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Introduction

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Four issues are central to this collection. First and foremost, there is the puzzling distinction between laws of nature and accidentally true generalizations. Second, there is the matter of how laws connect with the problem of induction. Third is the question of whether laws are sometimes contingent truths or whether they are always necessary truths. (This matter of the modality of laws plays a lesser role than do the other three.) Fourth, there is the topic of whether there really are any strict laws in either fundamental physics or the special sciences. Each of these four issues receives a preliminary presentation below. My hope is that this introduction frames the philosophical questions about laws in a way that helps to make clear the significance of the readings in this book.

1. Lawhood

Here are three reasons philosophers examine what it is to be a law of nature:

1. *Scientific Practice.* Science embraces and employs many principles that were at least once thought to be laws of nature. Newton's law of gravitation, his three laws of motion, and Mendel's laws are a few simple examples.
2. *Related Issues.* The account of counterfactuals championed by Roderick Chisholm (1946, 1955) and by Nelson Goodman (1947) and the deductive-nomological model of explanation advanced by Carl Hempel and Paul Oppenheim (1948) prompted much of the contemporary research on laws. Similarly, in his "The New Riddle of Induction" (1954), Goodman famously claimed that there is a connection between lawhood and induction. Thus, some philosophers following Goodman have an epistemological motivation for studying lawhood.

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3. *Philosophers love a good puzzle.* It may have been true that all the cigarettes in A. J. Ayer's cigarette case at some time *t* were made of Virginian tobacco (1956, 144), but this would not thereby have been a law of nature; even if true, this generalization would have been too accidental. Einstein's principle that no signals travel faster than light is also a true generalization but, in contrast, it is thought to be a law. Philosophers rightly wonder: What makes the difference?

What *does* make the difference? The generalization about the cigarettes is a restricted one in that it is about a specific time and a particular person's cigarette case; the principle of relativity is not similarly restricted. So, it is easy to think that, unlike laws, accidentally true generalizations are about specific times or objects. But that is not what makes the difference. Consider the generalization that all gold spheres are less than one mile in diameter. It is unrestricted and there are no mile-in-diameter gold spheres, but this is still not a law. The truly perplexing nature of the puzzle is clearly revealed when the gold-sphere generalization is paired with a similar generalization about uranium spheres:

All gold spheres are less than a mile in diameter.

All uranium spheres are less than a mile in diameter.

Though the former is not a law, the latter arguably is. The latter is not nearly so accidental as the first, since uranium's critical mass is such as to guarantee that enormous spheres of uranium will never exist (cf. van Fraassen 1989, 27). What makes this difference? What makes the former an accidental generalization and the latter a law?

One popular answer ties being a law to deductive systems. The idea dates back to John Stuart Mill (1947, first published 1843), but was defended by Frank Ramsey (1978, first published 1928) and made popular by David Lewis (1973, 1983, 1986, 1994) and John Earman (1984). A version of this idea is defended here in Barry Loewer's "Humean Supervenience" (Selection 10). According to Lewis (1973, 73), the laws of nature belong to all the true deductive systems with a *best* combination of simplicity and strength.[1] So, for example, as Loewer points out, the thought is that the uranium-spheres generalization is a law because it is part of the best deductive systems; quantum theory is an excellent theory of our universe and might be part of the best systems, and it is plausible to think that quantum theory plus truths describing the nature of uranium would entail that there are no mile-in-diameter uranium spheres. The thought is also that the gold spheres generalization is not a part of the best deductive systems. Of course, that generalization could be added as an axiom to any system, but not without sacrificing simplicity.

Other features of the systems approach are appealing. For one thing, it is reasonable to think that one goal of scientific theorizing is the formulation of true theories that are well balanced in terms of simplicity and strength. So, the systems approach seems to underwrite the truism that an aim of science is the discovery of laws. For another, it is appealing to many that the systems approach is in keeping with broadly Humean constraints on an account of lawhood in that there is no overt appeal to closely related modal concepts (for example, the counterfactual conditional) and no overt appeal to modality-supplying entities (for example, possible worlds or universals). Indeed, the systems account is crucial to Lewis's defense of the principle he calls *Humean supervenience,* "the doctrine that all there is to the world is a vast mosaic of local matters of particular fact, just one little thing and then another" (1986, ix).

In the late 1970s, there emerged competition for the systems approach and all other Humean attempts to say what it is to be a law. Led by David Armstrong (1978, 1983, 1991, 1993), and represented here by Fred Dretske's "Laws of Nature" (Selection 1) and Michael Tooley's "The Nature of Laws" (Selection 2), this rival approach appeals to universals to distinguish laws from nonlaws. Focusing on Armstrong's development of the view, here is one of his concise statements of the framework characteristic of the universals approach:

> Suppose it to be a law that Fs are Gs. F-ness and G-ness are taken to be universals. A certain relation, a relation of nonlogical or contingent necessitation, holds between F-ness and G-ness. This state of affairs may be symbolized as "N(F,G)." (1983, 85)

This framework promises to address our puzzle: Maybe the difference between the uranium-spheres generalization and the gold-spheres generalization is that being uranium does necessitate being less than one mile in diameter, but being gold does not. In sharp contrast to the systems approach, however, the universals approach is not a Humean solution to the problem of laws. Indeed, *non*supervenience is embraced by the universals approach.

The issue of supervenience is so divisive it will be worthwhile to consider two of the arguments that move philosophers to abandon Humeanism. Tooley (Selection 2) ingeniously asks us to suppose that there are ten different kinds of fundamental particles. So, there are fifty-five possible kinds of two-particle interactions. Suppose that fifty-four of these kinds have been studied and fifty-four laws have been discovered. The interaction of X and Y particles have not been studied because conditions are such that they never will interact. Nevertheless, it seems that it might be a law that, when X particles and Y particles interact, P occurs. Similarly, it might be a law that when X and Y particles interact, Q occurs. There seems to be nothing nonnomic about the particulars of this world that fixes which of these generalizations is a law.

Following Tooley's lead, though not pulled to the universals approach, my own nonsupervenience argument (1994, 60–68) begins with a possible world, U_1. Focusing on one specific moment t_0, let's suppose that there is a specific X particle, b, that is subject to a Y field. It has spin up; b's behavior is in no way exceptional. Whenever an X particle enters a Y field, it acquires spin up. Everyone would agree that L_1, the generalization that all X particles subject to a Y field have spin up, could be a law of U_1. U_2 is very similar to U_1. X particles that enter Y fields even do so at exactly the same time and place that they do in U_1. So, for instance, b enters that same Y field at time t_0. What is new about U_2 is that when b enters the Y field at time t_0 it does not acquire spin up. Of course, there must be at least one more difference between these two worlds. Though L_1 could be a law of U_1, L_1 could not be a law of U_2; L_1 is not true in U_2. Now there is nothing particularly remarkable about either U_1 or U_2—nothing to make a Humean suspicious. But here is the catch: It is natural to think that L_1's status as a law in U_1 does not depend on the fact that b entered that Y field at time t_0. That is, even if particle b had not been subject to a Y field at time t_0, L_1 would surely still be a law. Yet it is just as natural to think that L_1's status as a nonlaw in U_2 does not depend on the fact that b entered that Y field at time t_0. L_1 would not be a law in U_2 even if particle b had not been subject to that Y field. Just as you cannot *prevent* L_1 from being a law in U_1 by stopping b from entering that Y field, you cannot *make* L_1 a law by doing the same in U_2. So, it seems that there are two more possible worlds. In one, U_{1^*}, X particles are subject to a Y field, all of them have spin up, and L_1 is a law. In the other, U_{2^*}, X particles are subject to a Y field, all of them have spin up, but L_1 is not a law. Like Tooley's case, my U_{1^*} and U_{2^*} constitute an apparent counterexample to supervenience.

Though the universals approach and certain other views can take Tooley's and my examples at face value, Humeans must contend that these so called possible worlds are not both really possible. Challenges to nonsupervenience arguments from the Humean camp are raised by Helen Beebee in "The Nongoverning Conception of Laws of Nature" (Selection 12) and more briefly by Loewer (Selection 10). Beebee accuses Tooley and I of begging the question in virtue of assuming a *governing* conception of laws.

Two views put forward in the wake of the universals and systems approaches are the no-laws view and antireductionism:

1. *No Laws.* The majority of contemporary philosophers agree that there are laws. There are, however, philosophers who disagree. For example, at the end of his critical discussion of the universals approach in "Armstrong on Laws and Probability" (Selection 6), Bas van Fraassen anticipates the view he develops at length in his *Laws and Symmetries*—the view that there are no laws of nature. Van Fraassen finds support for this in the problems facing accounts of lawhood (for example, Lewis's and Armstrong's), and the perceived failure of philosophers to describe an adequate epistemology that permits rational belief in laws (van Fraassen 1989, 130, 180–81). Ronald Giere also finds support for this view in the origins of the use of the concept of law in the history of science (1995, 122–27), and in his belief that the generalizations often described as laws are not in fact true (127–30).

2. *Antireductionism.* Marc Lange (2000) and I (1994) advocate antireductionist views (also see Woodward 1992). Regarding the question, "What is it to be a law of nature?" we reject the answers given by Humeans and see no advantage in an appeal to universals. We reject all attempts to say what it is to be a law that do not appeal to nomic concepts. Yet we still believe that there are laws; we are not eliminativists à la Giere and van Fraassen. My focus is on arguing for the nonsupervenience of lawhood and highlighting the interconnections between lawhood and other related concepts. Lange's treatment includes an account of what it is to be a law in terms of a counterfactual notion of stability.

There are many, many other attempts to say (or—for principled reasons—not to say) what it is to be a law in the contemporary philosophical literature. Robert Pargetter's (1984) possible worlds account (also see Bigelow and Pargetter 1990, 214–62) and Simon Blackburn's (1984 and 1986) quasi-realism (also see Ward 2002) are just two more important examples.

That, anyway, is how the lines have been drawn about lawhood. What will be interesting is how matters will progress. How will philosophy move beyond the current divisions in logical space? Make no mistake, the divisions are serious ones: supervenience versus nonsupervenience, sparse ontology versus lush ontology, laws versus no laws, reducibility versus irreducibility. These are all major issues about which leading philosophers have made quite contrary judgments both about individual examples and about methodology. Do laws govern or just describe? Does scientific practice or common sense judgments provide the ultimate testing ground? How primary a role should epistemology or semantics play in the formulation of the correct metaphysics of lawhood? New work will have to address these debates head-on. The subsequent collision should generate new fruitful territory.

2. Induction

Goodman thought that the difference between laws of nature and accidental truths was linked inextricably with the problem of induction. In "The New Riddle of Induction," Goodman says,

> Only a statement that is *lawlike*—regardless of its truth or falsity or its scientific importance—is capable of receiving confirmation from an instance of it; accidental statements are not (1954, 74).

(Terminology: P is lawlike if and only if P is a law if true.) Goodman claims that, if a generalization is accidentally true (and so not lawlike), then it is not capable of receiving confirmation from one of its instances.

This has prompted much discussion, including some challenges. For example, Dretske (Selection 1) asks us to consider ten flips of a fair coin, and to suppose that the first nine land heads. The first nine instances—at least in a sense—confirm the generalization that all the flips will land heads; the probability of that generalization is raised from $(.5)^{10}$ up to .5. But this generalization is not lawlike; if true, it is not a law. So accidental statements are capable of receiving confirmation. It is standard to respond to such a challenge by arguing that this is not the pertinent sense of confirmation (that it is mere "content-cutting"), and by suggesting that what does require lawlikeness is confirmation of the generalization's unexamined instances. Notice that, in Dretske's coin case, the probability that the tenth flip will land heads does not change after the first nine flips. There are, however, other examples that generate a challenge for this idea, too. In "Confirmation and the Nomological" (Selection 4), Frank Jackson and Robert Pargetter ask us to consider a room with one hundred men. By directly questioning the first fifty you encounter, you find out that each one of the fifty is a third son. Intuitively, it seems that this would increase your reason to think that all the men in this room are third sons *and* that the next person you encounter will also be a third son. It does no good to revise Goodman's claim to say that no generalization *believed* to be accidental is capable of confirmation. About the third-son case, one would know that the generalization that all the men in this room are third sons, even if true, would not be a law. The discussion continues. Jackson and Pargetter have proposed an alternative connection between confirmation and laws on which certain counterfactual truths must hold. This suggestion is criticized by Elliott Sober in his "Confirmation and Law-likeness" (Selection 7).[2]

Sometimes the idea that laws have a special role to play in induction serves as starting point for a criticism of Humean analyses. Dretske (Selection 1) and Armstrong (1983, 52–59; 1991) adopt a model of inductive inference on which it involves an inference to the best explanation. This model is defended by John Foster in "Induction, Explanation, and Natural Necessity" (Selection 5). On its simplest construal, the model describes a pattern that begins with an observation of instances of a generalization, includes an inference to there being the corresponding law (this is the inference to the best explanation), and concludes with an inference to the generalization itself. The complaint lodged against Humeans is that, on their view of what laws are, laws are not suited to explain their instances and so cannot sustain the required inference to the best explanation. As Dretske and Armstrong see it, on their own views, laws are suited to play the required explanatory role, since on their views a law is not just a universal generalization, but is an entirely different creature—a relation holding between two universals.

Armstrong and Dretske make substantive claims on what can and cannot be instance confirmed: roughly, Humean laws cannot, laws-as-universals can. But, at the very least, these claims cannot be quite right. Humean laws cannot? As the discussion above illustrates, even generalizations known to be accidental can be confirmed by their instances. Dretske and Armstrong need a plausible and suitably strong premise connecting lawhood to confirmability and it is not clear that there is one to be had. Here is the basic problem: As Sober and others have pointed out, the confirmation of a hypothesis or its unexamined instances will always be sensitive to what background beliefs are in place. So much so, with background beliefs of the right sort, just about anything can be confirmed irrespective of its status as a law or whether it is lawlike. Thus, stating a plausible principle describing the connection between laws of nature and the problem of induction will be difficult. In order to uncover a nomological constraint on induction, something needs to be said about the role of background beliefs.

3. Necessity

Philosophers have generally held that some contingent truths are laws of nature. Furthermore, they have thought that if it is a law that all Fs are Gs, then there need not be any (metaphysically) necessary connection between F-ness and G-ness, that it is possible that something be F without being G. For example, any possible world that as a matter of law obeys the general principles of Newtonian physics is a world in which Newton's first is true and a world containing accelerating inertial bodies is a world in which Newton's first is false. The latter world is also a world where being inertial is instantiated but does not necessitate no acceleration. Some *necessitarians,* however, hold that all laws of nature are necessary truths (see Shoemaker 1980, 1998; Swoyer 1982; and Fales 1990). Other necessitarians have held something that is only slightly different. Maintaining that some laws are singular statements about universals, they allow that some laws are contingently true. So, on this view, an F-ness/G-ness law could be false if F-ness does not exist. Still, this difference really is minor. These authors think that, for there to be an F-ness/G-ness law, it must be necessarily true that all Fs are Gs. This view is advanced here in John Bigelow, Brian Ellis, and Caroline Lierse's "The World as One of a Kind: Natural Necessity and Laws of Nature" (Selection 8). In support of their view, necessitarians often argue that their position is a consequence of the correct theory of property individuation. Roughly, mass just would not be the property it is unless it had the causal powers it does, and hence obeyed the laws that it does. As they see it, it is also a virtue of their position that they can account for why laws are counterfactual supporting: They support counterfactuals in the same way that the truths of logic and mathematics do (Swoyer 1982, 209; Fales 1990, 85–87).[3]

4. Physics and the Special Sciences

Two separate (but related) questions that are not always clearly distinguished have received much recent attention in the philosophical literature surrounding laws of nature. Neither has much directly to do with what it is to be a law. Instead, they have to do with the nature of the generalizations scientists try to discover. First: Does *any* science try to discover exceptionless regularities in its attempt to discover laws of nature? Second: Even if one science—fundamental physics—does, do others?

Philosophers draw a distinction between what they call *strict generalizations* and *ceteris-paribus generalizations.* The contrast is supposed to be between universal generalizations of the sort discussed above (for example, that no signals travel faster than light) and seemingly less strict generalizations such as: other things being equal, all raptors have a hooked beak. The idea is that the former would be contradicted by a single counterinstance, say, one superluminal signal, though the latter is consistent with there being a deformed or injured raptor without any beak at all. Though in theory this distinction is easy enough to understand, in practice it is often difficult to distinguish strict from ceteris-paribus generalizations. This is because many philosophers think that many utterances which include no explicit ceteris-paribus clause implicitly do include such a clause.

For the most part, philosophers have thought that if scientists have discovered any exceptionless regularities that are laws of nature, they have done so at the level of fundamental physics. A few philosophers, however, are doubtful that there are exceptionless regularities at even this basic level. For example, in "Do the Laws of Physics State the Facts?" (Selection 3), Nancy Cartwright argues that the descriptive and the explanatory aspects of laws conflict. Consider Newton's gravitational principle, $F = Gmm'/r^2$. Properly understood, according to Cartwright, it says that for any two bodies the force between them is Gmm'/r^2. But if that is what the law says then the law is not an exceptionless regularity. This is because the force between two bodies is influenced by things other than just their mass and the distance between them (such as the charge of the two bodies as described by Coulomb's law). The statement of the gravitational principle can be amended to make it true, but that, according to Cartwright, at least on certain standard ways of doing so, would strip it of its explanatory power. For example, if the principle is taken to hold only that $F = Gmm'/r^2$ if there are no forces other than gravitational forces at work, then it would be true but would not apply except in idealized circumstances. In his "Natural Laws and the Problem of Provisos" (Selection 9), Marc Lange makes a similar point. Consider a standard expression of the law of thermal expansion: "Whenever the temperature of a metal bar of length L_0 changes by ΔT, the length of the bar changes by $\Delta L = kL_0 \Delta T$," where k is a constant, the thermal expansion coefficient of the metal. If this expression were used to express the strict generalization straightforwardly suggested by its grammar, then such an utterance would be false since the length of a bar does not change in the way described in cases where someone is hammering on its ends. It looks like the law will require provisos, but so many that the only apparent way of taking into consideration all the required provisos would be with something like a ceteris-paribus clause. Then the concern becomes that the statement would be empty. Because of the difficulty of stating plausible truth conditions for ceteris-paribus sentences, it is feared that, "Other things being equal, $\Delta L = kL_0 \Delta T$" could only mean "$\Delta L = kL_0 \Delta T$ provided that $\Delta L = kL_0 \Delta T$."

Even those who agree with the arguments of Lange and Cartwright sometimes disagree about what ultimately the arguments say about laws. Cartwright believes that the true laws of nature are not exceptionless regularities, but instead are statements that describe causal powers. So construed, they turn out to be both true and explanatory. Lange ends up holding that there are propositions properly adopted as laws though in doing so one need not also believe any exceptionless regularity; there need not be one. Ronald Giere (1995) can usefully and accurately be thought of as agreeing with Cartwright's and Lange's basic arguments but insisting that law statements do not have implicit provisos or implicit ceteris-paribus clauses. So, he concludes that there are no laws.

In "*Ceteris Paribus,* There Is No Problem of Provisos" (Selection 11), John Earman and John Roberts hold that there are exceptionless and lawful regularities. More precisely, they argue that scientists involved in fundamental physics do attempt to state strict generalizations and are such that these generalizations would be laws if they were true:

> Our claim is only that . . . typical theories from fundamental physics are such that *if* they were true, there would be precise proviso free laws. For example, Einstein's gravitational field law asserts—without equivocation, qualification, proviso, *ceteris paribus* clause—that the Ricci curvature tensor of spacetime is proportional to the total stress-energy tensor for matter energy; the relativistic version of Maxwell's laws of electromagnetism for charge free flat spacetime asserts—without qualification or proviso—that the curl of the **E** field is proportional to the partial time derivative, etc.

About Cartwright's example, they say that a plausible understanding of the gravitational principle is as describing only the *gravitational* force between the two massive bodies. (Cartwright argues that there is no such component force and so thinks such an interpretation would be false. Earman and Roberts disagree.) About Lange's example, they say the law should be understood as having the single proviso that there be no external stresses on the metal bar. In any case, much more would need to be said to establish that *all* the apparently strict and explanatory generalizations that have been or will be stated in physics have turned or will turn out to be false.[4]

Supposing that physicists do try to discover exceptionless regularities, and even supposing that our physicists will sometimes be successful, there is a further question of whether it is a goal of any science other than fundamental physics—any so-called special science—to discover exceptionless regularities and whether these scientists have any hope of succeeding.[5] Consider an economic law of supply and demand that says that, when demand increases and supply is held fixed, price increases. Notice that, in some places, the price of gasoline has sometimes remained the same despite an increase of demand and a fixed supply, because the price of gasoline was government regulated. It appears that the law has to be understood as having a ceteris-paribus clause in order for it to be true. This problem appears to be a very general one. As Jerry Fodor (1989, 78) has pointed out, in virtue of being stated in a vocabulary of a special science, it is very likely that there will be limiting conditions—especially underlying physical conditions—that will undermine any interesting strict generalization of the special sciences, conditions that themselves could not be described in the special-science vocabulary.

As I see it, progress on the problem of provisos requires three basic issues to be distinguished. First, there is the question of what it is to be a law, which in essence is the search for a necessarily true completion of: "P is a law if and only if. . . ." Obviously, to be a true completion, it must hold for all P, whether P is a strict generalization or a ceteris-paribus one. Second, there is also a need to determine the truth conditions of the generalization sentences used by scientists. Third, there is the a posteriori and scientific question of which generalizations expressed by the sentences used by the scientists are true. The second of these issues is where the action needs to be.

On this score, it is striking how little attention is given to the possible effects of context. Might it not be that, when the economist utters a certain strict generalization sentence in an "economic setting" (say, in an economics textbook or at an economics conference), context-sensitive considerations affecting its truth conditions will have it turn out that the utterance is true? This might be the case despite the fact that the same sentence uttered in a different context (say, in a discussion among fundamental physicists or better yet in a philosophical discussion of laws) would result in a clearly false utterance. These changing truth conditions might be the result of something as plain as a contextual shift in the domain of quantification or perhaps something less obvious. Whatever it is, the important point is that this shift could be a function of nothing more than the linguistic meaning of the sentence and familiar rules of interpretation.

Consider a situation where an engineering professor utters, "When a metal bar is heated the change in its length is proportional to the change in its temperature," and suppose a student offers, "Not when someone is hammering on both ends of the bar." Has the student shown that the teacher's utterance was false? Maybe not. Notice that the student comes off sounding at least a little bit insolent. In all likelihood, such an unusual situation as someone hammering on both ends of a heated bar would not have been in play when the professor said what he did. In fact, the reason the student comes off sounding somewhat insolent is because it seems that he should have known that his example was irrelevant. Notice that the professor's sentence need not include some implicit ceteris-paribus clause in order for his utterance to be true; in ordinary conversation, plain old strict generalization sentences are rarely used to cover the full range of actual cases.[6]

If special scientists do make true utterances of generalization sentences (sometimes ceteris-paribus generalizations sentences, sometimes not), apparently nothing stands in the way of them uttering true special-science lawhood sentences. The issue here has been the truth of special-science generalizations, not any other requirements of lawhood. Just like the generalization sentences, these lawhood sentences will be false in plenty of contexts, but that is inconsequential.

5. On What Is to Come

The readings contained in the rest of this collection are meant to reflect the contemporary history of the problem of laws, but are not intended to be a thorough survey of the literature on laws, not even the most recent (post–1976) literature. No doubt the choice of selections was dictated by some of my own views about what are the most interesting and important issues. My judgment was that this was the best way to put together a coherent package of readings. The essays have been ordered chronologically based on their first date of publication, and my hope is that this format will give the reader a feel for the recent historical development of the problem.[7]

Notes

1. Lewis later (1986, 1994) made significant revisions to his account in order to address problems involving physical probability.

2. Marc Lange (2000, 111–42) uses a different strategy. He tries to refine further the relevant notion of confirmation, characterizing what he takes to be an intuitive notion of inductive confirmation, and then contends that only generalizations that are not believed not to be lawlike can be (in his sense) inductively confirmed

3. Alan Sidelle challenges this argument in his "On the Metaphysical Contingency of Laws of Nature" (2002).

4. Volume 57 (2002) of *Erkenntnis* is devoted to the topic of ceteris paribus laws. It includes new papers by Cartwright and Lange.

5. Donald Davidson prompted much of the recent interest in special science laws in his "Mental Events" (1980, first published 1970). He gave an argument specifically directed against the possibility of strict psycho-physical laws. More importantly, he made the suggestion that the absence of such laws may be relevant to whether mental events ever cause physical events. This prompted a slew of papers dealing with the problem of reconciling the absence of strict special-science laws with the reality of mental causation (see Loewer and Lepore 1987, 1989; Fodor 1989, 1991; Schiffer 1991; Pietroski and Rey 1995).

6. Ceteris-paribus clauses may be overused in the special sciences and elsewhere. Sometimes speakers fail to appreciate the context sensitivity of the sentences they use. We all sometimes feel the need to hedge what we say in order to head off the nitpickers. My suggestion is that *sometimes* the nitpickers are out of line and are nothing more than context changers.

7. The introduction is based on the original version of my entry on laws of nature for *The Stanford Encyclopedia of Philosophy*. This entry will be regularly updated and so will provide a useful way to stay current with the literature.