Do Statistical Laws Solve the 'Problem of Provisos'?

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1. Introduction

In 'Ceteris Paribus, There is No Problem of Provisos' (1999), John Earman and John T. Roberts defend the view that the laws of fundamental physics are expressed by universal generalizations. These fundamental physical laws, Earman and Roberts argue, are strict laws and not qualified by ceteris paribus clauses. As physics, the special sciences also use generalizations to explain, to predict and to intervene. But, in contrast to physics, these generalizations should not be understood as strict universal laws. Earman and Roberts's proposal is to interpret non-strict special science generalizations as statistical generalizations about correlations. Earman and Roberts claim that the statistical generalizations are not qualified by ceteris paribus (henceforth, cp) conditions. As a consequence of their view, neither the law statements of fundamental physics nor special science generalizations are cp-law statements. Hence, they conclude, there simply is no problem of provisos (that is, cp-conditions) that philosophers ought to address. In a sequel to Earman and Roberts (1999), Earman, Roberts and Smith pitilessly break the

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news to the "innamorati" of cp-laws: "the object of their affections does not exist" (Earman et al. 2002: 281). Not being committed to cp-conditions seems to be a prima facie advantage of their account of special science generalizations, because the exact meaning of cp-conditions is controversial.[2] Unfortunately, this account of special science laws has not received the attention it deserves, and since it does not have a name, I call it the *statistical account*.

In this paper, I leave aside Earman and Roberts's claim about the laws of physics (see Hüttemann [this volume] for a critical discussion). I focus on their statistical account of special science generalizations and present two objections. Section 2 introduces preliminaries for the discussion: the terminological distinction of lazy and non-lazy cp-laws, and Lange's dilemma. Section 3 argues that the statistical account – *pace* Earman and Roberts's intention – does not dispense with so-called 'non-lazy' cp-conditions. Section 4 presents the objection that the statistical account cannot be a general and complete account of special science laws, as it fails in the case of idealized special science generalizations.

2. Preliminaries

In order to provide a clear foundation for the discussion of the statistical account, I first review a useful distinction that is commonly drawn in the recent literature on cp-laws. Then I introduce Lange's dilemma for accounts of special science laws.

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[2] Two clarifications: (1) the focus of this paper is on cp-law statements and their truth-conditions. I am not primarily concerned with the metaphysics of cp-laws, which determines what cp-laws are in the world (for instance, regularities, dispositions, subjunctive facts etc.). (2) I take it as a premise that a generalization's lawhood is (partly) determined by whether the generalization is used to explain, predict and manipulate. To the best of my knowledge, this characterization accords with Earman and Roberts' views of the epistemic role of special science generalizations.

Lazy versus non-lazy cp-conditions. Earman and Roberts introduce the helpful distinction of lazy and non-lazy cp-conditions. They argue that a cp-clause is dispensable if all the conditions that have to obtain for the generalization to be true can be listed and it is merely a matter of convenience and the result of "laziness" – and not due to our presently incomplete state of knowledge – that the conditions are not listed explicitly. Earman and Roberts refer to such a finite list as a "lazy" cp-condition. Laws with lazy cp-conditions can easily be turned into strict laws. Earman and Roberts hold that only "non-lazy" cp-conditions are proper cp-conditions.[3] According to Earman and Roberts, a list of conditions is non-lazy if it cannot be completed for reasons that are not due to our presently incomplete state of knowledge.[4] Following Earman and Roberts's characterization of non-lazy cp-conditions, I distinguish two criteria of non-laziness:

1. cp-conditions are $\textit{non-lazy}_{\text{open}}$ iff the list of conditions cannot be completed – not even with the best and complete knowledge from all scientific disciplines – because the list of conditions is open-ended or infinite (Earman and Roberts 1999: 439, 441, 444, 467; Earman et al. 2002: 284).

[3] See Unterhuber (this volume) for a critical discussion of this claim.

[4] Earman et al. discuss this point explicitly: "There are two reasons why one might not be able to make explicit a more precise conditional [i.e. a strict law or a lazy cp-law]: (1) we do not know how to state the conditions under which the qualified regularity holds; or (2) there is reason to suspect that even with the best of knowledge, these conditions could not be made explicit, because they will comprise an indefinitely large set. The first possibility is not really relevant here; a putative example of a CP law whose CP clause could not be eliminated just because we didn't know how to eliminate it would not show that physics actually discovers CP laws, only that it might. For all we know, future empirical research could reveal the conditions under which the regularity obtains." (Earman et al. 2002: 284)

2. cp-conditions are *non-lazy*$_{\text{scope}}$ iff the set of conditions cannot be completed by special science S – even assuming complete knowledge about the domain of S – because the relevant cp-conditions for a law of S are (partly) outside of the conceptual and methodological scope of S and, thus, cannot be described by the theoretical means of S alone (Earman and Roberts 1999: 462-3).[5]

A list of cp-conditions is *non-lazy* iff at least one of the two criteria applies to the list. Respectively, a list of cp-conditions is *lazy* iff neither of the two criteria is satisfied.

It is a conceptual possibility that some set of cp-conditions is non-lazy$_{\text{scope}}$ and not non-lazy$_{\text{open}}$. That is, even if the relevant conditions cannot be stated in the vocabulary of a special science, there might be a finite list if one allows for further conceptual resources of, for instance, the physical sciences. Earman and Roberts ultimately reject non-lazy cp-laws, because they hold that such laws (a) face Lange's dilemma, (b) they are semantically defect (as their truth-conditions cannot be stated in a finite way), (c) they do not allow predictions, and, hence, cannot be confirmed by evidence (Earman and Roberts 1999: 465).

Lange's Dilemma. The problems concerning cp-laws are usually introduced by way of a dilemma, according to which law statements of the special sciences are either empirically false or trivially true. This dilemma is a challenge for accounts of special science laws, as it is a well-established criterion of the adequacy for such accounts to understand laws as true empirical statements. For instance, the claim 'if the supply of a commodity increases and the demand is constant, the price decreases' is false, if read as

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[5] This criterion presupposes that there are sufficiently clear-cut boundaries between the domains of various sciences. This is a non-trivial background assumption that Earman and Roberts as well as many friends of non-lazy cp-laws seem to accept.

a strict generalization, because there may be state interventions and other factors not mentioned in the law statement, which lead to counter-instances to the strict generalization. This is the *first horn* of the dilemma (*falsity*). If, on the other hand, the law is hedged by a (non-lazy) cp-clause, then the law statement reads 'if the supply of a commodity increases and the demand is constant, the price decreases – unless an interfering factors occurs'. If the claim about interfering factors is not made more precise and it cannot be expressed as a list of lazy cp-conditions, then the laws statement in question is in danger of lacking empirical content. This is the *second horn* of the dilemma (*triviality*). I refer to this dilemma as 'Lange's dilemma' (named after Marc Lange [1993: 235]; Reutlinger et al. 2011: section 4).

3. The Statistical Account

Earman and Roberts (1999: 448-460) raise forceful objections to accounts of cp-law statements. They conclude their discussion with the pessimistic verdict that accounts of non-lazy cp-law statements are "in sum, a royal mess" (Earman and Roberts 1999: 471). However, their main line of argument is that even if no such account can be provided, this is not a problem for philosophers who aim at understanding the role of laws in the sciences, because neither the fundamental physical laws nor special science generalizations are qualified by non-lazy cp-conditions. The laws of physics and generalizations of the special sciences hold if lazy conditions of application are satisfied. Lazy conditions of application do not deserve to be counted as genuine cp-conditions. Hence, Earman and Roberts argue that there is no "problem of provisos", which philosophers ought to solve.

Earman and Roberts grant that cp-laws may figure in "unfinished" scientific theories as "an element of a 'work in progress', an embryonic theory on its way to being developed to the point where it makes definite claims about the world" (Earman and Roberts 1999: 466). Cp-laws might play a role in the "context of discovery" of generalizations that are precise, well-defined and not qualified by non-lazy cp-conditions. In the case of the special sciences, such a well-defined and precise generalization is a *statistical* generalization; in physics, it is a strict law. Whereas strict laws and statistical generalizations deserve philosophical analysis, because they are part of "finished" theories, cp-laws do not deserve the same philosophical attention since the latter are merely part of "work-in-progress" or "embryonic" theories (Earman and Roberts 1999: 465-466).[6]

The statistical account portrays special science generalizations as statements about "actual correlations among variables" (Earman and Roberts 1999: 467). These statements assert "a certain precisely defined statistical relation among well-defined variables" (Earman and Roberts 1999: 467) of the following form:

> In population H, a variable P is positively statistically correlated with variable S across all sub-populations that are homogeneous with respect to the variables $V_1, \ldots, V_n$. (Earman and Roberts 1999: 467).

In general, two event types F and G are correlated iff F is statistically relevant for G. That is, F is positively statistically relevant for G – that is, P(G|F) > P(G|not-F); or F

[6] It is a strong and highly non-trivial claim that if some notion N is part of an "unfinished" theory, then N does not deserve philosophical attention. A friend of non-lazy cp-laws could question this assumption, but I will accept it for the sake of the argument.

has negative statistical relevance for G – that is, P(G|F) < P(G|not-F). Earman and Roberts stress that statistical relevance usually concerns the relevance of F for G given that other variables $V_1, \ldots V_n$ take certain values $v_1, \ldots, v_n$. In statistics, claims about correlations are typically interpreted as straightforward claims about relative frequencies. A relative frequency is the proportion of Gs among the Fs in a given domain (for instance, the proportion of people suffering from heart disease among the smokers in Great Britain). F is positively statistically relevant for G, if the frequency of Gs among the Fs is higher than the frequency of Gs among the non-Fs in a given domain (for instance, the proportion of people in Great Britain suffering from heart disease who also smoke is greater than the proportion of people suffering from heart disease who do not smoke).

Earman and Roberts present an example from Jeffrey Paiges's study of revolutions in agrarian societies: in agrarian societies, the economic organization of labor is strongly positively correlated with political activities. According to Paiges' study, there are two kinds of organizing labor: commercial hacienda systems (run by a single owner, with little autonomy of the workers) and plantation systems (run autonomously by the workers); political activities considered are revolts and labor reforms. Earman and Roberts hold that the above special science generalization should be reconstructed as follows: in all intended applications (that is, all agrarian societies, in which the values of the specified variables $V_1, \ldots, V_n$ are fixed), there is a high positive non-strict correlation between commercial hacienda systems and agrarian revolt, as well as between plantation systems and labor reform.

The statistical account is promising in three respects: *first*, Earman and Roberts claim that the account dispenses with dubious non-lazy cp-clauses. The conditions expressed by the fact that the variables $V_1, \ldots, V_n$ take the values $v_1, \ldots, v_n$ are *lazy* conditions, such as the "proximity of progressive urban political parties" (Earman and Roberts 1999: 468). *Second*, non-lazy cp-conditions are not required to capture the 'non-strict' and 'exception-ridden' character of special science generalizations, because statements about non-strict correlation naturally allow for exceptions. *Third*, it is rather uncontroversial that statistical generalization – unlike non-lazy cp-law statements – can be confirmed by evidence. In total, if the statistical account could be defended, the pay-off would be considerable.

To be fair, Earman and Roberts's view about the scope of the statistical account is vague. It is unclear whether the account is intended to cover all special science laws. Maybe Earman and Roberts are less ambitious. They might weaken their view by holding that some but not all special science laws are statements about correlations. However, Roberts's recent work on "nomic frequentism" suggests that what really is at stake is the stronger claim that all special science generalizations are laws about correlations involving lazy cp-conditions only (Roberts 2004; this volume; manuscript). Be that as it may. This stronger reading of the statistical account is the target of the remaining sections. This paper is not primarily concerned with the exegesis of Earman and Roberts's views. I think it is worth discussing the statistical account as such, since it looks exactly like what everybody wants: it is a simple and intelligible theory of special science laws without the need for mysterious cp-conditions.[7]

4. Statistical Generalizations – Not So Lazy After All

The first objection to the statistical account is that it does not get rid of non-lazy cp-

[7] Strevens [this volume] classifies a statistical approach to cp-conditions as a "softness" approach.

conditions (cf. Hüttemann and Reutlinger [2013: section 6] for the original version of the argument, which is elaborated here). If this is true, then the objection undermines the claim that the statistical account dispenses with non-lazy cp-conditions. This challenge is inspired by Carl Hempel's critical remark that a statistical account "faces the difficulty that scientific theories do not, in general, provide probabilistic laws that would obviate the need for provisos" (Hempel 1988: 152-153).

If the statistical account is true, then all special science generalizations describe correlations between variables given that other variables $V_1, \ldots, V_n$ are held fixed at particular values $v_1, \ldots v_n$. Generalizations about correlations merely involve *lazy* cp-conditions – that is, a set of conditions[8] $V_1=v_1, \ldots, V_n=v_n$ that is finite (i.e. not non-$\text{lazy}_{\text{open}}$) and entirely in the scope of the special science in question (i.e. not non-$\text{lazy}_{\text{scope}}$). The question I want to press is whether advocates of the statistical account are justified to claim that the relevant conditions are indeed lazy cp-conditions. This is not the case, or so I will argue, because sometimes a subset of the relevant conditions is not within the conceptual and methodological scope of the discipline in question. That is, I claim that there are cases of the following structure: F and G are actually correlated given $V_1=v_1, \ldots, V_n=v_n$ in population P and the correlation depends on the presence (or absence) of a non-$\text{lazy}_{\text{scope}}$ condition C. I propose to understand the claim 'a correlation depends on non-lazy condition C' as a claim about the failure of stability of a correlation under changes in C. Borrowing Woodward's (2010: 291-293) concept of stability, a correlation between F and G depends on background condition C iff the correlation fails to be stable (i.e. ceases to exist) under changes in C. More precisely, the actual correlation of Fs and Gs in population P depends on an actual non-$\text{lazy}_{\text{scope}}$ condition C

[8] A statement of the form '$V_i=v_j$' expresses the proposition that variable V_i takes value v_i.

obtaining in P iff the following subjunctive conditional is true: if C were not present, then the correlation of Fs and Gs would break down or, at least, be different than it actually is. Following Woodward's 'counterfactual' approach to stability, the subjunctive conditional might refer to another 'local' actual population, in which C is not present; the antecedent may also refer to a counterfactual population, in which C does not obtain. To illustrate the former case, suppose there is a correlation among economic variables obtaining in Spain but not in India (despite the fact that other variables are constant in both countries), because the existence of the correlation depends a (non-lazy) condition C, which actually prevails in Spain and which is actually absent in India. In this scenario, the relevant subjunctive conditional – assessing the dependence on C – refers to an actual local population (in India). However, the subjunctive conditional might also appeal to hypothetical populations, in which C is not satisfied, in order to determine whether a correlation depends on the actual condition C (see the examples below). Of course, evaluating such a counterfactual requires drawing on our background knowledge (including physical theories), which often allows us to infer what would happen in the absence of C. But this should not come as a surprise, if we are talking about non-lazy cp-conditions.[9]

Consider two examples instantiating this structure. First, according to the statistical account, economic generalizations such as 'rational agents maximize their expected utilities' and 'if the supply of a commodity increases and the demand remains constant, the price decreases' are reinterpreted as statements about actual non-strict

[9] I grant that if one is merely interested in whether a statement about a frequency in some actual population P is true, then the truth of this statement simply depends on the frequencies in P. However, I am interested in the stability of an actual frequency, which is a different matter and it compels us to consider frequencies in other local actual populations (besides P) or in counterfactual populations.

correlations such as: an increase in supply is actually correlated with a decrease of the price given the demand is constant. However, this correlation depends on the (non-lazy) complex background condition C that the agents interacting on the market are actually not repeatedly drugged, not disrupted by the outbreak of war or natural catastrophes, not subject to mass brain surgery etc. How does one determine whether this actual correlation (in some population P) depends on such a condition C? Following the recipe introduced above, the actual correlation depends on C iff the correlation breaks down in another local actual or a hypothetical population P*, in which C is not satisfied. That is, if the supply increased and the demand stayed constant and if, subsequently, all (or a large number of) the agents on the market were – unlike in the actual target population – repeatedly drugged, or disrupted by a natural catastrophe or a war etc., then the price for the good would not develop as predicted by the law about the actual correlation of supply and price. If this counterfactual is true, then the actual correlation of supply and price depends on non-lazy background conditions. If one told an economist that the correlation depends on the presence of C, then the economist would likely not consider this information as disconfirming the economic generalizations. Rather, the information that the actual correlation of price and supply depends on C reveals that conditions (including not dealing with repeatedly drugged agents, etc.) are (a) relevant for the truth of the economic statistical generalization in question and (b) outside of the scope of standard microeconomics.

Second, Lange's discussion of the area law provides another illustration of how a correlation depends on non-lazy background conditions. The area law reads as follows:

> [T]he equilibrium number S of a species of a given taxonomic group on an island (as far as creatures are concerned) increases [polynomially] with the islands area [A]: $S = c \times A^z$. The (positive-valued) constants c and z are specific to the taxonomic group and island group. (Lange 2002, 416f.)

Lange argues that the correlation of S and A partly depends on conditions that lie outside of the scope of island biogeography.[10] According to Lange, the actual correlation stated by the area law depends on the actual strength of the Earth's magnetic field. This dependence is revealed by the following counterfactual: "had Earth lacked a magnetic field, then cosmic rays would have bombarded all latitudes, which might well have prevented life from arising, in which case *S* would have been zero irrespective of *A*" (Lange 2002: 417). The correlation of S and A depends on the actual strength of the magnetic field if the following counterfactual is true: if the magnetic field were different than it actually is, then the correlation of S and A would be different than it actually is. Lange continues:

> The area law is not prevented from qualifying as an island-biogeographical law […] by its failure to be preserved under [this] […] counterfactual supposition […]. The supposition concerning Earth's magnetic field falls outside of island biogeography's range of interest. It twiddles with a parameter that island biogeography takes no notice of […]. (Lange 2002: 418)

[10] See note 5.

Stated in my terminology, the condition that the magnetic field of the Earth has its actual strength is a non-lazy$_{\text{scope}}$ condition (relative to island biogeography). Lange argues that this condition is "off-stage", since the condition is not expressed by the value of a variable explicitly figuring in the area law or any other law of this discipline (Lange [2002: 417-419] and Reutlinger et al. [2011: sect. 6.1] for an exposition of further examples of "off-stage" conditions).

What precisely do these two examples show? *First* and most importantly, the examples show that some statistical generalizations have non-lazy$_{\text{scope}}$ cp-conditions. Therefore, understanding special science laws as generalizations about correlations does not replace non-lazy cp-conditions. This result contradicts the original intention motivating the statistical account – that is, to obviate non-lazy cp-conditions. This strongly suggests the obligation to clean up the "royal mess" and to provide an account of non-lazy cp-conditions (for my own positive account, see Reutlinger 2011, 2013).

Secondly, if statistical generalizations do in fact have non-lazy cp-conditions, then the statistical account fails to avoid Lange's dilemma. Suppose there is a non-lazy$_{\text{scope}}$ condition C for a statistical generalization 'F and G are correlated given $V_1=v_1$, …, $V_n=v_n$', as presented in the two examples above. If C is not added to the antecedent of the statistical generalization, then the generalization is false in a population where C is often not satisfied. This is the first horn of Lange's dilemma. Now suppose that C is not merely non-lazy$_{\text{scope}}$ but also belongs to a list of non-lazy$_{\text{open}}$ cp-conditions C, D, E, … – this is a supposition one cannot exclude on a priori grounds. Then, according to the critics of cp-laws, the statistical statement 'F and G are correlated given $V_1=v_1$, …, $V_n=v_n$ and an open-ended list of conditions C, D, E, … obtains' is (in danger of becoming) a trivial truth such as 'A and B are correlated, unless something interferes'. This is the second horn of Lange's dilemma.

To sum up, the statistical account does not succeed in solving a problem it is designed for: it fails to dispense with non-lazy cp-conditions.[11]

5. Do Statistical Generalizations Sometimes Lie?

The second objection to the statistical account targets the claim that *all* special science laws can be understood as statistical generalizations. One kind of generalizations resists a straightforward statistical interpretation: highly idealized generalizations (cf. Hüttemann and Reutlinger 2013: section 5)[12]. Laws with idealized antecedent conditions (or idealized conditions of application) are frequent in the special sciences such as economics, population ecology, evolutionary biology and statistical mechanics. For instance, such idealizations include 'if conditions of perfect competition hold …', 'if the population size is infinite …', 'if mating occurs randomly …', 'if the molecules in a gas do not collide …', 'if a fluid consists of infinitely many interacting micro-components …' and so on (cf. Cartwright 1983, Batterman 2002, Weisberg 2007, Strevens 2008). Idealized laws are widely taken to be either literally false or to be vacuously true (Cartwright [1983: 47]; Pietroski and Rey [1995: 84]; Strevens [2008: ch 8]). An idealized law is either literally false because it is an inaccurate representation of the behavior of real systems (i.e. real markets, populations of organisms, gases, liquids

[11] See Strevens [this volume], for instance, for mechanistic approaches to cp-conditions.

[12] In this section, I refine Hüttemann and Reutlinger's (2014: section 5) argument and I extend the scope of their argument from actual frequentism (to which their discussion is restricted) to other interpretations of statements about objective probabilities and correlations.

etc.); or the law is vacuously true because it does not have any – or, at best, very few – actual instances and only applies to non-actual ideal systems. As this dilemma is most forcefully presented Nancy Cartwright's (1983) *How the Laws of Physics Lie*, I refer to it as 'Cartwright's dilemma'. This dilemma is challenging because it conflicts with the assumption that special science law statements are true and also have a considerable degree of empirical strength.

The resulting challenge for the statistical account is whether the statistical account applies to idealized generalizations. In section 5.1, I argue that the account does not apply, because idealized laws cannot be understood as statements about correlations, if correlations are understood in terms of frequencies (which is the received view of correlations in statistics). However, even if one adopts an alternative interpretation of the objective probabilities involved in the generalizations about correlations, the statistical account still fails for idealized generalizations.[13] In section 5.2, I discuss two prominent alternatives to frequentism (David Albert and Barry Loewer's, and Michael Strevens's accounts). This list of alternatives is certainly not exhaustive but I believe that the discussion suffices to establish that idealized generalizations pose a problem for the statistical account.

5.1 Laws about Frequencies

Earman and Roberts introduce the statistical account in terms of "actual correlations among variables" (Earman and Roberts 1999: 467). The most natural interpretation of this characterization is that special science generalizations are straightforward state-

[13] I assume that Earman and Roberts are exclusively concerned with objective probabilities.

ments about actual frequencies.[14] As stated in the section 3, an actual frequency of Fs and Gs is the number of actual Gs that are also F. Can idealized laws be interpreted as laws about actual frequencies? The prospects for this strategy are dim, because – given Cartwright's dilemma – idealized laws are either literally false or vacuously true. If the law is taken to be literally false, then it is not a good guide to the actual frequencies in the world; if the law is taken to be vacuously true then it does not represent actual frequencies at all and, hence, lacks empirical strength (cf. Backmann and Reutlinger 2014).[15]

I anticipate a question at this point. One might wonder whether laws about limiting (or hypothetical) frequencies are an alternative to laws about actual frequencies. Laws about limiting (or hypothetical) frequencies refer to the relative frequency of Fs among the Gs if there *were* an *infinite* sequence of Gs. For my current concerns, I merely wish to stress that, although appealing to limiting frequencies might be useful and the most attractive version of frequentism, statements about limiting frequencies are themselves idealized (as they are about hypothetical infinite frequencies) and, therefore, do not help to avoid Cartwright's dilemma (see Hayek [2009] for a detailed criti-

[14] Roberts [this volume: section 5] responds to the challenge arising from Cartwright's dilemma. He takes idealized law statements about actual frequencies to have the following form: a high proportion of Fs that are approximately F+ are also approximately G, where an F+ is an idealized version of a real F. This is certainly an improvement of the statistical account. However, it is controversial whether all kinds of idealized laws can indeed be taken to describe how things behave approximately (cf. Weisberg 2007; Strevens 2008).

[15] A proponent of the statistical account might reply that an idealized law statement is not literally true but such a law describes how non-ideal entities approximately behave. I agree with the critics that the concept of approximation is not helpful to improve our understanding of idealizations, because the notions of approximation and approximate truth stand in as much need of clarification as the notion of idealization does (Strevens 2008). Backmann and Reutlinger (2014) provide an in depth discussion of approximation and other accounts of idealizations.

cal discussion of hypothetical frequentism including objections targeting the idealized assumptions of this view).

5.2 Alternatives to Frequentism?

Proponents of the statistical account are not committed to a version of frequentism. Instead they might adopt an alternative interpretation of the (objective) probabilities involved in the characterization of correlations. Two currently prominent candidates are David Albert and Barry Loewer's best system account, and Michael Strevens's account of deterministic probabilities. I will explore whether one can understand idealized special science laws in terms of laws about correlations given one of those interpretations.

Albert and Loewer's Best System Account. Inspired by David Lewis's[16] best system account (henceforth, BSA) of laws and chances, Albert and Loewer hold that a contingently true generalization is a law of nature iff the generalization is an axiom or theorem of the deductive system summarizing the actual non-nomic facts in the simplest and empirically most informative way (Albert 2000, Loewer 2008). Albert and Loewer's best system consists of three axioms: (a) the dynamical laws of fundamental physics, (b) the past hypothesis, and (c) the statistical postulate. The past hypothesis is the claim that the initial macro-state of the universe was a low-entropy state. The statistical postulate states that there is a uniform probability distribution over the physically possible initial micro-states of the universe compatible with the past hypothesis (i.e. the possible realizers of the initial macro state referred to in the past hypothesis). Albert and Loewer call these axioms of their best system 'the mentaculus'. The mentaculus

[16] Lewis's (1994) best system account is not adequate for macro-probabilities (Callender and Cohen 2009: section 4.2). The discussion provided here generalizes to so-called "better best system accounts" as advocated by Callender and Cohen 2009, Schrenk 2007 and this volume, Unterhuber this volume (Backmann and Reutlinger 2014).

relates laws and objective probabilities straightforwardly: the probabilistic statements entailed by the mentaculus tell us what the objective probabilities are. The (deterministic and statistical) contingent generalizations entailed by the mentaculus are the (macro) laws. According to Albert and Loewer, the mentaculus entails (a probabilistic version of) the second law of thermodynamics. What is more, Albert and Loewer argue for the significantly stronger claim that not only the Second Law but all macroscopic (statistical) generalizations of the special sciences are theorems of the mentaculus.

Is it possible – in order to defend the statistical account – to interpret idealized laws as probabilistic laws (or as laws about correlations) according the mentaculus version of the BSA? By the standard definition, a deductive system includes only those true generalizations that offer the best balance of empirical strength and simplicity. According to Cartwright's dilemma, idealized laws are either literally false or they lack empirical strength, because they are vacuously true. I will discuss whether adopting the BSA can avoid Cartwright's dilemma.

First, suppose one opts for taking idealized law statements to be literally false. The standard version of the BSA requires that *each and every* axiom (and theorem) of the best system be a (contingently) *true* proposition, according to Lewis (1994) and others (including Earman 1986, Cohen and Callender 2009). Call this the 'veridicality requirement'. If the veridicality requirement holds and if one takes idealized laws to be false statements, then advocates of the BSA have to choose between two theoretical options: (i) either idealized laws are not laws according to the BSA. In that case the BSA is inadequate, as it does not apply to statements that the sciences treat as laws. (ii) Or a proponent of the BSA drops the veridicality requirement – i.e. the axioms and theorems of the best system may be literally false statements if they offer the best balance of simplicity and strength. Braddon-Mitchell (2001) defends such a non-standard 'instrumentalist' version of the BSA, which is compatible with accepting the falsity of idealized laws. However, I am discussing the standard version that requires law statements to be true.

Secondly, could one happily accept that vacuously true statements are axioms of the best system? A Humean might argue that idealized laws *qua* being vacuous laws earn their place in the best system, because idealized laws lead to a gain in simplicity that outweighs the superior fit of more realistic models (as a referee suggested).[17] Newton's first law might be an example fitting this characterization, because it may be taken as a proposition about actually non-instantiated behavior in the absence of forces (inertial behavior), which simplifies the overall theory. Call this strategy of justifying that a vacuous truth earns its place in the best system the 'simplification argument'. I concede that the BSA may be able to deal with some idealizations by appealing to the simplification argument. However, it remains to be shown whether this simplification argument applies to all idealized laws (for instance, to laws that apply if economic agents are perfectly rational, if mating is random, or if the molecules in a gas do not collide, and so on; cf. Strevens 2008: chapter 8). Whether the simplification argument can indeed be successfully applied to all idealized laws depends on an elaboration of the concepts of simplicity and strength (as used in the BSA). I side with Woodward's (forthcoming) and van Fraassen's (1989: 55-59) criticism that it is difficult to judge the soundness of arguments such as the simplification argument, because the central notions of simplicity and strength, to which the friends of best systems appeal, are (a)

[17] It is worth noting that several Humeans present strategies to avoid interpreting idealized law statements as vacuously true (most prominently, Earman et al. [2002: 285-287] and Roberts [this volume]).

notoriously unclear (and often taken to be primitive) and (b) it is, to say the least, an entirely open question whether the notions of simplicity and strength (as figuring in the BSA) play any role in the sciences at all.

Even if an idealized law is not an axiom it may be a *theorem* of the best system: the axioms might entail a vacuously true (statistical) generalization. This idea is in the spirit of the mentaculus approach, according to which all special science laws are theorems of the best system. If it were the case the axioms of the mentaculus entailed all the probabilistic special science laws, then some of these theorems might be vacuously true. According to this approach, the idealized probabilistic laws of the special sciences just are the vacuously true probabilistic theorems of the mentaculus – and, in the context of the statistical account, the vacuously true theorems of the mentaculus that are statements about correlations. This strategy embraces the vacuity horn of Cartwright's dilemma. The most urgent trouble with adopting this strategy is that there is no positive evidence to believe that the mentaculus does in fact entail all of the required special science laws (about correlations). Although one might grant that Albert and Loewer convincingly show that the second law of thermodynamics is a theorem of the mentaculus, the critics correctly insist that it is an entirely open question whether this success can be extended to laws of the special sciences in general (Cohen and Callender [2010: 437-439]; Weslake forthcoming). Therefore, the mentaculus account cannot be used to interpret idealized special science generalizations as vacuous statements about correlations.

Strevens's Account of Deterministic Probabilities. Michael Strevens accounts for special science macro-probabilities by way of explaining statistical patterns as the result of a process that involves two kinds of facts: (a) deterministic dynamical micro-laws describing an underlying micro-process, and (b) a probability distribution over initial conditions. For instance, a statistical pattern, which Strevens aims to explain, is that a sequence of (actual) coin tosses and outcomes of heads and tails is a Bernoulli process with the probability of 0.5 (Strevens 2003: 30; Strevens 2008: 369). Statistical patterns are "a certain sort of long-term statistical order, namely, a stable frequency or sets of frequencies" (Strevens 2003: 30). The explanation of a statistical pattern requires that the micro-dynamics have a feature Strevens calls "micro-constancy", while the probability distribution over initial conditions ought to be "macro-periodic" (Strevens 2003: 47-60; Strevens 2008: 368-379). The basic idea of these requirements is as follows: the micro-laws are micro-constant if (1) a small change in initial micro-conditions can lead – in according with the dynamical micro-laws – to a different outcome, and (2) for any 'small' region in the space of possible initial conditions, the proportion of different initial conditions producing particular types of outcomes is the same. Macro-periodicity is a 'smoothness' requirement for the probability distribution over initial conditions.

Concerning the merit of the statistical account, the crucial question is whether Strevens's account applies to all special science generalizations including idealized ones. Strevens's account applies iff two conditions are satisfied: first, each and every special science generalization instantiates a statistical pattern. Second, each of these patterns can be explained with the help of micro-constant laws and a macro-periodic probability distribution over initial conditions. Cartwright's dilemma is a reason to believe that the first condition is not generally met, because some special science laws are idealized and do not instantiate statistical patterns. Since statistical patterns are nothing but long-term stable frequencies, the objection targeted against the actual frequentist reading (see above) can be reapplied here: if an idealized law is taken to be literally false, then it is not a good guide to the 'correct' frequencies in the world; if the law is vacuously true, then it does not represent frequencies at all and lacks empirical strength. Thus, Cartwright's dilemma supports the claim that Strevens's account of macro-probabilities cannot be applied to idealized laws of the special sciences.[18]

This result need not worry Strevens, because he does not adopt the statistical account. Instead he provides independent accounts of deterministic and probabilistic special science laws (Strevens 2008: ch. 7, 10), non-lazy cp-laws (Strevens 2012), and – most strikingly – of idealizations in terms of factors that do not make an explanatory difference (Strevens 2008: chapter 8).

To recap section 5, I have discussed three interpretations of the objective probabilities referred to in statements about correlations (frequentism, the mentaculus, and Strevens's account). None of these interpretations support the claim that idealized laws can be straightforwardly understood as laws about correlations. This result shows that the statistical account is not a complete account of special science laws.

6. Conclusion

According to the statistical account, special science laws are laws about correlations. At first glance, this account is attractive because it captures the non-strict character of special science generalizations without commitment to potentially problematic non-lazy cp-conditions. I have presented two challenges to the statistical account. According to

[18] It is also an open question whether the underlying micro-processes of all special science laws are subject to micro-constant laws. Strevens sketches how his account might be extended from statistical mechanics and population ecology to the social sciences and evolutionary biology (Strevens 2003: chapter 5).

the first challenge, the statistical account does not get rid of non-lazy cp-conditions. This result undermines one of the alleged central advantages of the statistical account. The second challenge is that the statistical account, *qua* general theory of special science laws, is weakened by the fact that idealized law statements resist a purely statistical interpretation.

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