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LAWS, CETERIS PARIBUS CONDITIONS, AND THE DYNAMICS OF BELIEF

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Abstract: The characteristic difference between laws and accidental generalizations lies in our epistemic or inductive attitude towards them. This idea has taken various forms and dominated the discussion about lawlikeness in the last decades. Likewise, the issue about *ceteris paribus* conditions is essentially about how we epistemically deal with exceptions. Hence, ranking theory with its resources of defeasible reasoning seems ideally suited to explicate these points in a formal way. This is what the paper attempts to do. Thus it will turn out that a law is simply the deterministic analogue of a sequence of independent, identically distributed random variables. This entails that de Finetti's representation theorems can be directly transformed into an account of confirmation of laws thus conceived.

1. Preparations[1]

Laws are true lawlike sentences. But what is lawlikeness? Much effort went into investigating the issue, but the richer the concert of opinions became, the more apparent their deficiencies became, too, and with it the profound importance of the issue for epistemology and philosophy of science.

The most widely agreed prime features are that laws, in contrast to accidental generalizations, support counterfactuals, have explanatory power, and are projectible from, or confirmed by, their instances. These characteristics have long been recognized. However, the three topics they refer to – counterfactuals, explanation,

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[1] I am deeply indebted to Christopher von Bülow, Ludwig Fahrbach, Volker Halbach, Kevin Kelly, Manfred Kupffer, Arthur Merin, and Eric Olsson for a great lot of valuable comments. I have taken up many of them, but I fear I have dismissed the more important ones, which showed me how many issues would need to be clarified and substantiated, and which would thus require a much longer paper. I am also indebted to Ekkehard Thoman for advice in Latin.

Nancy Cartwright remarks, at the very end of the introduction of her book (1989), that my views on causation are closest to hers. The closeness, though, may not be easy to discover. This is the first paper for several years in which I continue on our peculiar harmony. I dedicate it to her.

and induction – were little elaborated in the beginning and are strongly contested nowadays. Moreover, the interrelations between these subjects were quite obscure. Hence, these features did not point to a clear view of lawlikeness, either. In this paper, I try to advance the issue. We shall see that the advance naturally extends to *ceteris paribus* laws, the general topic of this collection. Let me start with three straight decisions.

The first decision takes a stance on the priority of the prime features. I am convinced that it is the inductive behavior associated with laws which is the most basic one, and that it somehow entails the other prime features. I cannot justify this stance in a few lines. Suffice it to say that my study of causation (1983) led me from Lewis' (1973) theory of counterfactuals over Gärdenfors' epistemic account of counterfactuals (cf., e.g., Gärdenfors 1981) ever deeper into the theory of induction where I finally thought I had reached firm ground. In Spohn (1991) I explained my view on the relation of induction to causation and thus to explanation. However, I did not return to counterfactuals (because I always felt that this subject is overlaid by many linguistic intricacies that are quite confusing). My decision finds strong support in Lange (2000) who starts investigating the relation between laws and counterfactuals and also arrives at induction as the most basic issue.

The second decision concerns the relation between laws and their prime features. When inquiring into lawlikeness the idea often was to search for something which *allows* us to use laws in induction, explanation and counterfactuals in the way we do. That is, given that induction is really the most basic aspect, lawlikeness should be something that *justifies* the role of laws in induction. This idea issued in perplexity; no good candidate could be found providing this justification.

There is an alternative idea, namely that lawlikeness is *nothing but* the role of laws in induction. In view of the history of inductive scepticism from Hume to Goodman – which made us despair of finding a deeper justification of induction and taught us rather to describe our inductive behavior and to inquire what is rational about it while being aware that this inquiry may produce only partial justification – this idea seems to be the wiser one. I do not mean to suggest that the lessons of inductive scepticism have been neglected; for instance, Lange (2000) endorses these lessons when explaining what he calls the root commitment concerning the inductive strategies associated with laws. But it is important to be fully aware of these lessons, and hence I shall pursue here the second idea and foreswear the search for deeper justifications. We shall see that we can still say quite a lot about rational induction.

We are thus to study the inductive properties of laws. This presupposes some account of induction or confirmation within which to carry out the study. This is what my third decision is about. I think that on this matter philosophy of science went entirely wrong in the last 25 years. Bayesianism was always strong, and rightly so. In the 1950's and 60's much effort also went into the elaboration of a qualitative confirmation theory. However, this project was abandoned in the 70's. The main reason was certainly that the efforts were not successful at all. Niiniluoto (1972) gives an excellent survey that displays the incoherencies of the various attempts. An additional reason may be the rise and success of the theory of counterfactuals, which answered many problems in philosophy of science (though not problems of induction) and thus attracted a lot of the motivation originally directed to an account of induction.

In any case, the effect was that Bayesianism was more or less the only remaining well-elaborated alternative. This hindered progress, because deterministic laws and probability do not fit together well. Deterministic laws are not simply the limiting case of probabilistic laws, just as deterministic causation is not the limiting case of probabilistic causation. It is, for instance, widely agreed that the entire issue of *ceteris paribus* laws, to which we shall turn below, cannot find an adequate probabilistic explication. We find a parallel in the disparity between belief, or acceptance-as-true, and subjective probability, which was highlighted by the lottery paradox and has as yet not found a convincing reconciliation. My conclusion is, though I have hardly argued for it, that Bayesianism is of little help in advancing the issue of lawlikeness.

Philosophical logic was very active since around 1975 in producing alternatives, though not under the labels "induction" or "confirmation". However, these activities were hardly recognized in philosophy of science. Instead, they radiated to AI where they were rather successful. It is precisely in this area where we shall find help. Let me explain.

What should we expect an account of induction to achieve? I take the view (cf. Spohn 2000) that it is equivalent to a theory of belief revision or, more generally, to an account of the dynamics of doxastic states. This is why the topic is so inexhaustible. Everybody, from the neurophysiologist to the historian of ideas, can contribute to it, and one can deal with it from a descriptive as well as a normative perspective.

Philosophers, I assume, would like to come up with a very general normative account. Bayesianism provides such an account that is almost complete. There, rational doxastic states are described by probability measures, and their rational dynamics is described by various conditionalization rules. As mentioned, however, in order to connect up with deterministic laws, we should proceed with an account of doxastic states which represents plain belief or acceptance-as-true. Doxastic logic is sufficient for the statics, but it does not provide any dynamics. Probability < 1 cannot represent belief, because it does not license the inference from the beliefs in two conjuncts to the belief in their conjunction. Probability 1 cannot do it, either, because we would like to be able to update with respect to information previously disbelieved, because disbelieved propositions would have probability 0 according to this approach, and because Bayesian dynamics does not provide an account of conditionalization with respect to null propositions (that is why I called Bayesianism almost complete). Hence, Bayesianism is unhelpful. Belief revision theory (cf., e.g., Gärdenfors 1988) was deviced to fill the gap. Unfortunately, the dynamics it provides turned out to be incomplete as well (cf. Spohn 1988, sect. 3). There have been several attempts to plug the holes (cf., e.g., Nayak 1994 and Halpern 2001), but I still think that ranking theory, proposed in Spohn (1983, sect. 5.3, and 1988), though under a different name, offers the most convincing account for a full dynamics of plain belief.

In any case, this is my third decision: to carry out my study of the inductive behavior of laws strictly in terms of the theory of ranking functions. This framework may be unfamiliar, but the study will not be difficult, since ranking theory is a very simple theory. Still, there will be little space for broader discussion. Some of my results may appear trivial and some strange. On the whole, though, the study seems to me to be illuminating. But see and judge by yourself!

The plan of the paper is now almost obvious. In section 2 I shall introduce the theory of ranking functions as far as needed. Section 3 explicates lawlikeness, i.e., the difference between laws and accidential generalizations insofar as it can be expressed in ranking terms. We shall see that this explication naturally leads to an inquiry of the role of *ceteris paribus* conditions and the like, a task taken up in section 4. Since section 3 analyzes belief in a law not as a belief in a regularity or some more sophisticated proposition, but rather as a certain inductive attitude, the immediate question arises how a law, i.e., such an inductive attitude, can be confirmed. This crucial question is addressed in section 5. Section 6 will close with a few comparative remarks.

I thus focus entirely on the epistemological aspects of laws. I do not deny, but only neglect that laws have important metaphysical aspects as well. I have been less negligent in Spohn (1993), where I tried to understand causal laws as objectifications of inductive schemes, and in Spohn (1997), where I discussed both aspects of reduction sentences, the laws associated with disposition predicates. The two papers thus partially precede and partially transcend the present paper, and the unity of the three papers is less than perfect.

2. Ranking Functions

Let us start with a set *W* of possible worlds, small rather than large worlds, as we shall see soon. Each subset of *W* is a truth condition or *proposition*. I assume propositions to be the objects of doxastic attitudes. Thus I take these attitudes to be intensional. We know well that this is problematic, and we scarcely know what to do about the problem. Hence, my assumption is just an act of front alignment.

The assumption also entails that we need not distinguish between propositions and sentences expressing them. Hence, I shall often use first-order sentences to represent or denote propositions and shall not distinguish between logically equivalent sentences, since they express the same proposition.

That is all we need to introduce our basic notion: κ is a *ranking function* (for *W*) iff κ is a function from *W* into **N** (the set of non-negative integers) such that $\kappa(w) = 0$ for some $w \in W$. For each proposition $A \subseteq W$ the *rank* $\kappa(A)$ of *A* is defined by $\kappa(A) = \min \{\kappa(w) \mid w \in A\}$ and $\kappa(\emptyset) = \infty$. For $A, B \subseteq W$ the (*conditional*) *rank* $\kappa(B \mid A)$ of *B given A* is defined by $\kappa(B \mid A) = \kappa(A \cap B) - \kappa(A)$. Since singletons of worlds are propositions as well, the point and the set function are interdefinable. The point function is simpler, but auxiliary, the set function is the one to be interpreted as a doxastic state.

Indeed, ranks are best interpreted as *grades of disbelief*. $\kappa(A) = 0$ says that *A* is not disbelieved at all. It does not say that *A* is believed; this is rather expressed by $\kappa(\overline{A}) > 0$,[2] i.e., that non-*A* is disbelieved (to some degree).[3] The clause that $\kappa(w) = 0$ for some $w \in W$ is thus a *consistency* requirement. It guarantees that at least some proposition, and in particular *W* itself, is not disbelieved. This entails the *law of negation*: for each $A \subseteq W$, either $\kappa(A) = 0$ or $\kappa(\overline{A}) = 0$ or both.

The set $C_\kappa = \{w \mid \kappa(w) = 0\}$ is called the *core* of κ (or of the doxastic state represented by κ). C_κ is the strongest proposition believed (to be true) in κ. Indeed, a

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[2] $\overline{A}$ is the complement or the negation of *A*.

[3] I apologize for the double negation; after a while one gets used to it.

proposition is believed in κ if and only if it is a superset of C_κ. Hence, the set of beliefs is *deductively closed* according to this representation.

There are two laws for the distribution of grades of disbelief. The *law of conjunction*: $\kappa(A \cap B) = \kappa(A) + \kappa(B \mid A)$, i.e., the grade of disbelief in A and the grade of disbelief in B given A add up to the grade of disbelief in A-and-B. And the *law of disjunction*: $\kappa(A \cup B) = \min\{\kappa(A), \kappa(B)\}$, i.e., the grade of disbelief in a disjunction is the minimum of the grades of the disjuncts. The latter is again only a consistency requirement, though a conditional one; if that law would not hold the inconsistency could arise that both $\kappa(A \mid A \cup B)$, $\kappa(B \mid A \cup B) > 0$, i.e., that both A and B are disbelieved given A-or-B.

According to the above definition, the law of disjunction indeed extends to disjunctions of arbitrary cardinality. I find this reasonable, since an inconsistency is to be avoided in any case, be it finitely or infinitely generated. Note that this entails that each countable set of ranks has a minimum and thus that the range of a ranking function is well-ordered. Hence, the range **N** is a natural choice.[4]

However, here we better avoid all complexities involved in infinity. Therefore I shall outright assume that we are dealing only with finitely many worlds and hence only with finitely many propositions. This entails that each world in W (or the set of its distinctive features) is finite in turn. Hence, as announced, they are small worlds. One may think that this is a strange start for an investigation of natural laws. However, an analysis of lawlikeness should work also under this finiteness assumption. After all, our world seems both to have laws and to be finite. Generalizing my observations below to the infinite case would require a separate paper.

There is no need here to develop ranking theory extensively. A general remark may be more helpful: ranking theory works in almost perfect parallel to probability theory. Take any probability theorem, replace probabilities by ranks, the sum of probabilities by the minimum of ranks, the product of probabilities by the sum of ranks, and the quotient of probabilities by the difference of ranks, and you are almost guaranteed to arrive at a ranking theorem. For instance, you thus get a ranking version of Bayes' theorem. Or you can develop the whole theory of Bayesian nets in ranking terms. And so on. The general reason is that one can roughly interpret ranks as the orders of magnitude of (infinitesimal) probabilities.

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[4] In Spohn (1988) I still took the range to consist of arbitrary ordinal numbers. But the advantages of this generality did not make up for the complications.

The parallel extends to the laws of doxastic change, i.e., to rules of conditionalization. Thus, it is at least plausible that ranking theory provides a complete dynamics of doxastic states (as may be shown in detail; cf. Spohn, 1988, sect. 5).

It is still annoying, perhaps, that belief is not characterized in a positive way. But there is remedy: β is the *belief function* associated with κ (and thus a belief function) iff β is the function assigning integers to propositions such that $\beta(A) = \kappa(\overline{A}) - \kappa(A)$ for each $A \subseteq W$. Similarly, $\beta(B \mid A) = \kappa(\overline{B} \mid A) - \kappa(B \mid A)$. Recall that at least one of the terms $\kappa(\overline{A})$ and $\kappa(A)$ must be 0. Hence, $\beta(A) > 0$, < 0, or $= 0$ iff, respectively, A is believed, disbelieved, or neither; and A is the more strongly believed, the larger $\beta(A)$. Thus, belief functions may appear to be more natural. But their formal behavior is more awkward. Therefore I shall use both notions.

Above, I claimed that a full dynamics of belief is tantamount to an account of induction and confirmation. So, what is confirmation with respect to ranking functions? The same as elsewhere, namely *positive relevance*: *A confirms* or is a *reason for B* relative to κ iff $\beta(B \mid A) > \beta(B \mid \overline{A})$, i.e., iff $\kappa(\overline{B} \mid A) > \kappa(\overline{B} \mid \overline{A})$ or $\kappa(B \mid A) < \kappa(B \mid \overline{A})$ or both.[5]

There is an issue here whether the condition should require $\beta(B \mid A) > \beta(B)$ or only $\beta(B \mid A) > \beta(B \mid \overline{A})$, as stated. In the corresponding probabilistic case, the two conditions are equivalent if all three terms are defined, but the first condition is a bit more general, since it may be defined while the second is not. That is why the first is often preferred. In the ranking case, however, all three terms are always defined, and the second condition may be satisfied while the first is not. In that case the second condition on which my definition is based seems to be more adequate.[6]

A final point that will prove relevant later on: Ranking functions can be mixed, just as probability measures can. For instance, if κ_1 and κ_2 are two ranking functions for W and if κ^* is defined by

$$\kappa^*(A) = \min\{\kappa_1(A), \kappa_2(A) + n\} \quad \text{for some } n \in \mathbf{N} \text{ and all } A \subseteq W,$$

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[5] I believe that if epistemologists talk of justification and warrant, they should basically refer to this relation of *A* being a reason for *B*; cf. Spohn 2001. That's, however, a remark about a different context.

[6] A relevant argument is provided by the so-called problem of old evidence. The problem is that after having accepted the evidence it can no longer be confirmatory. However, this is so only on the basis of the first condition. According to the second condition, learning about *A* can never change what is confirmed by *A*, and hence the problem does not arise. This point, or its probabilistic analogue, is made by Joyce (1999, sect. 6.4) with the help of Popper measures.

then κ^* is again a ranking function for W. Or more generally, if K is a set of ranking functions for W and ρ a ranking function for K, then κ^* defined by

$$\kappa^*(A) = \min\{\kappa(A) + \rho(\kappa) \mid \kappa \in K\} \quad \text{for all } A \subseteq W$$

is a ranking function for W. The function κ^* may be called the *mixture* of K by ρ.

This is all the material we shall need. I hope that the power and beauty of ranking theory is apparent already from this brief introduction. I have not argued here that if one wants to state a full dynamics of plain belief or acceptance-as-true, one must buy into ranking theory. I did so in Spohn (1988, sect. 3). Even that argument may not be entirely conclusive. However, I guess the space of choices is small, and I would be very surprised if a simpler choice than ranking theory were to be available.

Be this as it may, let us finally turn to our proper topic, the epistemology of laws.

3. Laws

Let me start with a simple formal observation. Given some ranking function κ, to believe $A \wedge B$ means that $C_\kappa \subseteq A \cap B$, i.e., $\kappa(\neg A \vee \neg B) > 0$, i.e., $\min\{\kappa(\neg A), \kappa(\neg B)\} > 0$. This, however, can be implemented in many different ways. In particular, it leaves open how $\kappa(\neg A \vee \neg B)$ relates to $\kappa(\neg A)$ and $\kappa(\neg B)$ and thus whether or not $\kappa(\neg B \mid \neg A) = 0$. Hence, if you start with believing $A \wedge B$, but now learn that $\neg A$ obtains, you may, or may not, continue to believe B, depending on the value of $\kappa(\neg B \mid \neg A)$.

Basically the same point applies to believing a universal generalization. This, I propose, is the clue to understanding laws. Let us take $G = \bigwedge x(Px \rightarrow Qx)$ as our prototypical generalization ($\rightarrow$ always denotes material implication). I have already simplified things by assuming the worlds in W to be finite. This entails that the quantifier in G ranges over some finite domain D. For $a \in D$, let G_a be the instantiation of G by a, i.e., $G_a = Pa \rightarrow Qa$. Now to believe G in κ means that $C_\kappa \subseteq G$, i.e., $\kappa(\neg G) > 0$, i.e., $\min\{\kappa(\neg G_a) \mid a \in D\} > 0$. Thus, the generalization is believed as strongly as the weakest instantiation.[7]

[7] Note, by the way, that this would also hold for an infinite domain of quantification. Hence, for ranking theory there is no problem of null confirmation for universal generalizations which beset Carnap's inductive logic.

Let us assume, moreover, that this is the only belief in κ, i.e., that $C_\kappa = G$; thus, no further beliefs interfere. This entails in particular that $\kappa(Pa \wedge Qa) = \kappa(\neg Pa \wedge Qa) = \kappa(\neg Pa \wedge \neg Qa) = 0 < \kappa(Pa \wedge \neg Qa)$ for each $a \in D$ and hence that $\kappa(\neg Qa \mid Pa) > 0$, i.e., that Pa is positively relevant for Qa. In other words, under this assumption the belief in the material implication $Pa \rightarrow Qa$ is equivalent to the positive relevance of Pa for Qa.

Again, the belief in G can be realized in many different ways. Let me focus for a while on two particular ways, which I call the "persistent" and the "shaky" attitude. If you learn about positive instances, G_a, G_b, etc. you do not change your beliefs according to κ, since you expected them to be positive, anyway.[8] The crucial difference emerges when we look how you respond to negative instances, $\neg G_a$, $\neg G_b$, etc. according to the various attitudes:

If you have the *persistent* attitude,[9] your belief in further instantiations is unaffected by negative instances, i.e., $\kappa(\neg G_b) = \kappa(\neg G_b \mid \neg G_a)$ $(b \neq a)$, and indeed $\kappa(\neg G_b) = \kappa(\neg G_b \mid \neg G_{a_1} \wedge ... \wedge \neg G_{a_n})$ for any $n \in \mathbf{N}$ $(b \neq a_1,...,a_n)$. If, by contrast, you have the *shaky* attitude, your belief in further instantiations is destroyed by a negative instance, i.e., $\kappa(\neg G_b \mid \neg G_a) = 0$ and, a fortiori, $\kappa(\neg G_{\neq a} \mid \neg G_a) = 0$.[10]

The difference is, I find, characteristic of the distinction between lawlike and accidental generalizations. Let us look at two famous examples. First the coins:

(1) All German coins are round.

(2) All of the coins in my pocket today are made of silver.

It seems intuitively clear to me that we have the persistent attitude towards (1) and the shaky attitude towards (2). If we come across a cornered German coin, we wonder what might have happened to it, but our confidence that the next coin will be round again is not shattered. If, however, I find a copper coin in my pocket, my expectations concerning the further coins simply collapse; if (2) has proved wrong in one case, it may prove wrong in any case.

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[8] I am using here a technical notion of positive instance: a is a positive instance of G iff G_a, i.e. $Pa \rightarrow Qa$, is true. If $Pa \wedge Qa$, a positive instance in the intuitive sense, would be learnt, the beliefs would change, of course (at least given our assumptions that nothing except G is believed in κ).

[9] „Resilient" might be an appropriate term as well, but I do not want to speculate whether this would be a use of „resilient" similar or different to the one introduced by Skyrms; cf., e.g., Skyrms(1980).

[10] Here, $G_{\neq a}$ stands for $\bigwedge x(x{\neq}a \rightarrow G_x)$. Note that we have $\kappa(\neg G \mid \neg G_a) = 0$ according to both the persistent and the shaky attitude, simply because $\neg G_a$ logically implies $\neg G$.

Or look at the metal cubes, which are often thought to be the toughest example:

(3) All solid uranium cubes are smaller than one cubic mile.
(4) All solid gold cubes are smaller than one cubic mile.

What I said about (1) and (2) applies here as well, I find. If we bump into a gold cube this large, we are surprised – and start thinking there might well be further ones. If we stumble upon an uranium cube of this size, we are surprised again. But we find our reasons for thinking that such a cube cannot exist unafflicted and will instead start investigating this extraordinary case (if it obtains for long enough).

As far as I see, this difference applies as well to the other examples prominent in the literature (cf., e.g., the overview in Lange 2000, pp. 11f.). However, my wording is certainly more determined than my thinking. According to my survey, intuitions are often undecided. In particular, the attitude seems to depend on how one came to believe in the regularity; there may be different settings for one and the same generalization. However, at the moment I am concerned with carving out what appears to me to be the basic difference. Therefore I am painting black and white. As we shall see, ranking theory will also allow for a more refined account.

In any case, what the examples suggest is this: We treat a universal generalization *G* as lawlike if we have the persistent attitude towards it, and we treat it as accidental if we have the shaky attitude towards it. Hence, the difference does not lie in the propositional content, it lies only in our inductive attitude towards the generalization or, rather, its instantiations.[11]

Given how much we have learned from Popper about philosophy of science, this conclusion is really ironic, since it says in a way that it is the mark of laws that they are *not* falsifiable by negative instances; it is only the accidental generalizations that are so falsifiable. Of course, the idea that the belief in laws is not given up so easily is familiar at least since Kuhn's days (and even Popper insisted from the outset that falsifications of laws proceed by counter-laws rather than simply by counter-instances). But I cannot recall having seen the point being stripped down to its induction-theoretic bones.

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[11] In arriving at this conclusion, I am obviously catching up with Ramsey (1929) who states very early and very clearly: „Many sentences express cognitive attitudes without being propositions; and the difference between saying yes or no to them is not the difference between saying yes or no to a proposition" (pp. 135f.). „... laws are not either" [namely propositions] (p. 150). Rather: „The general belief consists in *(a)* A general enunciation, *(b)* A habit of singular belief" (p. 136).

What I have said so far may provoke a confusion that I should hurry up to clarify. The persistent attitude towards $G = \bigwedge x(Px \to Qx)$ is characterized, I said, by the *independence* of the instantiations; experience of one instance does not affect belief about the others. In this way, belief about an instance G_b, i.e., the positive relevance of Pb for Qb, is persistent. But didn't we learn that one mark of lawlikeness is *enumerative induction*, i.e., the confirmation of the law by positive instances? Surely, enumerative induction outright contradicts the independence I claim.

Herein lies a subtle confusion. Belief in a law is more than belief in a proposition, it is a certain doxastic attitude, and that attitude as such is characterized by the independence in question. If I would have just this attitude, just this belief in a law, my κ would exhibit this independence. Enumerative induction, by contrast, is not about what the belief in a law *is*, but about how we may acquire or confirm this belief. The two inductive attitudes involved may be easily confused, but the confusion cannot be identified as long as one thinks belief in a law is just belief in a proposition.

However, what could it mean at all to confirm a law if it does not mean to confirm a proposition? Indeed, my definition in section 2 applies only to the latter, and to talk of the confirmation of laws, i.e., of a second-order inductive attitude towards a first-order inductive attitude, is at best metaphorical so far; enumerative induction or falsificationism do not seem to make sense within this setting. In section 5 I shall make a proposal for translating and saving enumerative induction and the falsification of laws. But here and in the next section I am concerned only with the attitude in which the belief in a law itself consists.

Is my explanation of lawlikeness a deep one? No, it is just as plain as, for instance, that of the counterfactual theorist who says that lawlikeness *is* support of counterfactuals or that a law *is* a universally quantified subjunctive conditional. Analysis has to start somewhere, and it acquires depth only by showing how to explain other features of laws by the basic ones. That is a task that cannot be pursued here.[12] But I would like to insist that, as a starting point, the present analysis is to be preferred. There are good reasons for feeling uneasy about starting with subjunctives or a similarity relation between worlds. By contrast, ranking theory is a very plain theory with a very obvious interpretation.

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[12] But see my account of causal explanation in terms of ranking functions in Spohn (1991).

The only doubt one may have about my starting point may concern its sufficiency as a basis of analysis. In particular one may feel that the crucial property of laws is one which justifies the inductive attitude I have described, say, some kind of material or causal necessity. Maybe. But I am sceptical and refer to my second decision in section 1.

This does not mean that I have to sink into subjectivism, that I am bound to say that it is merely a matter of one's inductive taste what one takes to be a law. There may be objectivizations and rationalizations for our beliefs in laws. I do not intend to start speculating about this, but one very general rationalization is quite obvious. It is of vital importance to us to have persistent attitudes to a substantial extent. Something is almost always going wrong with our generalizations, and if we always had the shaky attitude, our inductions and expectations would break down dramatically and we could not go on living.

But of course, it is high time to admit that the distinction between the persistent and the shaky attitude is too coarse. It is not difficult, though, to gain a systematic overview within ranking theory. Let us see how many ways there are to believe the generalization G, i.e., for $\kappa(\neg G) > 0$. A natural and strongly simplifying assumption is

Symmetry: For all $a_1,...,a_n,b_1,...,b_n \in D$

$$\kappa(\neg G_{a_1} \wedge ... \wedge \neg G_{a_n}) = \kappa(\neg G_{b_1} \wedge ... \wedge \neg G_{b_n}).$$

In obvious analogy to inductive logic, symmetry says that the disbelief in violations of a generalization depends on their number, but not on the particular instances. For $n = 1$ symmetry entails that there is some $r > 0$ such that for all $a \in D$ $\kappa(\neg G_a) = \kappa(\neg G) = r$. More generally, symmetry entails, as is easy to see, that there is some function c from $\mathbf{N}$ to $\mathbf{N}$ such that for any n+1 different $a_1,...,a_n,b \in D$ the equality $\kappa(\neg G_b \mid \neg G_{a_1} \wedge ... \wedge \neg G_{a_n}) = c(n)$ holds, where $c(0) = r$. Indeed, all ranks of all Boolean combinations of the G_a are uniquely determined by the function c.

Another plausible assumption familiar from inductive logic is

Non-negative instantial relevance: For all $a_1,...,a_n,a_{n+1},b \in D$

$$\kappa(\neg G_b \mid \neg G_{a_1} \wedge ... \wedge \neg G_{a_n}) \geq \kappa(\neg G_b \mid \neg G_{a_1} \wedge ... \wedge \neg G_{a_n} \wedge \neg G_{a_{n+1}}).$$

This is tantamount to the function c being non-increasing.

Given the two assumptions there remain not so many ways to believe *G*; any non-increasing function *c* with $c(0) = r$ stands for one such way. Hence, the persistent attitude characterized by $c(n) = r$ for all *n* stands for one extreme, whereas the shaky attitude for which $c(n) = 0$ for $n \geq 1$ stands for the other. So, one may think about whether any ways in between fit the examples better than the extreme ones. Still, the consideration shows that the two attitudes I have discussed at length are suited best for marking the spectrum of possible attitudes.

4. Other Things Being Equal, Normal, or Absent

It is commonplace by now that laws or their applications are often to be qualified by some kind of *ceteris paribus* condition. As long as a law is conceived of as a proposition, the nature of this qualification is hard to understand. It seems to make the proposition indeterminate or trivial. But when we conceive of belief in a law as more than belief in a proposition, at least some mysteries dissolve in quite a natural way. Indeed, the account of laws given above almost yearns to be amended by such qualifications.

We should start, though, with the observation, often made in the literature, that we are dealing here with a mixed bag of qualifications. "*Ceteris paribus* condition" seems to have established itself as the general term, although it is clear to everyone that it really refers only to one kind of qualification. "*Ceteris paribus*" = "other things being equal" is obviously a relational condition. But what does it relate to? We shall return to this question. Another frequent qualification is that a law holds only in the absence of disturbing influences.[13] Still another way of hedging is to say that a law holds only under normal conditions.[14] A fourth kind are ideal conditions that are assumed by idealized laws though they are known not to obtain strictly. And there are other kinds, perhaps.

Yet another thing unclear is what exactly the qualifications are to act on. Some say it is the laws themselves that are hedged by the various conditions, while Earman and Roberts (1999) insist that the conditions exclusively pertain to the applications of laws to particular situations. Hence, provisoes in the sense of Hempel (1988, p. 151) which are "essential, but generally unstated, presuppositions of

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[13] Some call this a *ceteris absentibus* condition. My Latin expert informs me, though, that „*ceteris absentibus*" usually means only „other men (and not women or non-human things) being absent."

[14] My Latin expert also tells me that there is not really a good translation of „other things being normal" into Latin.

theoretical inferences" and hence part of the applications do not cover the phenomenon in full breadth, either.

This shows that the topic is not so uniform. Indeed, the inhomegeneity is common theme in this collection. Still, let us squarely approach the topic from the vantage point reached so far. This will illuminate at least normal conditions and the absence of disturbing factors.

We have arrived at the result that the belief in the generalization $G = \bigwedge x(Px \rightarrow Qx)$ as a law is represented by having $\kappa(\neg G_a) > 0$ for each a $\in D$ in a persistent way, i.e., unshattered by violations of the law. I have praised persistence as a virtue. But, to be honest, does it not appear just narrow-minded? Violations of a law are cause for worry, not for stubbornness. Sure, but the worry should concern the violation, not the future. Indeed, ranking functions provide ample space for such worries. There may yet be a ramified substructure of additional conditions. Let me explain:

Suppose $\kappa(Pa) = 0$ and $\kappa(\neg Qa \mid Pa) = r > 0$, that is, you do not exclude *Pa* and believe *Qa* given *Pa* according to κ. This allows for there being an exceptional condition *Ea* such that $\kappa(\neg Qa \mid Pa \wedge Ea) = 0$. This is due to the non-monotonicity of defeasible reasoning embodied in a ranking function. Of course, this entails via the ranking laws that $\kappa(Ea \mid Pa) \geq r$, i.e., that the exceptional condition *Ea* is at least as strongly disbelieved as the violation of the law itself.

This, I find, is quite an appropriate schematic description of what actually goes on. We encounter a violation of a law, we are surprised, we inquire more closely how this was possible, and we find that some unexpected condition is realized under which we did not assume the law to hold, anyway. In this way, hence, each ranking function representing the belief in the law *G* automatically carries an aura of *normal conditions* which is implicit at the level of belief, i.e., the function's core, and becomes explicit only if we look more deeply at the substructure below the core.

This substructure may indeed dispose to further changes of opinion. There may, e.g., be a further condition *E'a* such that the law *G* is reinstalled, i.e., $\kappa(\neg Qa \mid Pa \wedge Ea \wedge E'a) > 0$ for all $a \in D$. Defeasible reasoning may have arbitrarily many layers according to a ranking function.

Relative to a given κ embodying the belief in the law *G* we can even define the normal conditions hedging *G*. For, if *Ea* and *Fa* are exceptional conditions, $Ea \vee Fa$ is so as well. $\kappa(\neg Qa \mid Pa \wedge Ea) = \kappa(\neg Qa \mid Pa \wedge Fa) = 0$ is easily seen to imply $\kappa(\neg Qa \mid Pa \wedge (Ea \vee Fa)) = 0$. Hence, the disjunction E^* of all exceptional properties E for which $\kappa(\neg Qa \mid Pa \wedge Ea) = 0$ for all $a \in D$ (or for some $a \in D$, if symmetry is given) is the *weakest* exceptional property, and we may thus define the normal conditions N^* pertaining to G (relative to κ) as the complement or negation of E^*.

Note that N^* is not simply the disjunction N of all maximal properties M such that the law G holds given M, i.e., $\kappa(\neg Qa \mid Pa \wedge Ma) > 0$ for all $a \in D$. N^* is at least as strict as N and usually stricter. For instance, the condition $E \wedge E'$ under which the law G was assumed to be reinstalled two paragraphs above would be a specification of N, but not of N^*. The example also shows that normal conditions are more adequately explicated by N^*, because the condition $E \wedge E'$ should count as doubly exceptional and indeed counts as exceptional according to N^*, whereas it would count as normal according to N.

In any case, I find it entirely appropriate that normal conditions are thus explicated relative to a given doxastic state. Normalcy is something in the eye of the observer, in the first place, and therefore it is best described via its epistemic functioning. And ranking functions are particularly suited to grasp this.

However, this specifies only the statics of normal conditions. But we are rather interested in their dynamics, i.e., in the way in which our conception of them changes. After all, if we encounter a violation of a law, closer inspection of the case will often not confirm our previous understanding of exceptions, but will instead inform and revise it. This issue, however, belongs under the heading “confirmation of laws”, which I address only in the next section.

So much for the ramifications of the belief in a single law G. The next issue to face, hence, is: How to believe in several laws at once, in particular if they pertain to the same property? Let us look at the simplest example: Often we seem to believe in the law $G = \bigwedge x(Px \rightarrow Qx)$ *and* in a further law $G' = \bigwedge x(P'x \rightarrow \neg Qx)$ predicting *non-Q* for circumstances P'.[15] How can we do this?

This is the problem of the *superposition of laws* or, if the laws are causal, of the *interaction of causes*.[16] In mechanics the problem finds an elegant solution: the total force acting on a body is just the vector sum of the individual forces, each of which is governed by a specific force law. But in general there is no general solution. Only so much can be said:

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[15] The more familiar case will be that the laws do not predict that a quality Q is present or absent, but rather that a magnitude assumes different values in a given object. From a logical point of view this does not make much of a difference. Let us stick here to the simplest case.

[16] For the following discussion see in particular Cartwright (1983, ch. 2 and 3).

It is possible to believe both in G and G', though only if one also believes that $\neg\bigvee x(Px \wedge P'x)$. This is simply a matter of logic.

From the ranking perspective two remarks must be added. First, both laws can also be believed in the sense explained here, but only if the disbelief in each instance $Pa \wedge P'a$ is sufficiently strong. Second, and more importantly, even if a ranking function κ represents the belief in both G and G' as laws it still contains a prediction for the unexpected case that a instantiates both P and P'; $\beta(Qa \mid Pa \wedge P'a)$ must take some value. Hence, if two competing laws are believed in κ, they are automatically superposed in κ in some way (which may well be suspension of judgment, i.e., $\beta(Qa \mid Pa \wedge P'a) = 0$).

Even though this description is very unspecific (and is bound to be so), there is one point where it seems to be false. The description assumes that for each law it is exceptional in the above sense that the other law applies as well in a given case. But this is not how we normally look at the laws. We should be able to account for the superposition of G and G' even if $\kappa(Pa \wedge P'a) = 0$. This is why the present problem cannot be subsumed under the problem of normal conditions. But what else could be the account?

The only way seems to be to make the laws exclusive, i.e., to modify G into $\bigwedge x(Px \wedge \neg P'x \rightarrow Qx)$ and G' into $\bigwedge x(P'x \wedge \neg Px \rightarrow \neg Qx)$ and to modify κ correspondingly. The laws did not make any prediction for the case $\neg Pa \wedge \neg P'a$, anyway. What is left open, hence, is the case $Pa \wedge P'a$, for which one may, and has to, assume some degree of (dis-)belief in Qa. The resulting κ, according to which three laws, the modified G and G' and the new one, are believed, may also be called a superposition of the laws G and G'.[17] This consideration shows that the belief in a law as such, as I have described it, is implicitly understood in abstraction from other things, i.e., other relevant laws, and this abstraction is made explicit in the superposition in the second sense; i.e., in the modifications of G and G'.[18]

So, in which way do these remarks bear on the hedgings of laws familiar from the literature? Let me briefly summarize.

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[17] The superposition in the second sense could also be conceived of as the contraction of a superposition in the first sense by $\neg\bigvee x(Px \wedge P'x)$.

[18] An alternative way to remove the apparent conflict between G and G', which was envisaged by Cartwright (1983, pp. 57ff.), is to say that G and G' are not about the same Q. Rather, G is about Q-as-caused-by-P, and G' about Q-as-prevented-by-P'. In substance, though, the problem of superposition remains the same under this alternative.

The account of normal conditions I have given is exactly the one compellingly suggested by the literature on non-monotonic reasoning, default logic, or whatever the labels were, which has been richly produced since 1975. What I add is only the conviction that ranking theory, owing to its completeness concerning induction or belief revision, provides the optimal base for studying these phenomena.

The absence of disturbing influences or factors may stand for various things. It may simply mean the presence of normal conditions. Or it may mean that the case at hand is not governed by a further law which would require some guess or knowledge as to how the laws involved superimpose. To this extent, at least, this kind of hedge is covered by my remarks.

What about *ceteris paribus* clauses? As already mentioned, they require a standard of comparison which is usually left implicit. The default standard, I guess, is given by the normal conditions. In this case, other things being equal just means other things being normal. If, however, the standard of comparison is taken as variable, then the clause yields what Schurz (2002) calls comparative CP-laws, or it amounts to some such principle like "equal causes, equal effects" or "induction goes by suchnesses, not thisnesses" which might be explicated by symmetry principles like the one above. But I shall not pursue this issue.

Finally, I have not said anything about idealizations. This seems to be a somewhat different topic. But I should at least mention that it is accessible to the belief revision perspective as well, as has been shown by Rott (1991).

5. On the Confirmation of Laws

At several crucial points we missed an account of the confirmation of laws, and it was quite unclear how to give one, since the issue is not about the confirmation of propositions, which was already well handled by ranking functions. My paper would be badly incomplete without such an account.

But I have a proposal. Indeed, it will not be a surprise to anyone who is aware of the close similarity between probability and ranking theory, who has in particular noticed that a law according to my conception is analogous to a sequence of independent, identically distributed random variables, and who knows the work of de Finetti (1937). In his famous theorems de Finetti showed that there is a one-one-correspondence between symmetric probability measures for an infinite sequence of random variables and mixtures of Bernoulli measures according to which the variables are independent and identically distributed, and that the mixture concentrates more and more on a single Bernoulli measure as evidence accumulates. He thus showed to the objectivist that subjective symmetric measures provide everything he wants, i.e., beliefs about statistical hypotheses that converge toward the true one with increasing evidence.

The issue between objectivism and subjectivism is not my concern. Ranking functions are thoroughly epistemological and have as such no objective interpretation.[19] Still, we can immediately extract an account of the confirmation of laws from de Finetti's theory. Since this will look a bit artificial and formalistic, I shall demonstrate this with the basic construction and not discuss variants and ramifications.

Let us start with n mutually exclusive and jointly exhaustive properties or predicates $Q_1,...,Q_n$ (these are Carnap's Q-predicates). For each $i \le n$ we have the elementary law $G^i = \neg\bigvee x Q_i x = \bigwedge x \neg Q_i x$. For any proposition $A \subseteq W$ we may now count how often the law G^i is violated if A obtains; this is done by the function $v(A, i) = \text{card}\{a \in D \mid A \subseteq Q_i a\}$.[20] So, if we define the ranking function κ^i for W by $\kappa^i(A) = v(A, i)$, κ^i precisely represents the belief in the law G^i. Without any evidence, though, we do not believe in any law G^i. Our attitude towards the laws is rather represented by the ranking function ρ_0 for which $\rho_0(\kappa^i) = 0$ for each $i = 1,...,n$. Hence, our doxastic attitude towards the propositions $A \subseteq W$ is represented by the mixture κ_0 of the κ^i with respect to ρ_0, as defined by

$$\kappa_0(A) = \min_{i \le n} \kappa^i(A) + \rho_0(\kappa^i) = \min_{i \le n} v(A, i).$$

Now, how does this attitude change by experience? Via conditionalization, as always. But let us describe this in detail. Let $\mathbf{r} = \langle r_1,...,r_n \rangle$ stand for any sequence of n non-negative integers, and let $r = r_1 + ... + r_n$. Define next $E(\mathbf{r})$ to be the proposition (evidence) that among the first r objects precisely r_i instantiate Q_i ($i = 1,...,n$); the order of instantiation is irrelevant. Clearly, $\kappa_0(E(\mathbf{r})) = \min r_i$. Let B range over propositions about the remaining objects and not the first r ones, and let $\kappa_{\mathbf{r}}$ be the ranking function that we have for those propositions after receiving evidence $E(\mathbf{r})$. Then we have:

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[19] But see Spohn (1993).

[20] I am still jumping between sentences and the corresponding propositions as seems convenient to me.

$$\kappa_{\mathbf{r}}(B) = \kappa_0(B \mid E(\mathbf{r})) = \kappa_0(B \cap E(\mathbf{r})) - \kappa_0(E(\mathbf{r}))$$
$$= \min_{i \leq n}(v(B, i) + r_i) - \min_{i \leq n} r_i = \min_{i \leq n}(\kappa^i(B) + (r_i - \min_{i \leq n} r_i)).$$

That is, if we define $\rho_{\mathbf{r}}$ by $\rho_{\mathbf{r}}(\kappa^i) = r_i - \min_{i \leq n} r_i$, we have

$$\kappa_{\mathbf{r}}(B) = \min_{i \leq n}(\kappa^i(B) + \rho_{\mathbf{r}}(\kappa^i)).$$

Hence, $\kappa_{\mathbf{r}}$ is the mixture of the κ^i with respect to $\rho_{\mathbf{r}}$. So, the evidence $E(\mathbf{r})$ makes us change our attitude towards the laws from ρ_0 to $\rho_{\mathbf{r}}$, and $\rho_{\mathbf{r}}$ represents the degrees to which the various laws have been confirmed or rather disconfirmed. If $\rho_{\mathbf{r}}(\kappa^i) > 0$, we might say that κ^i is falsified, but note that falsification is never conclusive in this construction.

This account is essentially a translation of de Finetti's results into the framework of ranking functions. I find the translation basically plausible, and it strongly suggests following its course. One should characterize the class of ranking functions which represent mixtures of laws, and one should inquire the extent to which the representation is unique (for instance, there is an obvious one-one-correspondence between the $\kappa_{\mathbf{r}}$ and the $\rho_{\mathbf{r}}$ in the above mixtures). One should look at de Finetti's representation results for the infinite as well as for the finite case (recall the finiteness assumption made in this paper). The ranking analogue to de Finetti's notion of partial exchangeability would be particularly interesting. And so forth.[21]

On the other hand, the translation still looks artificial and quite detached from actual practice. For instance, if min r_i is large, one would tend to say that all of the laws G^i are disconfirmed by $E(\mathbf{r})$ and to conclude that none of the laws holds. One might account for this point by defining some κ^0 representing the belief in lawlessness, by mixing it into κ_0, say with the weight $\rho_0(\kappa^0) = s$, and by finding then that as soon as min $r_i > s$ we have $\rho_{\mathbf{r}}(\kappa^i) = 0$ only for $i = 0$. Moreover, one might wonder how precisely this story of mixtures carries over to the belief in a given law and its possible hedgings by various possible normal conditions, since one would like to be able to account for one hedging rather than another being confirmed by the evidence. And so on.

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[21] See, e.g., the rich results collected in the papers in Carnap, Jeffrey (1971) and Jeffrey (1980).

All this shows that there is a lot of work to do in order to extend the proposal and to apply it to more realistic cases. Still, the message should be clear already from the case I have explained in detail. The theory of mixtures provides a clear account of what it means to confirm and disconfirm not only propositions, but also inductive attitudes such as ranking functions representing belief in laws. Hence I was not speaking metaphorically when I talked about such confirmation earlier in the paper.

6. Some Comparative Remarks

The literature on *ceteris paribus* laws is rich and disharmonious, and so far I have only added to the polyphony. Since the idea of this ERKENNTNIS issue was to promote harmony (which does not require everybody to play the same melody), I should close with some comparative remarks.

So far, Schurz (1995) and Silverberg (1996) were the only ones to decidedly use the resources of non-monotonic reasoning for our topic (cf. also Schurz 2002, sect. 5). I emphatically continue on this line of thought, but we certainly have an argument about the most suitable account of non-monotonic reasoning.

What is novel to me is that the topic may also be approached from the learning-theoretic perspective. Indeed, I feel that Glymour (2002) and the present paper sandwich, as it were, the paper by Earman, Roberts, and Smith (2002), which is the focal challenge of this collection. How the two sides stick together is not clear. However, Kelly (1999) has established a general connection between formal learning theory and ranking theory, and the relation should become closer when one compares Kelly (2002) with the present section 5. So, let me briefly sketch my part of the pincer movement towards Earman et al. (2002), which will lead me across some other positions.

Clearly, my position is very close to that of Lange (2000), who says, for instance, that "the root commitment that we undertake when believing in a law involves the belief that a given inference rule possesses certain objective properties, such as reliability" (p. 189), and who reminds us on that occasion of the long tradition of the conception of laws as inference rules.[22] From a purely logical point of

[22] The insight that the issues concerning laws fundamentally rest on the theory of induction rather than the theory of counterfactuals is more salient in Lange (2000) than in Lange (2002). However, the theory of induction takes a probabilistic turn in Lange (2000, ch. 4), a move about which I have already expressed my reservations.

view, it was always difficult to see the difference between the truth of $\bigwedge x(Px \rightarrow Qx)$ and the validity of the rule "for any *a*, infer *Qa* from *Pa*". However, I find that the aspect of persistence, which was so crucial for me, is more salient in the talk of inference rules. Thus, what appeared to be merely a metaphorical difference turns out to have a precise induction-theoretic basis. It should have been clear, in any case, that ranking functions *are* (possibly very complex) inference rules, indeed, as my analysis of normal conditions has shown, defeasible inference rules that are believed to be reliable, but not necessarily universally valid. Hence, my account may perhaps be used to underpin Lange's much more elaborated theory, and conversely his many applications to scientific practice may confer liveliness and plausibility on my account.

To put the point differently, one might say that the emphasis in my account of laws is on the single case. The mark of laws is not their universality, which breaks down with one counter-instance, but rather their operation in each single case, which is not impaired by exceptions. Here, I clearly join Cartwright (1989) and her repeated efforts to explain that we have to attend to capacities and their cooperation taking effect in the single case. Her objective capacities or powers thus correspond to my subjective reasons as embodied in a ranking function, a correspondence which is salient again in the comparison of Cartwright (2002, sect. 2) with my sections 3 and 4. However, as I already said, I am content here with my subjective correlate and do not discuss its objectivization.

This is what separates me from Cartwright also according to the classification of Earman and Roberts (1999). They distinguish accounts that try to provide truth conditions for *ceteris paribus* laws from accounts that focus rather on their pragmatic, methodological, or epistemological role, and they place Cartwright in the first group, whereas my account clearly belongs to the second. Hence, I appear to be exempt from their criticism. However, though I agree with many of their descriptions, e.g., when they say that "a '*ceteris paribus* law' is an element of a 'work in progress'" (p. 466), I feel that pragmatics is treated by them, as by many others before them, as a kind of waste-basket category that consists of a morass of important phenomena defying clear theoretical description.

This feeling is reinforced by Earman, Roberts, and Smith (2002), who motivate their pragmatic or non-cognitivist turn in section 4 by their finding in section 3 that there is no solution to the "real trouble with CP-laws" that we have "no acceptable account of their semantics" and "no acceptable account of how they can be tested" (p. ##). In a way, the main purpose of this paper was to answer this challenge. To be sure, I did not provide a semantics in the sense of specifying truth conditions. But I gave an "epistemic semantics" in the sense of describing the doxastic role of unqualified as well as hedged laws, and I gave an account of how things having this role can be confirmed and disconfirmed. Of course, I did so on a fairly rudimentary formal level not immediately applicable to actual practice. But often, I find, the gist of the matter stands out more clearly when it is treated from a logical point of view.

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