a fertile ground on which antirealist accounts of science proliferate that seek to eliminate or at least to minimise the role in scientific practice of 'laws of nature' understood classically as strictly true empirical generalisations (e.g. Van Fraassen 1989; Giere 1999; Mumford 1998, 2005).

Any naturalistic account of CP laws according to which 'CP (F → G)' is precisely that, a strictly true empirical generalisation, therefore needs to account for the difficulty of explicitly and exhaustively specifying the conditions of breakdown of (F → G). How could CP laws possibly be considered *true*, if in reality many or most Fs are not Gs, and if we cannot know in detail the precise conditions when we may legitimately infer that a given F will be G? I believe that the case for the falsity of the laws of nature when conceived as strict generalisations, albeit suggestive, has been overstated, for two reasons: (I) it is not as self-evident as the above authors let on that there are no strictly true universal generalisations in the sciences, and that currently accepted laws are not viewed as such by the scientists themselves. And more importantly, (II) the view that a putative CP law's scientific legitimacy *requires* that

[9] See e.g. Scriven 1961; Cartwright 1980a, b; Giere 1988, 1999; and, perhaps, Hempel 1988 (for divergent interpretations of Hempel, see Giere 1988, Earman and Roberts 1999, Eliot 2011); cf. also Lange 1993, 234, and Lange 2000.

we rule out its falsification via exhaustive specification of its completer cannot be justified except via a perfectionist fallacy; scientific practice bears out no such requirement. We can and do very often legitimately infer from 'CP (F → G)' that Fs will be Gs.

Let's start with (I). There may be precise, universally true, laws of nature after all. Earman and Roberts 1999 and Earman, Roberts and Smith 2002, for example, point out that once the appropriate scientific resources are used, many purportedly indefinite and hence 'ineliminable' CP clauses can in fact be eliminated, i.e. a fully explicit completer can be stated. Most prominently, they claim that it is not the case that Newton's law of gravitation is *false*, because it "lies" about the actual behaviour of massive bodies, as Cartwright had suggested. Rather, the law correctly describes a regularity governing the exertion of a component force, gravity, which together with (potentially) other forces such as Coulomb's force, etc., is part of the total force on massive bodies (Earman, Roberts and Smith 2002, 286-87). So if we wish to apply the law of gravitation to a concrete physical system in order to derive, say, the actual motion of a binary star system, we can state the necessary hedge with *full precision*: 'the total force acting on each of the two bodies equals the gravitational force exerted upon it by the other body' (Hempel 1988, 30, cited in Earman and Roberts 1999, 444). This translates the apparently *indefinite*, *ineliminable*, and *unscientific* CP clause 'unless no other factor interferes' into a definite and easily manageable completer.[10] Earman and Roberts suggest that other fundamental laws in physics might in actual fact turn out to be equally free of ineliminable CP clauses (Earman and Roberts 1999, 446).

Strict laws may not be limited to physics, either. Contrary to much philosophical lore (including that of Earman, Roberts, et al.), there appear to be genuinely universal and exceptionless generalisations in biology. For example, the generalisation stating that an organism's metabolic rate is governed by the quarter-power allometric relation and the Boltzmann factor (Elgin 2006, 130, citing Gillooly et al. 2001, 2251); similarly, causal equations describing the dynamics of frequency-dependent selection of diploid populations can correctly predict, subject to a *precise* and *definite* exception clause, actual populations of fish in the African Rift valley (Gildenhuys 2010, 616ff, citing Hori 1993); and subject to an idealisation in the form of precise and testable procedural rules regarding perturbing factors, neuroscientific generalisations about the role of hippocampal place cells in rats' spatial memory, are said to be strict (Datteri

[10] Earman, Roberts and Smith acknowledge that their position on Newton's law requires a realism about component forces that Cartwright does not share. For criticism of Cartwright's anti-realism about component forces see e.g. Creary 1981 and Spurrett 2001; for a defence, see e.g. Lange 2002, 421.

and Laudisa 2012, 604ff). (For the role of idealised law-like generalisations in the study of working memory and mental arithmetic in humans, see Kowalenko 2009). Further strict law-candidates in the non-physical sciences might be in the offing—I suggest we better not treat the question of their existence as an *a priori* one.

(II) One important thought behind the denial of CP laws' truth—the first horn of Lange's dilemma—is that they simply are false representations: there are many circumstances actual as well as possible under which the relation between F and G that a CP law purports to hold, does not; and these are just too numerous to be listed in an explicit, non-vacuous, and exhaustive specification of the law's completer. Any CP generalisation when "cashed out" is therefore limited to selectively singling out some, but in general not all, of the factors that interfere with the relevant Fs being Gs, and hence it is not universally true. A selective representation, after all, is not a truthful representation; a partial picture is not the whole picture. In terms of our CP laws (i)-(iii), this thought amounts to the worry that the multivariate regression models from which they were derived, although they controlled for infant age, sex, weight, ethnic background, hypertension, etc., did not account for *other* possible interferers with the regularity they state; that in many conceivable circumstances there would be residual effects from these factors that the laws do not account for; and that they hence could not be considered strictly and universally true representations of the effect of maternal smoking on infant respiratory health.

Though seductive, this worry about truth does not capture well how scientists appear to think of knowledge derived from models. Daniela Bailer-Jones has conducted a series of interviews of scientists from various disciplines on the role of modelling in their practice. One of the upshots of this work is that theoretical and nuclear physicists, for example, do tend to believe that models in their respective fields are representations, albeit simplified, of 'part of the real world' such that 'elements within it have some *correspondence with the elements of reality*'; similarly, solid state physicists describe models as representations of the system modelled that, despite a messy empirical situation that makes it impossible to 'encapsulate the whole of reality', are designed to 'encapsulate the *essence* [of reality]'; and biogeochemists consider models simplifications of their target system that nevertheless incorporate what they think are the most important elements of that system, such that '*you are describing the system*, but you are not describing it in all of its detail' (Bailer-Jones 2002, 283-85; my emphasis).

The conception of scientific models that seems to be underlying these quotes is one of—granted, simplified and partial, yet—perfectly *true* representation. Idealised models (in the sense of models that selectively represent but one aspect of their target system) are, as Cartwright 2009 puts it, 'isolating tools': they are made with the explicit goal of 'carving nature at the joints', i.e. of separating nature (conceptually) into what we think are its real component parts and processes (see Liu 2004). Newton's law of gravitation, arguably, is based on an idealisation of this type: rather than being a wilfully *false* representation of the universe, the statement of the law is intended as a correct description of its "essence"—or, more precisely, of that of our observations of it, since the relevant 'essence' does not necessarily need to be the universe's ontological underbelly (in Newtons' case, it famously is not). Indeed, many statistical models make few physical assumptions and yet they too succeed, according to astronomers and solar physicists, in 'reproducing something *realistic*' (Bailer-Jones 2002, 287; my emphasis). Statistical understanding is just as valuable as physical insight in these fields, they claim, because the structure and the detail of what we observe (for example, the size distribution in solar flares) is more often related to 'statistical fundamentals [rather] than to physical ones'; "good" models in this context are models that select *a small plausible subset* of these fundamentals, and yield an insight into how that small subset ends up producing the phenomena we observe (Bailer-Jones 2002, 287).[11]

The claim, to summarise these comments, that in order to gain scientific legitimacy, CP laws need to rule out their potential falsification through an explicit listing of *all* possible interferers with the regularity they state, is incongruous with scientific practice. We successfully study planetary motion, metabolic rate, population dynamics, solar flares, and, of course, neonatal respiratory health, using models that may be 'isolating' and/or merely statistical, and that in both cases fail to *completely* describe reality; and we can encode this knowledge in CP laws inferred from such models. To reject a CP law candidate on the basis of the argument that it has been inferred from a model that does not satisfy an *impossible standard*, would be to commit the perfectionist fallacy.

Gildenhuys 2010—who mostly discusses causal equations, but argues that some of them function as strict laws—speculates, plausibly, that the worry about a mismatch between the descriptive content of a causal equation and our actual observations that prompts claims of falsity and inapplicability to the real world, is based on a misunderstanding of what it means when a cause or force is left out of an equation

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[11] The contemporary Cartwright has come around to the view that the laws of nature are strictly *true*, after all (see e.g. Cartwright 2002), because she now believes that they quantify over capacities or dispositions, and their relations (thus 'capturing the essence' of reality). I take the examples in the text to illustrate that one can justifiably believe in true laws that 'capture the essence' of what we observe—in the sense of capturing those elements of the system that produce the phenomena we observe—without such an ontological commitment.

(Gildenhuys 2010, 624). The absence of a given variable representing a force or a cause in an equation may make it seem as if it had been "overlooked"… However, the crucial point is that if the relevant force or cause had taken non-null value, then the evidence that justifies use of the equation likely would not have warranted its disregard (Gildenhuys 2010, 624). Exactly the same is true of the regression models we use to interpret the supporting evidence for CP laws: we include, among others, weight, history of asthma, and hypertension in our model of the data from pulmonary function tests, because the evidence does not warrant their disregard; but we leave out other variables, indeed an *indeterminate* number of them, because it does.

What makes causal equations non-vacuous is thus the fact that their applicability is tied to the actual conditions in which we collected the evidence for them; what renders them non-false is that these conditions can be understood as 'ones in which many causal influences that could defeat the causal equations were actually operative, although they took null values' (Gildenhuys 2010, 630). The same is true of CP laws: to say that 'CP (F → G)' is to say that 'F → G, under conditions $\{X_1 \ldots X_n\}$'; but this is *not* illegitimately to "leave out" an indeterminate further set of conditions $\{X_{n+1}, X_{n+2}, \ldots\}$ that could potentially interfere with Fs being Gs. It is, rather, to *implicitly state* that $\{X_{n+1}, X_{n+2}, \ldots\}$ are nomically irrelevant (= taking null value) to the regularity, *given the evidence*.[12] In other words, explicit mention of $\{X_1 \ldots X_n\}$ in the description of the conditions $C_F$ under which (F → G) holds, *implies* that as far as we currently know, conditions $\{X_{n+1}, X_{n+2}, \ldots\}$ are nomically irrelevant, i.e. have no effect on the truth or falsity of '(F → G)'.[13] The CP clause thus bisects the set of all conditions according to their nomic relevance, given the available evidence, for the truth of '(F → G)'. It makes, ultimately, a fairly weak epistemic claim: out of a myriad of conceivable causal factors, only a *finite* set is currently known to be nomically relevant.

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[12] As the Surgeon General's Report puts it, 'Standard analyses *implicitly assume* an absence of confounding from all unmeasured factors' (USDHHS 2004, 20; my emphasis); i.e. they implicitly assume that indeterminately many other factors take null value.

[13] The manner in which the CP clause '*implies'* this is not unrelated to a mode of semantic implication identified by Wittgenstein in his famous 'colour exclusion problem' (cf. Wittgenstein 1929, 1977). The use of the colour predicate 'red' in the description 'A is red' *says* that A is red, and ipso facto *implies* that A is not one of indefinitely many other possible colours; e.g. one among the semantic implications of 'A is red' is that 'A is *not* blue'. Similarly, the use of variables $\{X_1 \ldots X_n\}$ in the description of $C_F$ in 'F & $C_F$ → G' *says* that $\{X_1 \ldots X_n\}$ are relevant to the truth of putative law 'F → G' ('nomically relevant'), and it ipso facto *implies* that indefinitely many other variables $\{X_{n+1}, X_{n+2}, \ldots\}$ are nomically irrelevant. The difference between this case and Wittgenstein's is that $\{X_1 \ldots X_n\}$ and $\{X_{n+1}, X_{n+2}, \ldots\}$, rather than colour predicates in a 'colour space' linked by a set of internal relations associated with that space, are disjoint sets of conditions in what we might call a 'condition space' linked by the relevant class of CP laws associated with that space.

Hempel wrote that the proviso for predicting the motion of a binary star system using Newton's laws '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.' (Hempel 1988, 158; my emphasis). This is exactly right, on the present account, except that Hempel's list of interfering factors belongs to two different classes, one explicitly *stated* by the proviso, the other *implied*: a fully specified proviso must explicitly state (or, indeed, 'imply' in the sense of logical implication) the known interferers with the relevant law, i.e. it must state that the truth of the law requires that magnetic, frictional forces, and radiation pressure, are absent or negligible; and it must *imply* (in the sense of semantic implication)[14] that telekinetic, angelic, diabolic, plus an indeterminate number of further conceivable influences, are in fact nomically irrelevant, given the evidence. Obviously, if new data were to show that, in the case at hand, 'angelic influences' do occasionally affect the motion of binary stars, then we would need to include these in the set of factors relevant for the truth of the law and state our completer $C_F$ in such a way that it explicitly requires the absence of angelic influences, too; in other words, we would need to shift 'absence of angelic influences' from the implicit to the explicit part of the content of the *ceteris paribus* clause.

This is generally what happens when new observations produce data that, no matter how much we tweak parameter values, fits poorly with our current model: after having checked other possible sources of error (measurement, etc.), we conclude that there has been a model specification error—i.e. that one of our initial regression assumptions does not hold—and adjust the model by adding one or several new variables and/or removing others, ipso facto modifying the completer of the associated CP law. Crucially, the scientific reality that new evidence routinely calls for such a process of adjustment is neither an illustration of the intrinsic vagueness of all scientific models, nor a proof of the indeterminacy of all law-like hedged generalisations associated with such models; *nor* is it a demonstration of their falsity. After having added or removed a regressor, we typically assume that the modified model is now fully specified, and that the amended explicit content of the CP clause renders the corresponding law fully determinate again, as well as true (Hempel calls this the 'assumption of completeness', Hempel 1988, 157); until the next batch of evidence comes

[14] I do not assume a particular semantic theory of implicit meaning, content, or reference here; but I take it as uncontroversial that propositional content is (at least sometimes) underspecified by linguistic meaning, and suggest that sentences containing scientific CP clauses provide a further example of such underspecification.

in.[15]

**5. One objection, and a worry**

Earman, Roberts et al. distinguish three types of '*ceteris paribus*'-clause: a clause that is eliminable, but that we chose not to eliminate; a clause that is ineliminable, because we do not (currently) know how to make it explicit; and a clause that is ineliminable, because 'even with the best of knowledge' we could not make it explicit (Earman, Roberts and Smith 2002, 283). They dismiss the first two as philosophically uninteresting and 'not genuine' CP clauses, because the first is just a function of 'laziness', and the second simply a case of where 'what's needed is further scientific knowledge' (Earman, Roberts and Smith 2002, 283); the third is, according to their philosophical analysis, unsuitable for laws of nature. As an anonymous reviewer pointed out, if one accepts Earman, Roberts and Smith's requirement that genuine CP conditions be 'non-lazy', then I am not really advocating a theory of genuine CP laws at all, here.

The objection assumes that this classification of CP laws is correct. My reason for thinking that it is not is that it fails to save the (scientific) phenomena: in practice, scientists do not parse hedged regularities according to Earman, Roberts and Smith's criteria. In particular, they rarely seem to make a distinction between what is currently unknown, on the one hand, and what is currently unknown and *cannot be known* even with the best of knowledge, on the other—simply because no one has a clear grasp of what 'the best of knowledge' would entail. Historically, scientific progress has in numerous instances turned what we once deemed 'unknowable' (whether for theoretical, metaphysical, a priori, or pragmatic reasons) into the knowable, and scientists these days typically refrain from speculating on what might or might not for all eternity remain unknowable. Earman, Roberts and Smith's second type of CP clause therefore

[15] Most researchers would of course be justifiably suspicious of any putative evidence for 'angelic influence' (Hempel's example) as an interferer with the known forces governing binary star systems, simply because the theoretical disruption its inclusion in the completer of the relevant CP law would cause might not be a price worth paying for saving the law ('angelic force' as a fifth fundamental force of nature?). Lakatos noted that the successful falsification of a CP hypothesis depends on the degree of corroboration of its *ceteris paribus* clause; when that degree is not very high, i.e. when the explicit content of the CP clause is still the subject of active investigation and uncertainty, then scientists must rely on their 'instinct' or a 'hunch' (Lakatos 1970, 114n)—though the latter may be guided by the heuristics of their research programme. This is still true, except that on the present account the instinct must be brought to bear on the selection of the model from which CP laws are derived.

cannot be distinguished from the third. Consequently we ought not consider it any less “interesting”. Neither, incidentally, do scientists: cases where ‘what’s needed is further scientific knowledge’ are those that interest them most.

Furthermore, on the present analysis of the content of scientific CP clauses, there really are no CP laws of the first type, either (“lazy” CP laws). Any accepted scientific CP generalisation is one whose completing conditions we take ourselves to have fully specified, and we use ‘ceteris paribus’ simply as a shorthand for those known conditions. For we believe that our analysis of the evidence in support of the generalisation provides sufficient epistemic grounds for what we take to be a complete specification of the *explicit* part of the content of the relevant CP clause, *not* however grounds for a complete specification of its *implicit* content. Finite human evidence *never* provides sufficient epistemic grounds for a specification of the implicit content of a CP clause, hence any failure to achieve the latter would be a function neither of laziness, nor of ignorance, but simply of human finitude. Insofar as the naturalistic account provides a better fit with scientific practice—(i)-(iii), together with practically all generalisations of the special sciences, would qualify as non-laws on Earman et al.’s classification—the latter appears inadequate.

This leads me to a final worry: if CP laws are associated as closely as suggested here with the conditions in which the evidence for them was collected, then the content of the completers of (i)-(iii) will never be identical, given that the corresponding studies did not use identical data and models with identical variable sets, and that we infer $C_F$ from these sets. Moreover, any time a new study is conducted to confirm the obtained results, it will generate different data again. Technically, this does not lead to any inconsistencies, because the consequents of the inferred CP laws are not equivalent. Yet I introduced (i)-(iii) in the context of the U.S. Surgeon General’s Report, which collates the results of these studies to come to the ‘summary conclusion’ that ‘maternal smoking during pregnancy leads to a reduction of lung function in infants’. (In fact, we already find this very proposition or a similar one in the conclusions of each of our three studies). USDHHS 2004 merges the results of hundreds more studies of this type in other fields to come to the even more general conclusion that ‘smoking leads to adverse health consequences’. This does indeed give rise to a worry about consistency: the set of completing conditions of the corresponding global law would be enormously complex if it were derived simply from the sum of the interfering factors controlled for in *all* studies ever undertaken of the health effects of tobacco, and chances are that in this set we would find both an $X_n$ and a $\neg X_n$.

I do not currently have a satisfactory solution for this problem. It is related to Clark Glymour’s complaint about the proposal by Earman, Roberts and Smith, that the function of prima facie CP claims such as ‘smoking causes cancer’ is to ‘“signal” or “express” the empirical data that prompts their assertion, and also [to] signal a “research program” to “explain” their consequents in terms of their antecedents’ (Glymour 2002, 404, referring to Earman, Roberts and Smith 2002, 296). The problem with this proposal, Glymour points out, is that two oncologists who work with different data sets on smoking and cancer and who pursue different research programs should not be able to *agree* on the global statement that ‘smoking causes cancer’, since they would in fact be signalling and expressing *different* things by that proposition (Glymour 2002, 404). I have not been contending here that ‘CP (F → G)’ merely ‘signals’ (i.e. pragmatically conveys) that empirical data has been gathered about F and G, or that it ‘expresses a commitment’ to explain G by supplementing F with a suitable $C_F$. Yet, the worry is substantially the same: how do we (consistently) ascend from different local CP laws of the (i)-(iii) variety to the global law of the Surgeon’s Report? A first step towards the answer is to note that the completer of a general, high-level, CP law cannot straightforwardly be derived from the set of all interfering factors controlled for in the models associated with the more specific CP laws it is supported by, or derived from; relations between models and between laws are hardly summative, especially not when we move from statistical models to ‘isolating’ ones, or from statistical laws to causal ones. What those relations are is quite a moot point; but perhaps we can take solace in the fact that this vexed problem arises in many other places and contexts as well (for discussion, see e.g. McKim and Turner 1997, Cartwright 2007). I do not consider it a show-stopper, rather an indicator of a direction for further work.

## 6. Conclusion

Much of the present theory of CP laws agrees with the spirit of Lange’s remark that ‘a claim with a proviso attached […] possesses non-trivial content because the proviso is not a totally elastic “escape clause”. To understand the putative law is to know how it is supposed to be applied, and the proviso is true exactly when it is supposed to be proper to apply the putative law’ (Lange 2000, 161); applications, Lange noted, can be considered ‘proper’ as part of a specific model, a calculational procedure, idealisation, simplification, or approximation. Kincaid, similarly, believes that hedged claims can be integrated with standard scientific methodology, and describes nine testing methods that lend credence to CP laws (Kincaid 1996, 67-70). This paper focused on just *one* such model and calculational procedure, multivariate regression analysis—which, arguably, embodies aspects of Kincaid's methods 3 to 6 (Kincaid 1996, 68-69)—in one scientific field, remaining agnostic about the possibility that other types of model or procedure might equally well support legitimate CP law hypotheses in other fields. However, the ubiquity of multivariate regression analysis, the similarity of its methodological assumptions with that of all structural equation models, and the fact that in many sciences the latter are routinely interpreted as causal models, suggest a potentially wider applicability of the account of CP laws I advocate.

Multivariate regression analysis as practiced by Stick and Burton 1996, Lødrup et al. 1997, and Hoo et al. 1998 illustrates, I argued, that the epistemic rationale of multivariate regression is the same as that of a CP clause: to screen off the impact of potential interfering factors on a given regularity. More specifically, multivariate regression analysis of pulmonary function test data provides the epistemic grounds for, and the semantic content of, a set of specific CP laws describing the in utero effects of maternal smoking: CP laws (i)-(iii) can naturally be inferred from the multivariate models used in these studies, if we take the content of the laws' CP clause to be given by the interfering factors that have been controlled for in the corresponding model. I proposed a general account of (at least one class of) CP laws according to which '*ceteris paribus* (F → G)' means '(F & $C_F$) → G', where the completer $C_F$ cashes out the CP clause, if its content is generated by the multivariate regression analysis of the evidence in support of the law; thereby rendering the latter strict.

This approach stacks up well against many of the common objections in the literature against completer-accounts of CP laws: since $C_F$ consists of a determinate and finite list of conditions determined by a scientific process, vagueness can be ruled out; for the same reason, purported counterexamples alleging vacuity fail, as they cannot be associated in the same way as legitimate CP laws with a *bona fide* scientific model and data; testability is not an issue, either, given that a completed CP law is effectively a strict law with a precise exception clause; scientificity is built in by hypothesis. Finally, I argued that the objection from falsity (or the worry that completing CP laws in the way contemplated here is not even possible), though plausibly stated by the early Cartwright and others, does not show that strict laws *per se* are unachievable in science: there are several examples in the current literature of potential physical and non-physical strict laws. More importantly still, the charge of *falsity* seems inconsistent with scientific modelling practice. On the present account, the relationship between a CP law and its associated multivariate regression model is neither descriptive (as Giere 2004 suggests), nor part-whole, but inferential: CP laws are the result of an evidential inference from the results of multivariate regression on empirical data. The truth-status of CP laws, consequently, is mediated by the truth-status of models: CP laws are only as good as their models.

The relationship to the world of predictively and explanatorily successful models that selectively represent a small number of elements of their target system is generally not that of falsity, I argued: scientists do not think of these models as *false* representations—rather, they take them to convey genuine, albeit partial, knowledge of what is 'essential', i.e. knowledge of those elements of the relevant system that are responsible for what we observe of the world. The worry that CP laws cannot possibly be true, because their descriptive content does not precisely match 'reality', is in fact based on a misunderstanding of the significance of factors left out of a completer, or variables left out of an equation; and hence also on a misunderstanding of what it means for a CP law to be 'true'. On the naturalistic approach, the content of any CP law is a function of the conditions in which the finite evidence for it was collected. Our model of the latter and the CP law we infer from it will always appear to potentially "leave something out"; to require otherwise would be to commit a perfectionistic fallacy. To gain scientific legitimacy a CP law need not list an indeterminate number of conceivable interferers with the regularity it states—its hedging clause when spelt out states explicitly the finite number of those that are known to be nomically relevant, given the evidence, and implies that all others are not thus known.

The paper closed with an argument that Earman, Roberts and Smiths's tripartite classification of hedged generalisations is erroneous, and with the observation that the present account does not yet solve all problems; in particular, that of the relation between higher- and lower-level CP laws.

**Acknowledgements**

I would like to thank three anonymous referees for helpful comments on an earlier version of this paper.

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