The concept of physical law

Second edition

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For Sylvia, Diane, Efrem, Steve, Manjeet, Ruby, Paul, and Lorna (Theresa).

And in loving memory of my parents, Martin and Eva.

Contents

Preface to the first edition (1985)

I remember reading years ago about a young boy who complained about a book entitled *All about Whales*. He was aggrieved because the book did not deliver on what he took to be its promise, namely, relating everything there was to be said about whales. The child's parents had to explain to him that the title was ambiguous and should be read as meaning only that everything in the book was about whales.

Having taken the lesson to heart, let me warn that this is not a book that endeavors to say everything that can be said about physical laws. It aspires only to be a book in which everything said (this preface excepted of course) is about physical laws. And that is quite a difference.

Indeed, as you read this book you will doubtless be struck by how much I have chosen to omit altogether. In the following pages, you will find nothing, or nearly nothing, about the taxonomy of laws, for example, the distinctions between causal laws, laws of concomitance, laws of dynamics, and functional laws. Similarly, you will find nothing about empirical laws and theoretical laws; nothing about the difference between low-level and high-level laws; and nothing about basic laws and derived laws. You will find nothing about the difference between those laws whose nonlogical and nonmathematical terms refer only to observables and those laws that contain some descriptive terms that refer to unobservable (or theoretical) entities. And you will find nothing about whether time and space are discrete or continuous, and little about the analysis of "state of the world," "occasion," "natural kinds," and so forth. I have, in short, ignored – and, in some instances perhaps, run roughshod over – some traditional distinctions. I have done much of this knowingly and with design, for my purpose has been to "get back to basics," to try to analyze the *generic* concept of physical law, and in particular to examine the modal, as opposed to the epistemological, status of physical laws. As will become clear in due course, I think that elucidating this point has important consequences for how we view the world.

I would hardly be honest if I said that I have a great conviction that the thesis of this book is true. I have no such conviction. And indeed, the fact that I have disagreed so profoundly with so much of what is taken to be received wisdom about these matters, far from exhilarating me (as it might easily someone else of a different temperament), has caused me considerable worry. What prompts me to publish is the hope that I might not be wrong.

I have lived for many years with the problems I discuss in this book. I have, over those years, wrestled with and vacillated between two competing theories, Necessitarianism and Regularity. But, during the last few years, I have finally settled on the latter. In what follows, I try to defend Regularity against Necessity.

Jonathan Bennett, Raymond Bradley, and Terrance Tomkow were generous, unstinting really, in offering criticism of some early drafts of chapters. More recently, Philip Hanson and Hannah Gay read nearly completed versions of the manuscript and offered detailed comments. Their notes were enormously helpful and quite beyond my ability to repay. Two exceptionally helpful critics are unknown to me by name. , These latter philosophers, whom the editor, Richard Ziemacki of Cambridge University Press, enlisted to comment on the manuscript, provided a wealth of detailed criticism, challenges, encouragement, and suggestions. I have chosen to follow their advice in many places; but I have also been bold, or foolhardy, enough to decline some as well. Certainly, this book is much better for their careful thought. Steven Davis, E. Wyn Roberts, and David Zimmerman offered encouragement when my enthusiasm and stamina flagged.

The bulk of the manuscript was typed by Merrily Allanson, whose skills at her keyboard are to be compared to Franz Liszt's at his.

Cambridge University Press called on Alfred Imhoff to do the considerable job of copyediting this book. He managed to impose a uniform, integral style on an eclectic hodgepodge.

Sylvia, Diane, and Efrem graciously accorded me solitude when I became so immersed in these arcane studies that I scarcely could make conversation. Unlike other spouses and children who temporarily lose their mates and parents to the lure of golf or gambling, my wife and children had to endure my succumbing to the call of ratiocination. Their forbearance was inspiring.

Of the many books I consulted in the course of writing this essay, one proved invaluable: Tom Beauchamp's anthology, *Philosophical Problems of Causation* (1974). It was this collection of papers that provoked me to begin writing on this topic; and it was again this collection to which I most often turned in the course of working out my own thoughts.

Finally, a little piece of philosophy just for this preface.

One thing that surprised me as I reread what I had written in this book was how remarkably little I have said about the concept of causal laws and of causality in general. I did not set out consciously to avoid the latter subject; it is just that it did not naturally, of its own, come up very much. That, I think, is a pretty interesting philosophical discovery. If this book had not evolved in the way it did, I probably would not have believed that one could say as much as I have without also talking at length about causality. It seems to me, now, in judging this book, that the two notions – that of physical law and that of causal law (a specialization of the former) – can, profitably, be discussed apart. But, clearly, this claim is contentious, and I alert you to it so that you might judge for yourself.

Preface to the second edition (2003)

All known typographical errors in the first edition (Cambridge University Press), 1985, have been corrected. To improve readability, the body of the text has been reformatted. In particular, all quotations are now indented, and the size of the type has been set uniform (12 points) in the text, the quotations, and the footnotes. There are, in addition, other small changes throughout.

I would like to thank the university and college instructors who adopted the first edition for use in their philosophy courses. Thanks, too, to all those who have written to me with comments, questions, and suggestions.

By making this book available, both as a single file and as multiple files (chapter-by-chapter), I hope instructors who wish to use selected chapters as parts of their course materials will now find it easy to do so.

Since the original version of this book was written before I owned a word-processor, that edition did not exist as computer files. I have had to run every page in the book through an optical character reader (OCR) to produce this e-text version. Thanks to Scansoft Inc. for creating *TextBridge Pro*© software.

Most important, I must extend my especial thanks to Burke Brown, a singularly dear friend, who found that (some at least of) the theses of this book dovetailed with ideas of his that he has been developing for years. It was at his repeated urging that I have prepared this second edition.

PART I

Theory

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1

‘Near’ laws and ‘real’ laws

Physical laws[1] are propositions,[2] that is, they are the sorts of things that bear truth-values.

There is no such thing as a false physical law. In saying that physical laws are the sorts of things that are true or false, I mean that they form a subset of those things that are true or false or, more specifically, of those things that are *true.*

“Physical law” is a success term: If something we have taken (assumed, believed, etc.) to be a law is subsequently learned to be false, that proposition is not a false law, but no law at all. In this regard, that is, in implying the truth of its subject, “is a physical law” belongs to a class of predicates including such others as “is known” and “is logically necessary.” But to say this is

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1 I prefer the term “physical law” to either “law of nature” or “natural law” so as to avoid any seeming connection with the doctrine of “natural law” in ethics. (Here I follow the practice of Wollheim 1967.) Unfortunately, where the term “natural law” has unwanted connections with concepts of natural rights and the like, the term “physical law” has unwanted connections with theories of physicalism. So let me say explicitly that I will be using “physical law” in a broad sense. The term is to be understood to include those laws of living, as well as inanimate, matter; individual human, as well as social, behavior; and overt or public as well as private, human behavior. In using “physical law” to encompass all the latter, I am fairly sure that I am not begging any questions to be examined in this essay, nor do I think I am surreptitiously introducing materialism (although I am a materialist). Of the two terms, “physical” and “natural,” the former seems to have fewer problems associated with it. Neither term is wholly satisfactory; choosing between them is much a matter of taste.

2 Bradley and I have argued elsewhere (Bradley and Swartz 1979, pp. 65-86) that propositions are sui generis abstract entities, not to be identified with, e.g., indicative sentence-types or sentence-meanings. However, that particular theory of the ontology of propositions is not presupposed in this book. Here, my concern is with such matters as the modality of propositions that are physical laws. One need not have settled views, still less the same views as this author, about the ontology of propositions to pursue the kinds of issues that will be raised shortly.

not, however, remotely to suggest that physical laws need be known, still less that they are necessary.

Twenty-five years ago, Scriven began an essay by writing, “The most interesting fact about laws of nature is that they are virtually all known to be in error” (1961, p. 91). I still find his arguments persuasive, but with one important rider: By “laws of nature,” he does not refer to what I am here calling “physical laws”; that is, the difference between us is not just terminological. Scriven’s point is about the pronouncements of science, about what scientists call “laws”; what they invoke in their explanations of physical phenomena; what they advance in textbooks and teach to their students (see also Cartwright 1980a). In this essay, when I refer to “laws” I am talking about a certain class of truths about this world, a class wholly independent of whether or not anyone successfully discovers, formulates, announces, believes, or promotes those truths. If Boyle’s (so-called) Law is – as it certainly is – false, so be it. When I hereinafter talk about laws, I do not mean Boyle’s Law, or Bernoulli’s Theorem, or Fermat’s Principle, or Maxwell’s Rule, etc. I mean those (for the most part, unknown) true propositions that lie at the heart of the matter (no pun) and account for the verisimilitude of the pronouncements of science.

Scientific laws are conceptually distinct from physical laws. Only the barest handful of scientific laws are physical laws. I am unprepared to venture a guess as to how many these might actually be; I don’t know their absolute number, only that their relative number is very small.

The claim that few scientific laws are physical laws may at first appear implausible, for what then is science doing, if not producing physical laws?

> The scientific enterprise consists in observing, experimenting on, and of course hypothesizing about the world in order to explain, predict, control, and in a broad sense come to understand the world. The way these latter activities – explanation, prediction, control, and understanding – succeed is by bringing ever-greater parcels of the phenomena of the world under the umbrella of accepted scientific laws. Since science works, and works so exceptionally well, the laws it invokes that allow it to get on with its business must *be* the laws of the world.

This argument seems so self-evidently sound that few would question it. Nonetheless, there is good reason to challenge its conclusion.

Both Popper and Kuhn, for quite different reasons, have urged us not to believe that scientific laws are true. Kuhn has done so because he promotes the theory that there is no objective truth, and hence scientific laws are truth-valueless instruments. According to his account, scientific laws are not true, but this is because they are neither true nor false (1970, pp. 205-7). Popper, in contrast, allows that scientific laws do bear truth-values. However, in light of the frequency with which assumed laws of previous generations have been falsified, he cautions us not to believe that the current stock are true. But he does not counsel us to believe that scientific laws are false; instead, he recommends that we adopt an agnostic attitude, that we hold scientific laws only gingerly, tentatively, as conjectures and not as commitments. (1962a, pp. 50-2).

I agree with both Kuhn and Popper that we should not believe that scientific laws are true. But unlike Kuhn, I believe that scientific laws do bear truth-values; and unlike Popper, I think we ought to have beliefs about the truth-values of scientific laws. We ought to believe that virtually all of them are false.

In any event, the particular views of Popper and of Kuhn concerning the nature of scientific law have not dislodged the common view. The eighteenth-century conception of the nature of scientific law still prevails. This stock theory has it that science progresses, steadily most of the time and by a leap on occasion; but it moves nearly always forward, with the steady accumulation of more and more truths and with regular additions to the stockpile of known scientific laws.

The material progress of science cannot be doubted, even if its moral direction is a source of constant dispute. The information-explosion of the twentieth century and the dramatic increase in knowledge in the preceding three centuries both lend credence to the claim that the growth of scientific knowledge is exponential over time.

It is in explaining *how* this knowledge is possible that the common theory fails. The standard account is to the effect that scientific explanations are *valid inferences* from *true* statements of universal or statistical laws conjoined with appropriately chosen statements of relevant antecedent conditions.

This theory about the means by which science generates its multitude of successful explanations and predictions is untenable, and it should eventually atrophy like any other theory at such variance with empirical fact. To the extent that philosophers of science have believed, and contributed to, this common theory, the philosophy of science has perpetuated and promoted a mythic, normic model of explanation.

In the philosophy of science, as in every branch of philosophy, there are strong a prioristic tendencies.[3] These tendencies must be kept in check by empirical facts. Philosophy must beware not to promote models that depart substantially from the actual practice of scientists, whether these models concern, for example, the generation of hypotheses, the technique of explanation, or the dynamics of the acceptance or rejection of a theory. Normic models are relatively easy to construct: They do not have to pass the test of conformity to actual practice; but, by the same token, they may badly misrepresent actual practice and, indeed, even stultify that practice if an attempt is made to implement them.[4] The usual justification given for such philosophical theories, namely, that they endeavor to describe 'ideal' rather than actual practice, must not exempt them from critical probing. Quite the contrary. If a model is promoted as describing ideal practice, it must be subjected to heightened scrutiny because it would suggest the revising of actual, usually successful, practice.

One has only to begin actually to read scientific journals and to speak to scientists themselves to find that many of what casual observers of science take to be laws are little better than mere calculating algorithms. Many of these 'laws' are produced simply by curve fitting to empirical data and have no particular theoretical backing; that is, they are not derived from theory. Many others are 'derived' from theory, but not directly as we might suppose. Here, the paradigmatic models of derivation we have inherited from Hilbert's geometry and Peano's arithmetic do not aptly apply. To get from theories to working laws, scientists regularly, daily, advance simplifying assumptions.

Beginning students in science are from the outset introduced to simplifying assumptions in their homework assignments and laboratory experiments. They are told that they may assume

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3 There are many examples: Mill in his writing as if he had adduced recipes for a logic of discovery; Poincaré in his arguing that Euclidean geometry would always be conventionally adopted above all other competitors; and Carnap, at one period in his thinking, in his believing that Probability_1 (degree of confirmation) was a guide in life.

4 Bridgman's Operationalism is a case in point. In thrall to a bad philosophical theory, operationalist physicists and psychologists produced some of the most sterile research of the twentieth century. Fortunately, the episode was a temporary aberration (even if its effects have not quite disappeared). In 1953, 24 years after having written *The Logic of Modern Physics* (1929), Bridgman was to declare publicly (in an allusion that confused Mary Shelley's fictional doctor with that doctor's abominable monster): "I … have created a Frankenstein" (1961, p. 76).

that the expansion of the gas is adiabatic; that the rise in temperature is negligible; that the coefficient of friction is constant for the range of velocities; that the internal resistance of the power supply remains constant; that the light source is monochromatic; that the sample is uncontaminated; that the walls are perfectly reflecting; that the period of oscillation is a constant; that the heat sink is infinite in capacity; that the combustion chamber exhausts completely and that the outside air in the vicinity of the vent is at the same temperature as the air at remote distances, that is, that diffusion is complete and instantaneous; etc. This informal aspect of the training of scientists, although universal, is virtually invisible to the person who is not deliberately looking for it. There are no chapters in textbooks detailing what simplifying assumptions one may make, just as there are no chapters suggesting which areas of research are likely to prove rewarding. What it is reasonable to assume, what one may hypothesize it is permissible to ignore, and what mathematical techniques might be used to yield a solution are insights to be gained by practice, apprenticeship, and intuition.

In the scientific journals, the degree of sophistication increases and the vocabulary broadens, but the essential point remains the same: The scientific laws derived are not *deduced* from fundamental theories, but are arrived at through layers of simplifying assumptions and approximations.[5] We turn, for example, to but one recent issue of the *Physical Review A,* Volume 27, Number 1 (January 1983). In article after article, we find the authors laying out their assumptions and approximations Only physicists will be familiar with the specialized technical terms of the field, but the layperson can read these articles and attend profitably to the nontechnical terms, such as "approximation," "estimate," "corrections," "calculations of varying sophistication," "uncertain," and "spurious results."

- Assuming certain specified conditions hold, the new complementary functional will have a local maximum at each local minimum of the old energy functional and the value of the functionals will be identical at these extrema. (Berk 1983, p. 1)

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[5] Virtually nothing published in the professional journals in physics and chemistry concerns individual matters of fact. Physicists and chemists are intent to explain *classes* of facts, or *kinds* of events, not – as historians often are – singular events, e.g., why this particular photographic plate shows the peculiar tracks it does. The 'events' explained bear no calendrical dates or geographical coordinates. Laboratory results are credited only to the extent that they are thought to be representative of a kind of event or behavior. The aim in doing basic research, in making inferences from fundamental theory, and in publishing findings, is geared to the general case. Thus when various researchers, cited immediately below, write of electron scattering, resonance, capture, etc., they may be taken as advancing scientific *laws* of, respectively, electron scattering, resonance, capture, etc.

- In the Born-Oppenheimer approximation, the wave functions of molecules are expressed as the product of electronic and vibrational wave functions. This quasiseparability is conventionally attributed to the fact that the mass *m* of the electron is much less than the mass *M* of the nucleus with the result that the vibrational motion of the nuclei is adiabatic in comparison with the faster electronic motion. Such an explanation is not substantially supported by the actual size of the nonadiabatic coupling terms. ... there are other underlying dynamical factors for the validity of adiabatic approximations in molecular physics. (Lin 1983, p. 22)

- When relativistic and exchange corrections are omitted and the Born approximation is used for the scattering of an electron by an N-electron target system, the differential cross section can be defined in terms of the Compton profile. (Gasser and Tavard 1983, p. 117)

- There are, to our knowledge, no experimental determinations of the position and width of the lowest ^2P resonance in electron-beryllium scattering available at present. A number of theoretical calculations of varying sophistication have been performed on this resonance, which are largely in disagreement with one another. For this reason we have undertaken complex-basis-function calculations using configuration-interaction techniques to provide an accurate estimate of the resonance position and width. The lowest ^2P resonance state of Be^- can be thought of, to a first approximation, as a p-wave shape resonance ... (McNutt and McCurdy 1983, p. 132)

- Experiments in which fast negative muons are slowed and captured by atoms or molecules typically yield information on the capture times, final capture ratios for different atoms, and muonic x-ray cascades, but no detail on the slowing-down and capture processes. Nevertheless, knowledge of the energies of the muons just before capture and the characteristics of the capture orbitals is important for interpretation and must generally be supplied by theory. In view of this need, the theory of negative muon slowing down and capture is surprisingly uncertain even for hydrogen atoms. Estimates of the average muon energy for capture have varied from near thermal to several keV… Stopping powers and capture cross sections are required over a wide energy range and no single quantum-mechanical method is practical over the entire range. The Born approximation is valid at high energy and has been calculated. Some of the spurious results for muon capture are no doubt due to use of the Born approximation at low collision energies, as well as to use of inconsistent theories for the slowing down and capture processes. For low-to-intermediate energies there have also been several quantum-mechanical calculations, the results of which differ significantly. These calculations all make serious approximations, e.g., two states, straight-line trajectories, approximate wave-functions, etc., whose effects are difficult to evaluate. They also neglect inelastic scattering, which, according to the Born approximation, may contribute about 25% of the stopping power.

In the present work a quite different approach [the classical-trajectory Monte Carlo method] is taken… Except for classical mechanics and the use of a microcanonical ensemble for the ground-state hydrogen atom, there are no other approximations… The only additional source of error is statistical which can be reduced slowly by running more trajectories[6] (Cohen 1983, p. 167)

- This corrected $\Delta\nu$ must be treated with caution because the fitted line centers depend somewhat on the range over which the fit is made. It is to be hoped that the data of future measurements will be interpreted directly in terms of the more correct line shape discussed here. Finally, it is important to emphasize that a complete understanding of the positronium Zeeman resonance at the 1-ppm level needs a calculation of the α^2 corrections to the $\Delta\nu$ and at least a good estimate of the quadratic magnetic field contributions to the four-level effective Hamiltonian. (Mills 1983, p. 267)

The semiofficial 'standard' view of science[7] hardly reveals what we see above in the sampling of quotations. The route from basic theory, for example the capture of negative muons by hydrogen (see penultimate quotation above, Cohen 1983), is not simply a matter of *deducing* laws from basic theory, but of guessing, estimating, and selecting simplifying assumptions. When a physicist declares at the outset of his paper that he will assume that Newtonian mechanics applies, or that the trajectories are straight-line, or that temperature may be disregarded, etc., he rarely is using the term "assume" to mean "assume to be *true*." Quite the contrary, these particular assumptions are often made with the full knowledge that they are probably false, and more often with the knowledge that they are certainly false. The trouble is that Truth is often intractable and Deduction (of specialized laws from fundamental ones) beyond our powers of inference – beyond, that is, our own human mental powers and those of our computers.

It does not follow simply from the fact that there were false simplifying assumptions made in deriving them that most derived scientific laws are false. We know that it is logically possible for

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6 The Monte Carlo method is a technique for solving mathematical problems by averaging the results of trials using random numbers for the values of the variables. For a popular account and various illustrations of the method, see Millikan 1983.

7 See, e.g., Carnap 1966, ch. 25, "How New Empirical Laws Are Derived from Theoretical Laws."

a true proposition to be validly inferred from a set of propositions one or more of which is false. But although the falsity of the vast number of these derived laws is not guaranteed by the falsity of the simplifying assumptions used in inferring them, it is at least made probable by such assumptions. For the deriving of a false proposition from a set of propositions containing a false proposition is more probable than deriving a truth. Even so, the attribution of falsity does not rest solely on a priori probabilities. The falsity of the majority of derived laws is further attested to by the fact that they virtually all have limited application and, as a practical consequence of this, cluster into numerous sets each containing a few mutually inconsistent laws all treating of the same subject. Indeed, it is virtually de rigueur among physicists to begin their professional articles by reviewing others' work in their own field showing in what ways their commonly held stock-in-trade of specialized scientific laws is false.

Being false is hardly a sufficient condition for robbing a proposition of explanatory (or predictive, etc.) value. But being false is not a privileged state, either. Whether a proposition can function successfully in an explanation must depend on something other than its being true or false; probably on something akin to its being close-to-the-truth. However, closeness-to-the-truth cannot be exactly the special feature. For sometimes the truth is so complex that a proposition that approximated closely to it would be so unwieldy as to be useless.[8] The imperfectly understood property that confers suitability for use in explanation must be some complex property involving a weighted mixture of closeness-to-the-truth along with tractability and human comprehensibility.

Insofar as scientific laws are approximations or proxies, they must be approximations of, or proxies for, *something.* Of course, from a strictly logical point of view, scientific laws might be nothing more than approximations for still other approximations, and these in their turn, but approximations for still further approximations, and so on without end. In short, "*x*'s being an approximation" does not logically entail that there is some **true** proposition, *y*, whose approximation *x* is. Nonetheless, although the existence of a law of Nature as the endpoint of the series of approximations hardly logically follows from a scientific law's being an approximation,

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8 It follows from this, of course, that *truth* itself is not a sufficient condition for bestowing explanatory power on a lawlike proposition. For where truth exceeds human comprehension, it cannot be used to explain anything. Indeed truth – if complicated enough – can prove an obstacle to serviceability.

the existence of determinate laws of Nature is virtually axiomatic in the contemporary world view. If one is not going to allow that scientific laws are themselves physical laws, then there must be physical laws to which scientific laws approximate.

Is the World (i.e., Nature) governed by law? *If* it is – and this is a question to which I will devote considerable attention in Chapters l0, 11, and 12 – these 'governing' laws must be real laws, not scientists' workaday proxies or approximations. Lying behind the false, but consummately useful, laws of the laboratory and scientific journal are, we may suppose, the real laws of Nature itself, laws not subject to the vicissitudes of changing fashion, human idiosyncrasy, fortune, or genius; laws whose number does not increase with the growth of human knowledge; laws not subject to revision and that never suffer the indignity of being falsified. It is because of the existence of *these* laws that the world is the way it is. It is because of the existence of these laws – common wisdom has it – that science itself has an objective focus and a court of appeal beyond mere consensual favor among learned practitioners.

Nowhere is this belief in the existence of 'real' laws more strongly underlined than in the debates concerning miracles, free will, and determinism. In these debates when persons wonder, for example, whether there ever has been a supernatural intervention in the course of history, or whether the existence of physical laws is compatible with there being free will, or whether the future course of the world is causally predetermined, they clearly are taking "physical laws" in the fundamental, not in the scientific or epistemological, sense. If the existence of physical laws is seen to be a threat to the exercise of a free will, or if the existence of physical laws is thought to entail that the future of the world is necessitated by physical laws and antecedent conditions, or if miracles are regarded as the temporary suspension of the laws of Nature, then the physical laws so presupposed cannot be the fallible approximations and estimates of scientific journals. What threat there is, what necessitation there is, what temporary suspension there is, concern not the instrumental laws of science, but the real laws – known or unknown – that scientific laws imperfectly reproduce.

Each of these two kinds of law – scientific law (the surrogate laws of scientific practice), and physical law (the laws of Nature itself) – related though they are, poses its own unique problems for philosophical inquiry. I will endeavor to keep the two apart. I shall not here be especially concerned with the former class, with scientific law. Thus I shall not be examining such standard issues as inductive support, genesis of hypotheses, underdetermination among competing hypotheses, incommensurability of paradigms, theory-ladenness of observations, research programs, etc. What follows is not an essay in the philosophy of science insofar as the latter is usually regarded as a branch of epistemology. Instead, what follows is an examination of the concept of physical law.

Three main questions will interweave in this study. The answer I offer to each will have important implications for the answers to the others. This is to say that the issues are interconnected in important logical ways.

1. What are the truth-conditions for physical laws?

   What *‘facts’* or states-of-affairs ‘make’ a physical law true? Can an *un*instanced physical law be true? If its instances are not what ‘make’ it true, then what *are* its truth-conditions?

2. What is the modal status of physical laws?

   Physical laws are logically contingent, that is, each of them is true in the actual world and each of them is false in some other possible world. No physical law is true in every possible world. But are physical laws **merely** contingent, contingent – for example – in the way in which my liking the music of Charles Alkan is contingent? Or do physical laws have some special law-bestowing ‘natural necessity,’ intermediate between mere (or bare) contingency and logical necessity?

3. What is the number of physical laws?

   Are they finite and few, as many have supposed and speculated [9] or are they finite and many, or even, perhaps, infinite?

The answers to these questions are important, so much so that we must not rest content adopting standard accounts, however much those accounts may predominate and appear self-commending. What answers we give to these questions will determine much of how we view the world:

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[9] “Physics originally began as a descriptive macrophysics, containing an enormous number of empirical laws with no apparent connections. In the beginning of a science, scientists may be very proud to have discovered hundreds of laws. But, as the laws proliferate, they become unhappy with this state of affairs; they begin to search for underlying unifying principles.” (Carnap 1966, p. 244)

“In a significant sense, the ideal of science is a single set of principles, or perhaps a set of mathematical equations, from which all the vast processes and structure of nature could be deduced.” (Schlegel 1967, p. 18) whether events are necessitated; whether human beings can truly choose among alternative courses of action, or whether we are bound to do what we do in the way in which water will solidify when its temperature drops; and whether there is a limit in principle on human empirical knowledge.

A very great deal is known of this world. But what does this knowledge require us to think about the world's fundamental structure? How shall we conceive of the underlying reality of the world – its physical laws – in order that it should enjoy its epistemological character, its seeming toleration of free, deliberative, morally responsible actions, and its plenitude of variety and novelty? Questions such as these are transcendental questions, which is to say that what follows is an essay in – not the epistemology, but – the metaphysics of science.