



## The epistemology of hedged laws

Robert Kowalenko

Department of Philosophy, School of Social Sciences, University of the Witwatersrand, Private Bag 3, WITS 2050, Johannesburg, South Africa

### ARTICLE INFO

#### Article history:

Received 18 October 2010

Available online 6 May 2011

#### Keywords:

Ceteris paribus laws  
Methodology  
Laws of nature  
Modeling  
Multivariate regression  
Mill–Ramsey–Lewis  
Epidemiology  
Nutrition  
Nuts

### ABSTRACT

Standard objections to the notion of a hedged, or *ceteris paribus*, law of nature usually boil down to the claim that such laws would be either (1) irredeemably vague, (2) untestable, (3) vacuous, (4) false, or a combination thereof. Using epidemiological studies in nutrition science as an example, I show that this is not true of the hedged law-like generalizations derived from data models used to interpret large and varied sets of empirical observations. Although it may be 'in principle impossible' to construct models that explicitly identify all potential causal interferers with the relevant generalization, the view that our failure to do so is fatal to the very notion of a *cp*-law is plausible only if one illicitly infers metaphysical impossibility from epistemic impossibility. I close with the suggestion that a model-theoretic approach to *cp*-laws poses a problem for recent attempts to formulate a Mill–Ramsey–Lewis theory of *cp*-laws.

© 2011 Elsevier Ltd. All rights reserved.

When citing this paper, please use the full journal title *Studies in History and Philosophy of Science*

### 1. Introduction

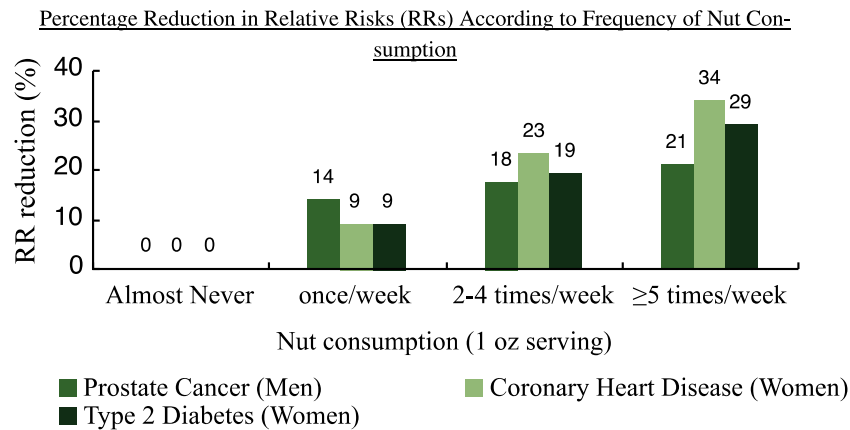
Ever since Cartwright (1983) and others popularized the view that it may be "*ceteris paribus* all the way down", in other words that even the fundamental laws of physics may have exceptions, philosophical discussion of law-like empirical generalizations hedged by *ceteris paribus*-clauses has intensified. On the one side, there are theorists who object to the very notion of a *ceteris paribus*-law of nature by arguing that propositions purporting to express such laws would be either irredeemably vague, untestable, trivial, or simply false—and in any case unscientific. On the other, there are those who believe that these problems can be solved, and that Cartwright was correct to claim that *cp*-laws are necessary in order to make sense of scientific practice.<sup>1</sup> The foil for the arguments of both sides is the demise of 20th-century logical empiricism and the subsequent rise of a number of accounts of scientific knowledge that seek to eliminate, reinterpret, or to minimize the importance of 'laws of nature' conceived in

the empiricist way as universally true, testable, and explanatory empirical generalizations.

This paper will focus on a presupposition typically shared on both sides of this debate, to wit, that '*ceteris paribus*' as a scientific concept is flawed or at least deeply problematic—requiring us to either weed it out from our account of science or to reinterpret it so as to remove its dangerous sting. I will argue that the worry about a possibly fatal indeterminacy of the meaning of '*ceteris paribus*' is misplaced. It is an anodyne fact of scientific life that large and varied amounts of empirical observations require modeling in order to gain an understanding of the data. These are often multivariate regression models that cannot feasibly include *all* possible confounding variables. It is thus indeed impossible in many cases to identify all causal factors with the potential to interfere with an observed law-like regularity, to enumerate all relevant 'other things', or to exhaustively specify the concept of 'normal' conditions. But any inference from this observation to the conclusion that all purported knowledge in the form of hedged law-like

E-mail address: [robert.kowalenko@wits.ac.za](mailto:robert.kowalenko@wits.ac.za)

<sup>1</sup> Among those who hold or have at some point held that hedged generalizations can express genuine laws of nature are Cartwright (1983), Fodor (1991), Hausman (1992), Pietroski & Rey (1995), Lipton (1999), Braddon Mitchell (2001), Lange (2002), Schrenk (2007), and Callender & Cohen (2010). The opposite view has been taken by e.g. Schiffer (1991), Woodward (2000, 2002), Schurz (2001), Earman, Roberts, & Smith (2002), Cartwright (2002), and Mitchell (2002); whose side Hempel (1988) is on is disputed.



**Fig. 1.** Sources: Hu et al. (1998, p. 1344), Jiang et al. (2002, p. 2557) and Mills et al. (1989, p. 601) (cf. also Fraser et al., 1992). The data are very similar also for colorectal cancer in men and women (Jenab et al., 2004), and gallstone disease in men (Tsai, Leitzmann, Hu, Willet, & Giovannucci, 2004).

generalizations derived from models of this type is vague and, worse, *unscientific*, requires additional warrant. It is not made by most practitioners, nor is it borne out in the way in which these generalizations are interpreted and used by scientists, institutions, and laymen.

In fact, the *ceteris paribus*-clause hedging a law-like generalization derived from a given regression model is given fully determinate content by that very model. It is true that such models rarely perfectly fit the existing data, and that new data often requires a modification of the model in order to account for what appears to be either interference or a causal structure different from the one initially hypothesized. But this fact is not an illustration of the intrinsic vagueness of all models, nor of the vagueness of hedged generalizations associated with models. For, adding or a removing a predictor from a given model amounts to a switch from one (precise) model to another and thereby also to a change in the content of the clause hedging the associated generalization(s): the *ceteris paribus*-phrase now implicitly refers to a new set of conditions which include the new variable(s) defined by the new model. There is, I will argue, not a whiff of indeterminacy or of vagueness here; neither is it true that all hedged empirical generalizations are vacuous, or that they cannot be tested. Finally, whether we ought to view them as strictly speaking *false* depends on larger issues in the philosophy of science that an account of *cp*-laws need not settle.

The structure of the paper is as follows: in Section 2, I take a closer look at epidemiological data in nutrition science and its interpretation by scientists and public institutions as supporting a 'qualified health claim' about a link between regular nuts consumption and positive health outcomes. Section 3 shows that the models used to interpret this data directly support appropriately phrased *cp*-generalizations, and that this removes all indeterminacy from the relevant *cp*-clauses. This model-theoretic approach to *cp*-clauses allows us to deal with the 'false-vacuous' dilemma that *cp*-laws are often said to face—namely that the impossibility of fully specifying the provisions of their hedging clause renders them either false or vacuously true. While the second horn is patently incorrect, Section 4 argues that the plausibility of the first horn, the necessary falsity of all *cp*-laws, rests on a doubtful distinction between the concepts of 'metaphysical' and 'epistemic impossibility'. Section 5 closes by suggesting that the advocated

approach rules out recent attempts to use the Mill–Ramsey–Lewis account of laws of nature to solve the conundrum of *cp*-laws.

## 2. Of nuts and (wo)men

For most of the 20th century nutritionists have thought that regular consumption of nuts is largely *unhealthy* due to the high fat content and caloric density of this food group.<sup>2</sup> Then, large-scale prospective cohort studies involving tens of thousands of participants began to uncover a correlation between regular nuts consumption and significant health benefits, such as reduced risk of coronary heart disease, of certain types of cancer, and of diabetes.<sup>3</sup> Today, the consensus is that the anti-oxidants, phyto-nutrients, and a number of other bio-active molecules contained in nuts, as well as mostly mono- and polyunsaturated fats, contribute to positive health outcomes in the human organism. More specifically, the evidence shows that men who eat about 1 oz of nuts 5 times per week or more are 21% less likely to have prostate cancer; women who consume nuts almost daily have a 34% lower risk of coronary heart disease than women who rarely or never eat them; and, women who frequently eat nuts have a 29% lower risk of Type 2 diabetes (see Fig. 1).

Given these correlations, the U.S. Food and Drug Administration in 2003 allowed food product labels to explicitly advertise a connection between nuts and lower risk of heart disease, and the latest U.S. Department for Health and Human Services 'Dietary Guide for Americans' recommends the consumption of 1.5 oz of nuts 4–5 times per week 'as part of a healthy diet'.<sup>4</sup> Of course, nowhere do these institutions make an explicit causal claim linking nuts and health, neither do they come close to holding that it is a *law of nature* that nuts are healthy; nor indeed do they explicitly say that 'nuts are healthy, *ceteris paribus*'. Rather, they authorize food producers to print the following sentence on their packaging:

'Scientific evidence suggests but does not prove that eating 1.5 oz per day of most nuts as part of a diet low in saturated fat and cholesterol may reduce the risk of heart disease.'

This is a 'qualified health claim' in FDA parlance, i.e. a claim that 'characterize[s] the relationship between a substance and its ability to reduce the risk of a disease or health-related condition.' Qualified health claims are described as based on scientific support that 'does

<sup>2</sup> The term 'nuts', incidentally, does not denote a natural kind—it includes tree nuts, such as walnuts, almonds, hazelnuts, etc., as well as peanuts, which botanically are legumes. The classification 'nut' has nevertheless some rationale, for despite their botanical differences the nutritional profile of all members of the class denoted by the term 'nuts' is very similar.

<sup>3</sup> These were, in particular, the Adventist Health Study, the Iowa Women's Health Study, and the Nurses' Health Study begun in the seventies, and more recently the European Prospective Investigation into Cancer and Nutrition (EPIC). See Fraser, Sabaté, Beeson, & Strahan (1992), Kris-Etherton et al. (1999) and Sabaté, Ros, & Salas-Salvadó (2006).

<sup>4</sup> U.S. Department for Health and Human Services (2005, p. 10).

not have to be as strong as that for significant scientific agreement', and must be accompanied by language worded in such a way that 'consumers are not misled about the nature of the supporting science' (ibid.). Note that although the verb 'to reduce' would seem to impute causality, this is carefully hedged using a modal auxiliary, and the emphasis is on the evidence merely 'suggesting' rather than 'proving' the conclusion. This is very cautious indeed, as even a deductive proof that 'X may reduce Y' would not amount to much in the absence of information about the degree of probability implied by the auxiliary. The double hedge here seems to be designed to achieve a specific communicative goal: to rule out even the slightest chance that consumers considering whether to purchase the product are going to think along the lines of a strict universal law. Probably for legal reasons more than anything else, consumers must not believe that eating the product is *guaranteed* to make them healthy.

On the other hand, despite their carefully avoiding the form of a (strict) causal law, qualified health claims are hardly of the form of a (strict) statistical law, either, such as e.g. 'any  $U_{238}$  atom has probability 0.5 of decay within 4.468 billion years.' The latter describes a probability distribution that provides no explanation (beyond the truth of the law itself) for why any individual particle happened to decay at that particular moment rather than any other. A healthy life free of heart attacks, cancer, etc., is very unlike radioactive decay insofar as health outcomes in humans are non-random events subject to the influence of a known series of causal factors—nuts consumption being but one of them, others including age, body mass index, diet, hypertension, and various other life style factors such as exercise, alcohol consumption, smoking and many more. It is possible to determine the probability of an individual who regularly consumes nuts having a heart attack, but in order to do so we require detailed knowledge of the presence or absence of these further factors.

This is why epidemiological evidence is typically framed in terms of 'relative risks' (see Fig. 1), defined as the ratio of the probability of a certain outcome among the segment of a population that has been exposed to a given causal factor whose efficacy we want to measure (the exposed group), to the probability of the outcome among the segment that has not been so exposed (the control group)—where both the exposed and the control group have been checked for further causal influences susceptible to significantly impact the monitored outcome. The conclusions of the studies cited above are that if we control for these confounding variables, that is hold their values steady in both groups, then increasing the value of 'nuts intake' in the exposed group tends to result in fewer incidences of negative health outcomes in that group than in the control. Of course, speaking of 'controlling for' a variable in the context of observational data is somewhat metaphorical. For obvious practical and ethical reasons most nutritional studies cannot be experimental, in other words, they do not involve randomly selecting two groups and serving them different diets over the course of several years. We cannot perform the experiment necessary to directly measure the causal contribution of nuts to health by physically manipulating the latter variable while randomizing or physically holding steady all other variables and measuring outcomes; but we can replace experimental data with observational data by applying statistical techniques to the latter that allow us to simulate the kind of control over causal variables that we have in experimental setups (so-called 'quasi-experimentation').

The tool of choice to achieve this these days is multivariate linear regression, a development of the classical method of least squares pioneered by C.F. Gauss. Gauss's original regression model with only one independent variable,  $Y_i = \beta_0 + \beta_1 X_i + e_i$ , proved insufficient in contexts, such as economics or epidemiology, in which we observe multiple causes that combine to produce multiple effects that cannot be fully specified or distinguished from one an-

other. To find the so-called 'net effects' of an individual causal factor in a system of multiple interrelated causes, we must gain an understanding of how the value of the dependent variable changes when any one of a system of indefinitely many independent variables (or 'predictors') changes while the others are held fixed. To this end, we require a model with additional predictors  $Y_i = \beta_0 + \beta_1 X_i + \beta_2 X_{i+1} + \dots + \beta_p X_p + e_i$ , as well as an estimate of the values of the parameters  $\beta_2 \dots \beta_p$ . Multivariate linear regression—which involves slightly more complex matrix algebra, but ultimately still boils down to the idea of finding the smallest sum of the squared differences between the observed values of the dependent variable and those predicted by the model—provides such an estimate. With the latter in hand we can calculate the conditional expectation (E) of the value of Y given the estimated value of the additional independent variables,  $E(Y|X, X_{i+1} \dots X_p)$ ; moreover, we can also calculate the *difference* between  $E(Y|X, X_{i+1} \dots X_p)$  and  $E(Y|X+1, X_{i+1} \dots X_p)$ . In other words, using our data set and the multivariate regression model with fully estimated parameter values, we can calculate the difference between our expectation for the value of Y when X varies in one case and when it does not in the other, but all other predictors are "held constant." This is the meaning of the phrase 'controlling for factor X' in observational data: X is not being manipulated and physically held constant in a laboratory-type set up, but held constant "in the mathematics", so to speak.

In the following section I will argue that the epidemiological data concerning nuts as interpreted in multivariate regression models can straightforwardly be interpreted as supporting an appropriately phrased cp-generalization; and, further, that *contra* e.g. Earman et al. (2002), the evidence thus interpreted gives fully determinate content to the cp-clause hedging the relevant generalizations.

### 3. The content of 'ceteris paribus'

Multivariate regression models for large and varied data sets cannot in most cases feasibly include *all* possible confounding variables—typically, we cannot hope to control for everything that could conceivably interfere with the systems we are modeling. Nevertheless, the expectation of practitioners is that through judicious choice of the most important factors we can reduce to negligible amounts the remaining observed interference as it manifests itself for example in the variance of the data, or the number of outliers. In practical terms, the degree to which we succeed in measuring the approximately correct net effect of a causal factor depends on the quality of the data, the quality of the model, and on whether the quantitatively most important predictors have in fact been included in the model. Individual studies in nutrition science typically control for a great number and variety of causal factors, ranging from age, body mass index, history of hypertension and cigarette smoking, to menopausal status, vitamin intake, and degree of education (see Fig. 2). Given that 'controlling for' a factor means nothing else but observing the effects of varying one factor while holding equal or statistically adjusting for variation in all others, a natural—in fact, *the only plausible*—way to describe the evidential import of each of these models is that they support an appropriately phrased *ceteris paribus*-generalisation. For the literal meaning of the *ceteris paribus*-clause is, precisely, 'holding equal all other factors'.

Thus, I submit that the data as modeled in the respective regression models licenses a direct inference to the following conclusions:

- (1) *ceteris paribus*, consumption of  $\geq 5$  oz nuts p/week decreases the risk of coronary heart disease in women by 34%. (Hu et al., 1998)

<p style="text-align: center;"><u>Study N°1</u> (Coronary Heart Disease) Hu et al. 1998</p> <ul style="list-style-type: none"> <li>• age,</li> <li>• body mass index</li> <li>• cigarette smoking</li> <li>• history of hypertension</li> <li>• diabetes</li> <li>• hypercholesterolaemia</li> <li>• menopausal status</li> <li>• parental history of myocardial infarction</li> <li>• use of multivitamins</li> <li>• use of vitamin E supplements</li> <li>• alcohol consumption</li> <li>• aspirin use</li> </ul>	<ul style="list-style-type: none"> <li>• exercise</li> <li>• intake of saturated fat</li> <li>• intake of polyunsaturated fat</li> <li>• intake of trans fat</li> <li>• intake of fibre</li> <li>• intake of vegetables</li> <li>• intake of fruits</li> </ul>	<ul style="list-style-type: none"> <li>• total energy intake</li> </ul>
	<p style="text-align: center;"><u>Study N°2</u> (Type 2 Diabetes) Jiang et al. 2002)</p> <ul style="list-style-type: none"> <li>• body mass index</li> <li>• age</li> <li>• alcohol consumption</li> <li>• exercise</li> <li>• cigarette smoking</li> </ul>	<p style="text-align: center;"><u>Study N°3</u> (Prostate Cancer) Mills et al. 1989</p> <ul style="list-style-type: none"> <li>• age</li> <li>• education</li> <li>• consumption of meat</li> <li>• consumption of poultry</li> <li>• consumption of fish</li> <li>• consumption of beans</li> <li>• consumption of legumes</li> <li>• consumption of fruit</li> <li>• consumption of tomatoes</li> </ul>

Fig. 2. Potential confounding factors controlled for by Hu et al. (1998), Jiang et al. (2002), and Mills et al. (1989).

- (2) *ceteris paribus*, consumption of  $\geq 5$  oz nuts p/week decreases the risk of type II diabetes in women by 29%. (Jiang et al., 2002)
- (3) *ceteris paribus*, consumption of  $\geq 5$  oz nuts p/week decreases the risk of prostate cancer in men by 21%. (Mills, Beeson, Phillips, & Fraser, 1989).

Note that despite being prefixed by a hedging clause, each of these generalizations is *fully determinate*, because the precise content of each clause is provided by the model the generalization is associated with. In other words, the answer to the question ‘what exactly does “*ceteris paribus*” in front of statements (1)–(3) mean?’ is that it means ‘everything else being equal’, of course, but that this expression picks out different, though entirely *specific*, things in each case. For (1), the content of its cp-clause is given by the set of variables controlled for in Hu et al. (1998): the claim that ‘*ceteris paribus*, an increase in nuts consumption by women by amount  $x$  decreases the risk of coronary heart disease by amount  $y$ ’, is nothing but the claim that were women to increase their intake of nuts by  $x$ , then they would lower their risk of heart disease by  $y$ , *provided* that during that time the indicators for their age, body mass index, cigarette smoking, hypertension, and so on ... (insert here all factors listed under ‘Study N°1’ in Fig. 2), remain the same or within a prescribed range. *Mutatis mutandis*, for (2) and (3).

The above generalizations express neither strict causal laws (in the sense that they do not imply that specific types of event or state are guaranteed to cause other types of event or state), nor statistical laws (they do not quantify the probability of them doing so). Arguably, they express *functional* cp-laws, i.e. they are quantitative claims about the functional property of an underlying system, stating that a given increase or decrease of the value of one parameter leads to a given increase or decrease of another parameter, provided that all other parameters describing the state of the system remain the same or within a prescribed range (see Schurz, 2002, p. 351). The literal meaning of a given cp-clause hedging a *prima facie* law-like claim may thus be the paraphrase ‘everything else being equal’ or some other such paraphrase; but in the context of empirical research the clause must be understood in terms of the evidence that is taken to support the relevant claim. More specifically, it must be understood as referring to the known causal interferers and other possible defeaters of the generalization that have been controlled for in the models used to interpret the data supporting the claim.

The cp-clause encodes a portion of our empirical knowledge of the underlying structure of the physical system being modeled, and it picks out entirely different things depending on which claim it is prefixed to and which model it is associated with. It is therefore incorrect to say that the cp-clause ‘violates a pragmatic aspect of ‘laws’ in that it collapses together interacting conditions of very different kinds’ (Mitchell, 2002, p. 332). For in a hedged generalization from a different domain, say ‘*ceteris paribus*, changes in GDP growth depend linearly on changes in the unemployment rate’, the cp-clause must be cashed out in a series of precise conditions listing interferers and defeaters entirely distinct from those that apply to, say, ‘*ceteris paribus*, smoking causes cancer’, or indeed, ‘*ceteris paribus*, nuts are healthy’. When this is done using the appropriate models in the respective sciences, no collapsing together of conditions of different kinds takes place.

Now, probably the most common objection to this way of giving precise meaning to the cp-clause consists in raising a false-vacuous dilemma: hedged generalisations, so the argument, are strictly speaking false if their cp-clause does not explicitly exclude *all* events potentially interfering with the truth of the generalization; if the clause is taken to mean nothing more than ‘unless *something* interferes’, on the other hand, where the reference of ‘something’ is left vague or partially indeterminate, then they are vacuously true. I shall take a look at the vacuous horn of the dilemma first. In order to illustrate the alleged nefarious consequences of leaving the cp-clause indeterminate, philosophers of science often invoke patently outlandish law-like ‘regularities’. For example, in the literature the following propositions

- ‘*ceteris paribus*, if you’re thirsty you will eat salt’
- ‘*ceteris paribus*, all charged objects accelerate at  $10 \text{ m/s}^2$ ’
- ‘*ceteris paribus*, all human beings with normal neurophysiological equipment speak English with a southern U.S. accent’
- ‘*ceteris paribus*, nuts are fatal’

have all been purported to pass the test of *cp-lawhood*.<sup>5</sup> For each of these propositions, it is easy to conceive of a condition or set of conditions standing in for the cp-clause that would render the proposition true. For example, thirsty rational agents will eat salt if they happen to justifiably believe that salt quenches thirst, and charged objects will accelerate at  $10 \text{ m/s}^2$  if placed in an electromagnetic field of the right strength. The general argument is that if we fail to fully specify what we mean by ‘*ceteris paribus*’, then we cannot

<sup>5</sup> Mott (1992), Earman et al. (2002), Woodward (2002), Papineau (personal communication), respectively.



rule out *any number* of non-standard interpretations of the clause that allow for *ad hoc* conditions of this type, thereby rendering cp-laws inherently vacuous and destroying their scientific plausibility.

Yet, those who employ this argument do not dwell much on the conspicuous fact that none of these “counter-examples” would be *de facto* law-candidates at the present state of knowledge in the relevant science. This is crucial from our present perspective, since it means that these generalizations cannot be associated with a scientific model. For example, nuts consumption only risks to have fatal consequences for individuals susceptible of severe anaphylactic reactions due to nuts hyper-sensitization. The confounding factor ‘nuts allergy’ is implicitly (though not explicitly) excluded in all studies examining the nutritional effects of nuts, because food scientists typically remove cases of this type on the basis of their background knowledge concerning allergies during the data reduction stage, prior to applying the relevant multivariate model. *Mutatis mutandis* for the other examples. The view that cp-law hypotheses must explicitly exclude all potential interferers in order to avoid vacuity and achieve the status of “genuine” science is simply incongruent with how empirical researchers actually handle interfering factors. The latter are handled via (usually fairly simple) models, which specify initial and boundary conditions and limit the range and number of the forces attributed to the underlying system so as to exclude what are deemed to be interferers given the model. When the model and the corresponding interfering factors have been correctly specified—in the sense of providing a satisfactory statistical fit with the data while fulfilling other entrenched criteria for correct model choice—then we *eo ipso* have a specification of the content of the cp-clause hedging an appropriate cp-law candidate. Any *bona fide* exception to such a cp-law will only count as such if it occurs while cp-conditions are instantiated.

This, incidentally, means that testability, just like vacuity, may be not be the serious problem it is often billed as. According to Earman et al. (2002), attaching the phrase ‘*ceteris paribus*’ to any scientific claim instantly renders it untestable. Their argument is the following: testable laws must allow us to derive a prediction, normally with the help of auxiliary hypotheses. Given a law that ‘all As are Bs, *ceteris paribus*’, the *ceteris paribus* condition enters into one of the auxiliary hypotheses: the prediction that the next observed A should be B can be verified only if, precisely, other things are “equal” at the time of observation. Yet, Earman et al. believe that cp-clauses are intrinsically indeterminate, and hence that we can never know when other things are equal. Therefore we do not know when the auxiliary hypothesis is true, and consequently we cannot test cp-laws (op. cit. p. 293). Given the above it is quite clear that we can argue, *contra* Earman et al., that cp-laws can be tested in the ordinary way: take two populations that are similar with respect to the factors that have been controlled for in the regression model used by, say, Hu et al. (1998); expose one of them to the causal factors described in the antecedent of the law or observe this happening naturally, statistically adjusting for confounding factors; if there is a statistically significant deviation from the values predicted by the law in the exposed group which cannot be observed in the control group, then the causal factor being tested does not appear not have the presumed effect, and we must check our data or modify our model and the associated law-hypothesis.<sup>6</sup>

In fact, there is no special problem of testability that pertains to cp-laws that would not *also* pertain to all other scientific generalizations that we are liable to make on the basis of empirical observations. For the methodology—use of data models, background knowledge—and the types of evidential inference on the basis of which these generalizations are formulated, are the same.

#### 4. Cp-laws: metaphysically or epistemically incomplete?

This leaves us with the rejoinder that trying to eliminate the vagueness of the cp-clause in this way in order to ensure its non-vacuity and testability, is just *self-defeating*, since it lands us squarely on the false horn of the dilemma. If a law-like hedged generalization were to turn out false, it could not be a law; and yet, is there not a general consensus that it is *in principle impossible* to eliminate most cp-clauses by fully and explicitly specifying the conditions they describe, and hence that any generalisation they are prefixed to is strictly speaking false? However, the situation is not as clear-cut as this rejoinder might suggest. When something is described as ‘in principle impossible’ in science we must be careful to distinguish whether it is claimed to be metaphysically or epistemically so—in other words, whether it is impossible to spell out the content of the cp-clause due to some underlying metaphysical truth(s), or due simply to limitations in our knowledge. Earman et al. suggest that philosophical attention should be given only to those cp-clauses that refer to conditions such that ‘even with the best of knowledge, these conditions could not be made explicit, because they will comprise an indefinitely large set’, and that only laws hedged by clauses of that type deserve being called ‘genuine’ cp-laws (op. cit. p. 284). To them, cp-conditions that cannot be fully specified just because we do not know how to are theoretically uninteresting, for this would be simply a case of ‘where what’s needed is further scientific knowledge’ (ibid.). They are joined in this stance by many other cp-law theorists.<sup>7</sup>

Initially plausible as it may be, this view again does not sit well with how we handle hedged generalizations in scientific practice. Most scientists would be loath to make a clear distinction between the epistemically and the metaphysically impossible—whether out of professional modesty or out of an acute sense that scientific progress has on many occasions obliterated the line separating what we do not yet know (because we have not yet been able to observe or experimentally test it) from what we think we shall *never* know. As the cosmologist Max Tegmark puts it:

‘the borderline between physics and metaphysics is defined by whether a theory is experimentally testable, not by whether it is weird or involves unobservable entities. The frontiers of physics have gradually expanded to incorporate ever more abstract (and once metaphysical) concepts such as a round Earth, invisible electromagnetic fields, time slowdown at high speeds, quantum superpositions, curved space, and black holes.’ (Tegmark, 2003, p. 41)

What we at one point might have been tempted to declare ‘metaphysically impossible’ has an unsettling tendency, after a scientific revolution or a paradigm-shift, to reveal itself as having been simply an expression of the limitations of our knowledge, understanding and/or technological limitations prior to the shift.<sup>8</sup>

<sup>6</sup> Earman et al. do not deny of course that a cp-law as construed above (namely with a precise exception clause) would be testable. They simply do not think that the meaning of a genuine *ceteris paribus*-clause is fully determined by the model used to interpret the evidence in favour of the law.

<sup>7</sup> E.g. most of the contributors to the 2002 special issue of *Erkenntnis* on cp-laws seem to make that assumption.

<sup>8</sup> Another purely metaphysical concept, that of infinitely many parallel universes, has recently been transformed into a scientific hypothesis (supported by well-tested theories such as general relativity and quantum mechanics) through the discovery and increasingly precise measurement of cosmic microwave background radiation; thus belatedly vindicating D. Lewis’ modal realism, which used to be attacked on grounds of sheer metaphysical implausibility (“David Lewis believes in flying pigs!”). For accessible expositions, see Tegmark (2003, 2007).

This points to a general problem: our epistemic limitations are such that we cannot determine with any degree of certainty whether our inability at any one time to fully specify the completing conditions of a given law are to be attributed to deep metaphysical principles, or to simply a case of ‘where what’s needed is further scientific knowledge’. Neither can we put much stock, as Earman et al. do, in the reliability of ‘reasons to suspect that even with the best of knowledge, these conditions could not be made explicit’ (ibid.), for we simply do not have a sufficiently good grasp of what ‘the best of knowledge’ looks like. When you don’t know what you don’t know, then you can’t reliably *suspect* how much you don’t know, either. It is just inadvisable to attempt to distinguish on the basis of intuitions informed by current theory or on *a priori* grounds between metaphysically indeterminate cp-clauses and epistemically indeterminate ones. Wisely, empirical scientists do not do that: they take the horizon separating the knowable from the unknowable precisely for what horizons are—receding imaginary lines—and refuse to speculate if there lies a land forever beyond this line of the in-principle-unknowable, concerning themselves simply with what is currently unknown. So should philosophers of science.

This has important consequences for the position of the sceptic *vis-à-vis* cp-laws. The purported unsuitability of cp-clauses for genuine laws of nature in a given domain cannot be claimed to be due to deep metaphysical truths governing that domain—say, the special and irreducible type of contingency and/or complexity found in biological systems, as Mitchell (2002) has argued. Any such unsuitability must be shown to follow from epistemic incompleteness alone. A purely epistemic argument would go somewhat like this: no cp-generalization(s) will ever fully do justice to the evidence, since any given model derived from it will include but a tiny subset of all potentially confounding variables. Given that the causal influence of the missing predictor(s) will inevitably lead to deviations between the predicted and the observed value of the dependent variable(s), the evidence will always threaten to falsify the model as well as the associated generalization(s); the latter cannot therefore amount to laws of nature. For example, none of the regression models in studies (1)–(3) contained what could plausibly be seen as exhaustive lists of all causal factors impinging on the respective health outcome being monitored; moreover, the three studies showed differences in the types of factor controlled for, and we cannot exclude the possibility that addition of one, several, or indefinitely many predictors could have improved the relevant model’s fit with the data, that it would have further minimized variance, better accounted for outliers, and so on.

However, as Cartwright (1983) and others have convincingly argued, none of these epistemic points apply differentially only to the way in which cp-generalizations are related to their evidence: they are equally true of the way in which data in support of purportedly *strict* generalizations would be modeled, and of how the latter would be confirmed by the former (cf. also Lange, 2000). One possible conclusion from the lack of epistemic difference between strict and cp-laws is Cartwright’s, namely that *all* laws of nature—if conceived as Humean regularities—are strictly speaking false. Another is to take a scientific realist stance and to observe that practicing scientists will typically consider their currently best model (and with it the associated cp-generalizations) to be at least approximately true, if it accounts for the quantitatively most important factors, reduces the remaining observed interference to negligible amounts, is easily integrable with other models/theories, etc. On this stance, the fact that *perfect* models are out of our reach is neither surprising nor significant, given that we are not epistemic superheroes. The quasi constant replacement of older

models with (what are thought to be) slightly or even radically better ones is just business as usual in the search for universal empirical truths, of *both* the hedged *and* the non-hedged kind.

But I do not take a model-theoretic approach to cp-clauses to carry a commitment to any particular general view of science—both realism and anti-realism appear to be compatible with it. Neither does it imply a commitment to a general model-theoretic account of laws. According to the latter, models are the primary content-bearing vehicles of scientific knowledge, fulfilling the representational function hitherto associated with laws or theories. A well-known difficulty with this latter idea is that it is slightly mysterious how models can represent their target systems, given that they are not linguistic entities and often involve simplifications and idealizations; models are, at least in a literal sense, *false* representations of nature if they are representations at all. Yet, the theory of cp-clauses advanced here made no claims to representation: multivariate regression models applied to epidemiological data about nuts consumption do not represent the underlying phenomenon itself (i.e. the causal mechanism connecting anti-oxidants, phyto-nutrients, etc., with coronary heart disease, diabetes, or prostate cancer.) They are rather mathematical models of the data which that mechanism generates. Model-theorists say that we should understand laws of nature as ‘part of the characterization of an abstract model and thus being true of the model. [*Ceteris paribus*] qualifications, then, concern only the range of application of the model’ (Giere, 2004, p. 749). But there are many categories of scientific models, and propositions (1)–(3) cannot without awkwardness be taken as literal descriptions of so-called ‘bottom-up’ data models. The awkwardness disappears if we take them instead at their face value as descriptions of a regularity involving nuts and health in people. In other words, the claim on a pack of peanuts that ‘eating 1.5 oz of nuts per day may reduce the risk of heart disease’ is not about a multivariate regression model that’s applicable to questionnaire data, it’s about you and me.

## 5. A problem with Mill–Ramsey–Lewis

This does not mean that the present account of cp-laws is entirely without wider theoretical implications. Recently, some philosophers have made use of a version of the Mill–Ramsey–Lewis theory of laws of nature in order to explain how there could be fundamentally and irreducibly non-strict laws with ‘real exceptions’, i.e. metaphysically incomplete cp-laws in my sense above.<sup>9</sup> Schrenk (2007) defines a real exception to a law as a case in which the antecedent of the law is exemplified and the consequent is not, where this is *not* the result of the intervention of regular natural forces subject to other laws of nature; rather, real exceptions are the secular equivalent of a miracle (cf. also Braddon Mitchell, 2001, pp. 266–267). Schrenk presents the Mill–Ramsey–Lewis account as the best way of accommodating the possibility of violated laws: a law, albeit exception-ridden, might still be part of the ‘best system’ of laws, i.e. of the one true deductive system of axioms that achieves a best combination of simplicity and strength. ‘Strength’ in this context refers to descriptive power: a generalisation displaying a high degree of strength is one that is empirically more adequate by correctly describing more matters of fact about the world, than a weaker one. Descriptive power competes with simplicity: adding more axioms (i.e. laws) to a deductive system or increasing their syntactic complexity increases its strength at the cost of simplicity, while reducing the number of axioms or their complexity increases simplicity at the cost of descriptive power. There could be systems that are tied with respect to the combined criteria, in which case a

<sup>9</sup> E.g. Braddon Mitchell (2001), Schrenk (2007), and Callender & Cohen (2010).

generalisation would be a law if and only if it is a member of all best systems.

This is an intriguing suggestion, especially since the authors apply it also to cp-laws as I have conceived of them here, i.e. to epistemologically incomplete laws with mere ‘pseudo-exceptions.’<sup>10</sup> The attraction of Mill–Ramsey–Lewis is the following: it seems to offer a both plausible and elegant way to explain how a given law-like generalisation could be a law of nature even though it is not universally true; laws of nature could be “lossy”, in Braddon Mitchell’s phrase, and not always get all the facts right as long as they represent the best possible way to deductively systematize the way the world is. Mill–Ramsey–Lewis thus offers us a way to agree with Cartwright’s dictum that the laws of nature “lie”, without necessarily following her into an anti-Humean metaphysics according to which laws state more than just regularities in the mosaic of nature.

My criticism of this use of Mill–Ramsey–Lewis to solve the conundrum of cp-laws is that it not only fails to circumvent some of the standard objections to Mill–Ramsey–Lewis *qua* general theory of laws, it seems to render these objections even stronger. A worry commonly raised about Mill–Ramsey–Lewis is that it does not make it clear how our actual, historically conditioned, empirical investigations could ever produce the correct laws of nature, because the criteria by which we chose laws and theories in science are not (exclusively) the very general Mill–Ramsey–Lewis criteria of system choice: we have little reason to ‘plausibly expect science to reach one of Lewis’s best theories, even if all goes ideally well’ (Van Fraassen, 1989, p. 56). This worry is exacerbated when the laws in question are *ceteris paribus*: Mill–Ramsey–Lewis does not provide a scientifically plausible interpretation of cp-clauses, and more importantly, it does not provide a correct explanation of how cp-generalisations are discovered, why they are accepted, and believed. In fact, the theory quite severely clashes with scientific practice.

Schrenk (2007)’s account of special science cp-laws begins with the assumption of an epistemically ideal world, in which the best system of laws for each special science would contain a list of statements describing social, biological, psychological, etc., regularities appended with potentially indefinitely long lists of explicit exceptions (*ibid.*). In such a world, the cp-clause of each cp-law could be eliminated by fully describing the conditions the clause refers to; the resulting statements would qualify as the true laws of nature in virtue of their membership of the deductive system displaying the best balance between simplicity and strength. Schrenk claims that if one were to *delete* the lists of exceptions from the laws in this best system and retain only the statements about regularities themselves, then one would effectively translate the lengthy law statements of the ideal system into the law statements typical of the relevant special science, hedged with a cp-clause (*ibid.*). But this can’t be right. For the cp-laws typical of the special sciences often are descriptively highly inadequate; they describe what things would be like under conditions that do not necessarily obtain. The effect of taking away the clause which refers to these conditions leaves the remaining generalisation exceptionally weak in terms of the strength-criterion; if, on the other hand, one were to flesh out the cp-clause with a complete list of all exceptions that the law suffers (as in the ideal system), then this would render the resulting generalisation exceptionally weak in terms of the simplicity-criterion.

Take for example ‘*ceteris paribus*, sea turtles have long life spans’, a generalisation from marine biology. When we look at

the evidence supporting this claim, we find studies showing that the predation rate of, say, Green Sea Turtle hatchlings within the first hour of life averages 31%, and that most first year mortality for sea turtles likely occurs as a result of predation within the first hour of entering the sea (Gyuris, 1994, pp. 140–143). Generally, data on sea turtle survival displays so-called type III survivorship curves: sea turtles are organisms that have a very large rate of mortality when young, but those few individuals that survive their youth put significant energy into continued survival, since their minimal declines in fecundity with age guarantee that the longer they survive, the more progeny they will produce. What this means for the cp-clause hedging the generalisation ‘sea turtles have long life spans’ is this: given the varying predation rates during sea turtles’ different life-history stages and the fact that statistically the majority of them do not survive infancy, it is obvious that we cannot model their longevity (which is an inherited trait) on data about average life expectancy (which is not). ‘Lack of predation’ or a similar condition is a necessary hedge that must be built into longevity models for sea turtles, on pain of obtaining the paradoxical result that a species with genes that code for sexual maturity at 30 years should *also* have evolved genes that code for a life span of no more than 1 hour. ‘Sea turtles have long life spans’ thus illustrates what is true of all hedged generalisations, whether in the special sciences or elsewhere: ‘*ceteris paribus*, all As are Fs’ does not mean ‘most As are Fs’, nor does it express any particular probability that an A is F. In fact, when the cp-clause is not satisfied, it is frequently the case that very few or no As at all are Fs.

This wreaks havoc with any acceptability criterion for cp-laws derived from Mill–Ramsey–Lewis. Propositions such as ‘sea turtles have long life spans’ could not be members of the best system of laws because if they are not hedged by a crucial cp-clause they are very *weak*, failing to correctly describe the majority of facts—and yet they are also not any *simpler* than their descriptively more adequate opposite (‘sea turtles have short life spans’). On the other hand, adding a list of exceptions to the law that enumerates each case of a young individual falling prey to predators will leave the law exceptionally *complex*—though again not any more descriptively adequate than its *simpler* opposite. Generally speaking, cp-laws will have varying ratios of positive to negative instances depending on the type of model they are associated with, and some might have few or no positive instances at all when their hedging clause is left unspecified. Schrenk is aware of this (*op. cit.* p. 169), but he does not take it to be a significant problem, for he excludes approximations and idealisations from the purview of his account: ‘Idealisations and approximations are intentional distortions of reality. [...] False statements, however, cannot pick out laws and since laws are my subject I will have nothing to say about idealisations and approximations’ (*op. cit.* p. 20). The reader may be forgiven for asking what, under these circumstances, Schrenk’s ‘laws’ have to do with the sort of laws scientists normally talk about. For a best-system competition that takes no heed of the approximative or idealized nature of many empirical law-like generalizations will risk classifying as non-laws core scientific principles contained in every textbook. (The same problem obtains, *mutatis mutandis*, for the very similar proposals of Braddon Mitchell (2001) and Callender and Cohen (2010).)

Surely, it is reasonable for philosophers of science to adopt the desideratum that any theoretical account of laws ‘should make it plausible that laws of nature are the truths which science aims to discover’ (Van Fraassen, 1989, p. 55), at least in the long run. In the specific case of cp-laws, it is reasonable to demand of our

<sup>10</sup> Incidentally, Schrenk concedes that the notion of a law with ‘real exceptions’ is highly exotic, for the only known plausible candidate of an area of space-time where such exceptions take place are physical singularities, e.g. black holes (*op. cit.* pp. 164–165). He also acknowledges that the social sciences, in particular, don’t ostensibly contain laws with real exceptions in his sense; rather they look much more like they state laws implicitly hedged by *ceteris paribus*-clauses as conceived here (*ibid.*) Braddon Mitchell and Callender and Cohen, on the other hand, appear to be comfortable with the notion of secular miracles in the social sciences, too.



philosophical understanding of cp-laws that it harmonize with our interpretation of what scientists mean by a given cp-clause, of how they get by that meaning via the analysis of empirical data, and of how they put it to predictive and explanatory use. To paraphrase Nancy Cartwright on causes, ‘any metaphysical account of what cp-laws are that does not dovetail with what we take our best methods for finding them or the standard uses to which we put them should be viewed with suspicion.’<sup>11</sup>

## 6. Conclusion

This paper has taken the *explanandum* of a theory of cp-laws to be not ‘law-likeness’ in general, or the necessary and sufficient attributes of any statement purporting to describe a law of nature; nor indeed has it concerned itself with law-likeness specifically in nutrition science. Rather, my focus here was on the specific contribution, if any, of the cp-clause to otherwise law-like statements. I argued that *ceteris paribus* as a scientific concept is neither flawed nor intrinsically problematic when it is conceived as nothing more than an abbreviation for a specification of the conditions under which the relevant law holds—and that some aspects of scientific practice strongly suggest that it ought to be conceived thus. Using epidemiological studies in nutrition as an example I showed that, *contra* common claims in the literature, the cp-clause hedging a law-like generalization is fully determinate in content since the latter is given by the model used to interpret the relevant data in support of the generalization. Cp-laws thus understood are neither irredeemably vague, untestable, nor trivial. I argued, further, that they should not necessarily be viewed as simply *false*, either, because the objection that it is ‘in principle impossible’ that cp-clauses and their underlying models exclude every causal factor capable of falsifying the law, must be carefully interpreted. If it refers to epistemic impossibility, then it is a trivial observation any theory of science as performed by fallible cognitive agents must account for; if however it is thought to relate to metaphysical impossibility, as is commonly the case, then since the latter cannot reliably be inferred from epistemic impossibility, this leaves an open door for cp-laws that are at least approximately true, and at any rate not any less true than strict laws. Finally, I claimed that although a model-theoretic account of cp-laws is neutral with respect to general theories of science, it does seem to pose a problem for recent attempts to formulate a Mill–Ramsey–Lewis theory of cp-laws.

## References

- Braddon Mitchell, D. (2001). Lossy laws. *Nous*, 35(32), 260–277.
- Callender, C., & Cohen, J. (2010). Special sciences, conspiracy and the better best system account of lawhood. *Erkenntnis*, 73(3), 427–447.
- Cartwright, N. (1983). *How the laws of physics lie*. Oxford: Clarendon Press.
- Cartwright, N. (2002). In favor of laws that are not *ceteris paribus* after all. *Erkenntnis*, 57(3), 425–439.
- Cartwright, N. (2007). *Hunting causes and using them*. Cambridge: Cambridge University Press.
- Earman, J., Roberts, J. T., & Smith, S. (2002). *Ceteris paribus* lost. *Erkenntnis*, 57(3), 281–301.
- Fodor, J. A. (1991). You can fool some of the people all of the time, everything else being equal: hedged laws and psychological explanation. *Mind*, 100(1), 19–34.
- Fraser, G. E., Sabaté, J., Beeson, W. L., & Strahan, T. M. (1992). A possible protective effect of nut consumption on risk of coronary heart disease. The Adventist Health Study. *Archives of Internal Medicine*, 152, 1416–1424.
- Giere, R. (2004). How models are used to represent reality. *Philosophy of Science*, 71, 742–752.
- Gyuris, E. (1994). The rate of predation by fishes on hatchlings of the green turtle. *Coral Reefs*, 13(3), 137–144.
- Hausman, D. M. (1992). *The inexact and separate science of economics*. Cambridge: Cambridge University Press.
- Hempel, C. G. (1988). Provisos: A problem concerning the inferential function of scientific theories. In A. Grünbaum & W. C. Salmon (Eds.), *The limitations of deductivism* (pp. 19–36). Berkeley: University of California Press.
- Hu, F. B., Stampfer, M. J., Manson, J. E., Rimm, E. B., Colditz, G. A., Rosner, B. A., et al. (1998). Frequent nut consumption and risk of coronary heart disease in women: Prospective cohort study. *British Medical Journal*, 317, 1341–1345.
- Jenab, M., Sabate, J., Slimani, N., Ferrari, P., Mazuir, M., Casagrande, C., et al. (2004). Consumption and portion sizes of tree nuts, peanuts and seeds in the European Investigation into Cancer and Nutrition (EPIC) cohorts from 10 European countries. *British Journal of Nutrition*, 96(Suppl. 2), 12–23.
- Jiang, R., Manson, J., Stampfer, M. J., Liu, S., Willett, W. C., & Hu, F. B. (2002). Nut and peanut butter consumption and risk of type 2 diabetes in women. *Journal of the American Medical Association*, 288(20), 2554–2560.
- Kris-Etherton, P. M., Yu-Poth, S., Sabate, J., Ratcliffe, H. E., Zhao, G., & Etherton, T. D. (1999). Nuts and their bioactive constituents: Effects on serum lipids and other factors that affect disease risk. *American Journal of Clinical Nutrition*, 70(Suppl), 504S–511S.
- Lange, M. (2000). *Natural laws in scientific practice*. New York: Oxford University Press.
- Lange, M. (2002). Who’s afraid of *ceteris-paribus* laws? or: How I learned to stop worrying and love them. *Erkenntnis*, 57, 407–423.
- Lipton, P. (1999). All else being equal. *Philosophy*, 74(288), 155–168.
- Mills, P. K., Beeson, L. W., Phillips, R. L., & Fraser, G. E. (1989). Cohort study of diet, lifestyle, and prostate cancer in adventist men. *Cancer*, 64, 598–604.
- Mitchell, S. D. (2002). *Ceteris paribus*—An inadequate representation for biological contingency. *Erkenntnis*, 57(3), 329–350.
- Mott, P. (1992). Fodor and *ceteris paribus* laws. *Mind*, 101(402), 335–346.
- Peacocke, C. (1999). *Being known*. Oxford: Oxford University Press.
- Pietroski, P., & Rey, G. (1995). When other things aren’t equal: Saving “*ceteris paribus*” laws from vacuity. *British Journal for the Philosophy of Science*, 46(1), 81–110.
- Reutlinger, A. (2009). Review of Markus Schrenk, *The metaphysics of ceteris paribus laws*. *British Journal for the Philosophy of Science*, 60(1), 229–233.
- Sabaté, J., Ros, E., & Salas-Salvadó, J. (2006). Nuts: Nutrition and health outcomes. *British Journal of Nutrition*, 96(Suppl 2), S1–S2.
- Schiffer, S. R. (1991). *Ceteris paribus* laws. *Mind*, 100(397), 1–17.
- Schrenk, M. (2007). *The metaphysics of ceteris paribus laws*. Heusenstamm bei Frankfurt: Ontos Verlag.
- Schurz, G. (2001). Pietroski and rey on *ceteris paribus* laws. *British Journal for the Philosophy of Science*, 52(2), 359–370.
- Schurz, G. (2002). *Ceteris paribus* laws: Classification and deconstruction. *Erkenntnis*, 57(3), 351–372.
- Tegmark, M. (2003). Parallel universes. *Scientific American*, 288(5), 40–52.
- Tegmark, M. (2007). Many lives in many worlds. *Nature*, 448, 23–24.
- Tsai, C. J., Leitzmann, M. F., Hu, F. B., Willett, W. C., & Giovannucci, E. L. (2004). A prospective cohort study of nut consumption and the risk of gallstone disease in men. *American Journal of epidemiology*, 160(10), 961–968.
- U.S. Department for Health and Human Services (2005). *Dietary guidelines for Americans 2005*. Washington, DC: U.S. Government Printing Office.
- Van Fraassen, B. (1989). *Laws and symmetry*. Oxford: Oxford University Press.
- Woodward, J. (2000). Explanation and invariance in the special sciences. *British Journal for the Philosophy of Science*, 51(2), 197–254.
- Woodward, J. (2002). There is no such thing as a *ceteris paribus* law. *Erkenntnis*, 57(3), 303–328.

<sup>11</sup> Cartwright (2007, pp. 1–2); see also Reutlinger (2009, p. 232). Peacocke (1999) calls the requirement that for any domain of inquiry X we must harmonize our metaphysics of X with our epistemology of X, the ‘Integration Challenge’.