CONCEPTS OF LAW OF NATURE

BY

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DISSERTATION

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ABSTRACT

Over the past 50 years, there has been a great deal of philosophical interest in laws of nature, perhaps because of the essential role that laws play in the formulation of, and proposed solutions to, a number of perennial philosophical problems. For example, many have thought that a satisfactory account of laws could be used to resolve thorny issues concerning *explanation, causation, free-will, probability,* and *counterfactual truth*. Moreover, interest in laws of nature is not constrained to metaphysics or philosophy of science; claims about laws play essential roles in areas as diverse as the philosophy of religion (e.g., in the argument from design) and the philosophy of mind (e.g., in the formulation of Davidson's anomalous monism).

In my dissertation, I consider and reject the widely-held thesis that the facts concerning laws can be reduced to the facts concerning the particular entities that the laws "govern," and that the laws thus have no independent existence. I instead defend a version of *nomic primitivism,* according to which the facts about laws cannot be reduced to facts that are themselves non-nomic – i.e., to facts that do not fundamentally involve laws, counterfactuals, causes, etc. Insofar as the truth or falsity of reductionism about laws has implications for many of the problems mentioned above, I think that this result should be of interest even to those who who do not work in metaphysics or the philosophy of science.

My methodology, which I lay out and defend in Chapter One, is a version of Carnapian explication. This method emphasizes the importance of articulating and maintaining clear distinctions between (1) the vague concept (or concepts) *law of nature* inherent in ordinary language and scientific practice and (2) the precise analyses of "law of nature" that philosophers have proposed as potential replacements for this concept. I argue that metaphysics-as-explication has clear advantages over rival conceptions of metaphysical methodology; in particular, it allows us to formulate evaluative criteria for metaphysical claims.

In Chapter Two, I offer an example of how careful attention to concepts already in

use can help resolve philosophical debate. Specifically, I argue that much recent literature has mistakenly assumed that there is only one concept of "law of nature" in use, while there are in fact at least two. *Strong laws* are the principles pursued by fundamental physics: they are true, objective, and bear distinctive relationships to counterfactuals and explanation. *Weak laws*, by contrast, lack at least one of these distinctive characteristics but play central roles in both the "special sciences" and in everyday life.

In Chapters Three and Four, I offer extended arguments against the two most prominent versions of reductionism about laws – *Humeanism* and *law necessitarianism*. According to philosophical Humeans, the laws of nature supervene upon the non-modal, non-nomic facts concerning the behavior of particular things at particular times and places. Law necessitarians, by contrast, argue that the laws are in fact *metaphysically necessary*, and that which laws there are is determined by a class of primitive, modally-loaded facts concerning the *essences*, *natures*, or *dispositions*. I argue that both of these views are mistaken insofar as they disagree with well-entrenched scientific practices and those in favor of reductionism have failed to provided sufficient reason for thinking that these practices should be revised. Much of my argument is focused on the role played by a number of supposed methodological principles, including appeals to *intuition*, *parsimony*, and *methodological naturalism.*

While the conclusions of this dissertation are explicitly constrained to laws, many of the arguments should be of interest to those who are concerned about philosophical methodology (especially in the role of intuition in philosophical argument) or the appropriate relation between metaphysics, science, and the philosophy of science.

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CHAPTER 1

INTRODUCTION AND METHODOLOGY

There has been a great deal of philosophical interest in laws of nature over the past three decades. At least some of this interest has sprung from the central role that laws of nature are perceived as playing in the formulation of (or proposed solutions to) certain traditional philosophical problems. For instance, many have thought that laws of nature are central in formulating the problem of free will and determinism, that they could be used to distinguish between mental events and physical events, or that they demarcate physically possible worlds from those that are not[1]. It might also seem plausible that, if one were to provide an account of laws of nature, one might then be able to provide an analysis of concepts such as *causation*, *explanation*, or *induction*. Even if one leaves aside these more traditional "philosophical" problems, laws of nature have long seemed central to understanding scientific practice. If, as is often thought, laws of nature are among those relationships that science aims to discover, a successful account of laws of nature might help clarify the nature of scientific inquiry.

[1]Some prominent examples include van Inwagen (1986), which makes repeated appeals to laws of nature in an argument for incompatibilism, and Beebee and Mele (2002), which appeals to a Humean analysis of law of nature to argue for compatibilism. Davidson (1970) appeals to laws of nature to formulate and argue for *anomalous monism*; his assumptions about laws have been critiqued by van Fraassen (1989, 23-25) and Lange (2000, 173-176). Goodman (1983) is probably the most well known attempt to spell out the relation between laws and induction. Lewis (1973; 1984; 1994) makes use of his analysis of laws to offer analyses of causation, chance, and counterfactual truth.

1.1 OVERVIEW OF THE DISSERTATION

In this dissertation, I will be examining some of the major debates that have sprung up about laws in recent years. In particular, I will be examining (1) what the relationship is between the laws of special sciences such as psychology, history, and biology to the laws of fundamental physics, (2) whether the laws of nature reduce to facts about particular entities, and (3) whether the laws of nature are metaphysically necessary. I will defend a version of *nomic primitivism*, which is the view that the facts about laws cannot be reduced to (and do not supervene upon) the facts about the entities that they "govern". In arguing for this thesis, I will also be defending a certain view of philosophical methodology that is inspired by Carnapian explication. I will argue that many of the philosophical debates concerning laws can be resolved by paying careful attention to the proper relationship between a philosophical analysis of law of nature and the concept(s) of law used in scientific reasoning.

In the remainder of this chapter, I will be doing a bit of ground clearing. I will begin by introducing some of the main areas of philosophical debate about laws, and will briefly note how I intend to address these debates in my dissertation. I will then offer an extended argument in favor of adopting Carnapian explication and, in particular, of carefully distinguishing between our ordinary concept of *law of nature* and the philosophical analyses of this concept that have been proposed. I will argue that doing so allows us to solve a problem shared by many philosophical analyses of law—i.e., the failure to make explicit the intended relation of the proposed "theory" or "analysis" to the concept *law of nature* used by practicing scientists and the scientifically educated folk. In particular, philosophers often fail to make clear whether their analyses should be interpreted as descriptions of an existing concept, as stipulative definitions of a new concept intended to play a certain role in theorizing, or as doing something between these two extremes. This ambiguity has made it difficult to determine how the sorts of evidence offered by philosophers (e.g., concerning intuitions about unrealized possibilities) bears on the success of a proposed analysis.

In order to avoid unnecessary complications, I will generally identify the laws of nature with certain propositions (or statements) and not with whatever it is that makes

these propositions true or false. This assumption, which is shared by many of the major philosophical accounts of laws, has the advantage of making discussions of the laws' necessity relatively straightforward, and I do not think that this assumption begs any substantive questions. It is, for example, compatible with the views that (1) there are no laws, (2) that each law corresponds to a strict regularity, or (3) no laws corresponds to a strict regularity. It is also compatible with the laws being false. Suppose, for instance, that the proposition *It is a law of nature that F=MA* is true. If laws are propositions, this entails that the proposition *F=MA* is a law. It does not entail that *F=MA* is true, however.

The assumption that laws are propositions (or perhaps statements or sentences) is widely shared. So, for example, the classic regularity view of laws claims that the laws of nature are simply certain types of true sentences or propositions (for example, propositions that do not involve particular objects). Similarly, both the law necessitarian position (that the laws are metaphysically necessary) and the Mill-Ramsey-Lewis "Best System" view (that the laws are the axioms of deductive systems) make sense only if the laws are either propositions, or something closely related to propositions. With this general caveat in mind, it will occasionally be useful to speak of "laws" as the types of things that make the propositions in question true. So, for example, it is common enough for philosophers to argue about whether or not the laws can truly be said to "govern" the universe. If the laws are propositions, this claim looks to be false (and perhaps trivially so). After all, propositions cannot govern anything; this must be left to the entities or relations that the propositions are (in some sense) *about*[2].

1.1.1 Two Concepts of *Law of Nature*

While there is widespread agreement over the importance of laws, there is little philosophical agreement on what, if anything, laws actually are. For example, there is considerable disagreement among philosophers concerning the particular roles that laws play in scientific explanation and on the characteristics that laws must have if they are

[2]In certain contexts, it will also prove important to distinguish laws of nature from both the concept *laws of nature* and the term 'laws of nature.' In cases where I wish to discuss the term or concept, I will use italics or single-quotes respectively.

to play these roles. In some contexts, such as that provided by classical mechanics, the obtaining of a law was supposed to entail the existence of a corresponding regularity and knowledge of the laws was supposed to play a central role in predicting and explaining the behavior of certain systems. So, for instance, in a Newtonian world, we can use the laws of Newtonian mechanics to calculate what the position and momentum of a particle will be at t_2 when we are given the relevant facts about its position and momentum at t_1, together with a specification of any forces that act on it between t_1 and t_2. Moreover, this same sort of "deduction from the laws" seems sufficient to explain *why* the particle has the position and momentum it does at t_2. This view of laws should be familiar to the scientifically literate "folk" and bona fide "scientific" examples of it can be found in disciplines such as cosmology.

This conception of law stands in marked contrast to the sorts of laws that are appealed to in the special sciences (i.e., sciences other than fundamental physics), where laws (1) need not entail exception-less regularities and (2) knowledge of these laws may be only marginally valuable for prediction or explanation. So, for instance, one could not use the so-called "laws of supply and demand" to predict what the value of a stock will be a year from now, even if one were provided with an exhaustive (and accurate) description of all the factors that such laws pick out as relevant. Similarly, while such laws undoubtedly play a role in the explanation of economic phenomena, the occurrence of the phenomena cannot be "deduced" from a statement of the relevant law. The same holds for the laws of sociology, of biology, of psychology, etc. At best, we might say that the laws of the special sciences have a certain "family resemblance" to the laws described in the previous paragraph. Most contemporary scientific claims about "laws" are probably best understood in terms of the weaker concept.

In chapter 2, I will argue that at least some of the seeming intransigence of the philosophical debates concerning laws springs from a failure to recognize that the phrase 'law of nature' is ambiguous. In particular, I will argue that there are at least two distinct concepts commonly expressed by the use of law terminology in scientific contexts. The first concept of law, which I will call *strong law*, picks out principles that entail the existence of strict regularities and bear distinctive relationships to both counterfactuals and explanation. The second concept is related to the weaker "laws" discussed in applied

physics, the special sciences, and everyday life. These types of laws need not entail the existence of strict regularities, nor need they have the strong relation to counterfactuals that characterizes the first variety of law; rather, such laws are characterized by a certain family resemblance to the first sort of law, and lack one or more of such laws' distinctive features. Such *weak laws* do not always yield true predictions (or, depending on the context, even approximately true ones). Similarly, such laws are often targets for explanation (e.g., in terms of strong laws), and may in some cases be of only marginal explanatory value themselves.

1.1.2 Reductionism and Non-Reductionism

A second area of debate concerns the possibility of reducing the facts about laws, causes, etc. (i.e., the *nomic facts*) to facts of some other type. So, for example, analyses in the *Humean* tradition typically claim that the nomic facts are either reducible to, or supervenient upon, the non-nomic facts. In support of this view, regularity theorists such as Hempel (1945) have identified laws as certain sorts of wide-ranging (but entirely non-nomic) universal generalizations, while thinkers such as Lewis (1973; 1983; 1994), Earman (1993), Loewer (1996), Earman and Roberts (1999; 2002; 2005a; 2005b), Roberts (1998; 1999; 2001) and Halpin (1999; 2003) have identified the laws with the axioms of a deductive system whose consequences best summarize the important non-nomic facts of our world. The core commitment of these views is that the laws of nature are merely concise descriptions of the non-nomic facts of the world. Insofar as the laws are entirely determined by what obtains at particular times and places, this excludes the possibility of laws "governing" the world in any significant sense.

Non-Humeans, by contrast, argue that the laws of nature have a sort of *necessity* or *invariance* that differentiates them from the non-nomic facts to which the Humeans purport to reduce them. Just what this necessity consists of has been a matter of debate: it has been argued to be a primitive relation between universals (Dretske, 1977; Tooley, 1977; Armstrong, 1983), a matter of our relation to other worlds (Pargetter, 1984), or a relation between the structure/nature of our world and the things in it (Vallentyne, 1988; Bigelow *et al.*, 1992; Bird, 2005b). Other non-Humeans have argued for

a non-reductive realism (Carroll, 1994; Lange, 2000; Woodward, 1992). On all of these accounts, it is possible for two worlds that agree on the non-nomic facts to disagree on facts about which laws hold[3].

One particularly clear example of the intransigence of the respective positions is provided by the on-going debates concerning the success of the purported counterexamples to Humean theories. In such examples, two worlds are described which apparently agree on all particular matters of fact but differ on the laws. For example, Tooley (1977, 669) proposes the following counterexample:

1. World A has 10 types of fundamental particles. There are 55 possible types of two-particle interactions.

2. 54 of these interaction types happen at some time in the world-history of A. Every type of interaction follows a set law, and this law is entirely independent of the laws pertaining to the interactions between other types of particles.

3. Particles X and Y have never and will never interact.

4. The non-nomic facts at World B are identical to the non-nomic facts at world A.

5. It is possible that in world A, X and Y would interact in a certain way while in world B, they would interact in another way[4].

[3]A few philosophers have argued that there are no laws and that we ought to stop talking as if there were such things. van Fraassen (1989), for example, is sympathetic to the general Humean world-view, but thinks the role played by laws can be fulfilled by considerations of *symmetry*. By contrast, Mumford's (2004) general metaphysical views are closer to those of the non-Humeans. However, he argues that there is no philosophical or scientific reason to posit the existence of laws, since appeals to *powers* are sufficient. As the contrast between van Fraassen and Mumford suggests, law-eliminativism does not presuppose any particular analysis of law. Rather, eliminativism about laws might be interpreted as the proposal that a new conceptual scheme be used in place of one that uses laws. The arguments for or against law-eliminativism hinge on the ability of the proposed conceptual schemes to do the work required of them. Van Fraassen, for instance, thinks that symmetry considerations can be used to do the work of laws, while Mumford thinks that powers can be used.

[4]Carroll's example is given in his (1994). While his argument is structurally similar to Tooley's, it has been modified in various ways to avoid Humean charges of question-begging. I will consider both Carroll's thought experiment and the Humean responses to it in chapter 3.

According to Tooley, thought experiments such as these show Humean reductionism is false, since (1) A and B have the same non-nomic facts but (2) it is possible for their laws to differ (i.e., it is possible that there are different laws governing the interaction of X and Y). The response from Humean reductionists to such thought experiments has typically been to deny that it is possible for worlds A and B to differ in such a way. The reason? The supposition that these two worlds are actually possible is incompatible with reductionism, and our commitment to such possibilities amounts to an unsupported assumption that reductionism is false (Roberts, 1998; Beebee, 2000; Earman & Roberts, 2005b). At first glance, it is hard to see what sorts of considerations could resolve this debate, since those proposing definitions of 'laws of nature' and 'physical possibility' do not agree on the general sort of thing that laws are, or on what general types of events and processes are physically possible.

While the debate between Humeans and non-Humeans has traditionally been the locus of reductionist/anti-reductionist debate, there are also substantive differences among the non-Humeans about the possibility of (other sorts of) reduction. For example, a number of writers have recently defended the thesis that the laws of nature are metaphysically necessary, and that facts about laws reduce to the essential natures of our world and the things in it. This thesis is usually called *law necessitarianism*, and Shoemaker (1980; 1998), Bird (2005a; 2007), and Ellis (2001) are among its most prominent contemporary defenders. While law necessitarianism is explicitly anti-Humean, it is nevertheless reductive insofar as it contends that the physical necessity of laws can be reduced to metaphysical necessity. Advocates of law necessitarianism claim that this thesis can explain the laws' relation to counterfactuals and explanation, and can account for certain aspects of our ordinary discussions of possibility.

In chapters 3 and 4, I will argue that both Humean reductionism and law necessitarianism are false. In both cases, I will argue that a careful attention to the way we actually apply our concept of *law of nature* (understood here as *strong law*) reveals that such laws cannot be reduced to other types of facts. Moreover, reductionists of both stripes have failed to provide convincing reasons that we ought to adopt some modified concept of law on which reductionism holds. In chapter 3, I will consider the role ontological parsimony, intellectual history, and intuition play in motivating Humean

reductionism, and will argue that we have little reason to suppose that such principles can be used to provide a cogent argument in favor of adopting a Humean conception of law. In chapter 4, I will consider the relation between intuition and metaphysical necessity in more depth and will argue that one can consistently recognize the cogency of Kripkean arguments for so-called "a posteriori necessities" without being committed to the thesis that the laws of nature are among such necessities. Moreover, I will suggest that there is considerable evidence against law necessitarianism.

1.2 PHILOSOPHY OF SCIENCE, METAPHYSICS, AND THE ROLE OF LANGUAGE

The philosophical debate about laws of nature is located within a cluster of debates that might be called the "metaphysics of science." This area of philosophy is concerned with spelling out the ontological commitments of various scientific theories and, in particular, the ontological commitments of fundamental physical theory. Other debates within this subfield include those concerning time, space, causation, explanation, and probability. For each debate, there are three broad positions that one can adopt. First, one might argue that the facts about the entities or properties in question can be *reduced* to facts about some other sorts of entities or properties to which the theory already commits us. Second, one might argue the entities or properties in question are *primitive* (i.e., irreducible) commitments of the relevant theory. Finally, one might argue that the theory only apparently commits us to the existence of such entities or properties, but that they can actually be *eliminated*.

One well-known debate within this field is the debate over the nature of space. Relationalists about space (such as Descartes and Leibniz) claim that the facts about space can be reduced to the facts about individual things, while substantivialists (such as Newton) argue that space is a primitive existent and cannot be so reduced. Later theorists, such as some of the logical positivists, claim that space is (at least in a certain sense) a matter of convention and may thus not really be a proper existent at all. Within these three broad positions, there are of course a wide number of sub-positions. Reductionists disagree on the nature of the reductive base; primitivists on the role that the primitive

thing plays; eliminativists on the precise form that a strictly accurate scientific language (e.g., one that does not mention the problematic term) might take. One can find structurally similar examples in a number of more contemporary debates: e.g., in the debates concerning process versus counterfactual theories of causation or the debates between frequentist and subjectivist theories of probability.

One might wonder whether the consideration of language has any role to play in the adjudication of such philosophical disputes. After all, it might be argued, this branch of metaphysics is not concerned about the way ordinary people (or even scientists) *talk* about laws, causation, or probability; rather, it is concerned with what laws, causation, and probability *really are*. This characterization of the subject area, however, presents a significant problem. Specifically, one is now faced with the problem of figuring out what the question "What are laws of nature, in reality?" could possibly mean, if 'law of nature' does not mean what it means in everyday discourse. Insofar as we deny that 'law of nature' means what is ordinarily meant, and provide no specification of which specific thing we are talking about, the question is incoherent. A philosopher is certainly free to specify that her use of 'law of nature' means something distinct from what is ordinarily meant; in this case, however, there is no *prima facie* reason to expect that her analysis of laws will be relevant to resolving the philosophical and scientific problems that make offering an analysis of laws worthwhile.

I think that a better way of conceiving of these philosophical problems involves paying more attention to the concepts of our everyday practice, insofar as these concepts provide metaphysicians with both (1) the starting points for their philosophical analyses and (2) the criteria for evaluating these analyses. In particular, I will argue that debates in the metaphysics of science are best interpreted via Carnap's methodology of *explication*, which makes explicit the distinction between philosophical theories and the ordinary-language or scientific concepts that must be used to evaluate these theories.

1.2.1 Carnap's Account of Explication

Carnap gives his most detailed account of explication in *Logical Foundations of Probability*. There, he describes the process as follows:

> The task of *explication* consists in transforming a given more or less inexact concept into an exact one or, rather, in replacing the first by the second. We call the given concept (or the term used for it) the *explicandum*, and the exact concept proposed to take the place of the first (or the term proposed for it) the *explicatum*. The explicandum may belong to everyday language or to a previous stage in the development of scientific language. The explicatum must be given by explicit rules for its use, for example, by a definition which incorporates it into a well-constructed system of scientific[,] logico-mathematical or empirical concepts. (Carnap, 1950, 3-4)

He proposes four criteria for a satisfactory explicatum (in order of importance): (1) similarity to the explicandum, (2) exactness, (3) fruitfulness, and (4) simplicity. I will say a little more about each of these requirements.

1. **Similarity to the explicandum** requires that, in general, an explicatum be defined so that it agrees with the explicandum on all (or nearly all) the most central (e.g., paradigmatic, prototypical, etc.) cases. So, to expand on one of Carnap's own examples, a good explicatum for *fish* (we'll call it *pisces*) ought to include the various species of sharks, salmon, etc. and ought not to include any rats or squirrels. The central importance of this requirement follows from the definition of explication, since our goal is to create a new concept that can take the place of the old concept in certain contexts. This does not require complete agreement between the two concepts, of course, since there would be little point in adopting an explicatum that was identical to the explicandum. An explicatum might diverge from the explicandum in cases where (1) the explicandum is vague or inconsistent or (2) a deviation is justified by appeal to one or more of the following criteria.

2. **Exactness** requires that the explicatum have clear rules governing both its application its relations with other concepts. These rules should, so far as it is possible, clearly stipulate to which cases the explicatum applies and to which it doesn't, even if the explicandum is vague in these cases. Insofar as an explicatum will often be more exact than the corresponding explicandum, we should not generally expect extensional equivalence between the two.

3. **Fruitfulness** requires that the explicatum be formulated in a manner that makes it practicable to formulate generally applicable principles or laws. So, to return to the above example, it might useful to exclude dolphins and whales from *pisces*, on the grounds that their inclusion would complicate the formulation and testing of the sorts of hypotheses that are likely to be of interest. For similar reasons, it is desirable to formulate explicata that have definite relations with concepts that are already well-understood; e.g., with concepts drawn from logic, mathematics, or fundamental physical science.

4. **Simplicity** requires that the explicatum be as simple as possible, insofar as this is possible (while still meeting the first three requirements).

While these rules do provide explicit criteria for judging the adequacy of a proposed philosophical analysis, they do not provide us with a mechanical procedure for deciding between rival analyses. For instance, an explicatum need not agree with the explicandum in all cases; in fact, the assumption of explication is that this is not always desirable, especially in cases where the explicandum is vague or inconsistent. So, the fact that an explicatum disagrees with the explicandum on (some) cases is not itself a counterexample to its adequacy, although this might provide reason to reject this explicatum in favor of an explicatum that is a better match. Moreover, this definition of explication is compatible with the adoption of multiple explicata for the same explicandum since the degree to which the various explicata meet the four requirements may well depend upon the domain to which they are applied. So, for example, it may be that our vague ordinary concept of *cause* should have several explicata, each of which is peculiar to a given domain (e.g., one for legal matters, another for physics). Of course, there are many good reasons for preferring a single explicatum: simplicity, the promise of greater fruitfulness, etc.

1.2.2 Descriptive and Revisionary Metaphysics

Explication, with its explicit distinction between philosophical theories and ordinary concepts, represents a substantive alternative to other, perhaps natural, ways of

conceiving of metaphysics. In order to bring out these differences, it will help to compare explication to two rival conceptions of metaphysics: (1) metaphysics involves a specification of the existents posited by some domain of discourse (of either scientific or ordinary language) and (2) metaphysics involves proposing a more useful or accurate specification of the existents in the world. These two standard ways of viewing metaphysics correspond (at least roughly) to Strawson's (1964, 9-12) famous distinction between *descriptive metaphysics* and *revisionary metaphysics*. After briefly laying out Strawson's descriptions of these "two ways" of doing metaphysics and presenting some problems with each, I will argue that explication avoids these difficulties.

Strawson claims there are two projects that fall under the heading of philosophical "metaphysics": *descriptive metaphysics* and *revisionary metaphysics*. While not mutually exclusive, these methods are presumably jointly exhaustive of metaphysics[5]. Descriptive metaphysics is "content to describe the actual structure of our thought about the world" while revisionary metaphysics is "concerned to produce a better structure" (1964, 9). While Strawson recognizes the potential value of both projects, he argues that descriptive metaphysics requires no justification beyond that of inquiry in general, while revisionary metaphysics is valuable only if it succeeds. Here, "success" seems to mean something like "produces a more accurate or more useful account of the world."

While few contemporary metaphysicians explicitly invoke Strawson's distinction, it seems to capture something essential about the philosophical debate concerning laws. For example, Strawsonian descriptive metaphysics plausibly encompasses much of the work done by those historians and philosophers of science who have been concerned to figure out the role played by discussion of "laws of nature" in certain historical eras or specific scientific subdisciplines. Strawson seems correct in his claim that such work has at least some interest even if the theories that are being described turn out to be (or are already known to be) false. It should not be surprising, for instance, that many contemporary thinkers are interested in the conceptions of laws put forward by Descartes or Newton, as these conceptions have played a significant role in the development of modern science. This sort of descriptively-oriented metaphysics is not limited to his-

[5]That is, any analysis of law of nature will fit into at least one of these categories. However, many projects contain some elements of each.

torians of science, however. Consider, for instance, some of the recent work that has been done on the relationship between models and laws (Cartwright, 1983; Cartwright, 1997; Cartwright, 1999; Giere, 1999) or on the relation between causation and conserved quantities (Salmon, 1998; Dowe, 1999; Dowe, 2004). These theses are defended primarily by appeal to (occasionally quite specific) aspects of contemporary scientific practice and they eschew the use of many prototypical philosophical methods, such as appeals to intuition or considerations of ontological parsimony. Similarly, they show little interest in producing accounts of laws or causes that will be useful in resolving the sorts of metaphysically weighty debates mentioned earlier. Rather, the primary goal is to produce accounts that cohere well with contemporary scientific practice[6].

On the other extreme, it might be appropriate to dub some of the discussions of laws in more traditional analytic metaphysics (e.g., Lewis (1973; 1984), Armstrong (1983), and Foster (1983)) as being revisionary. In labeling them as such, I do not mean that the conclusions of these projects are necessarily counterintuitive, or that those defending them are blind to the role that laws play in scientific practice. Rather, these accounts are distinguished by the way they attempt to fit laws of nature in a much more general metaphysical picture of the world and the things in it. These sorts of accounts are not as beholden as the first sort are to get the details right about contemporary (or historical) science; instead, there is a greater reliance on traditional philosophical methods—for example, appeals to intuitions or ontological parsimony. It is this same disconnect from the particulars of contemporary or historical scientific practice, however, that makes Strawson's criticism of revisionist metaphysics—that its conclusions are of interest only

[6]One place Strawson's distinction has been mentioned is with regards to the development of formal ontologies for various scientific subdisciplines. For a few examples of such ontologies, see the Foundation Model of Anatomy (Rosse & Jose L. V. Mejino, 2003; , n.d.) and the Gene Ontology (Ashburner *et al.*, 2000; Harris *et al.*, 2004). Something close to Strawsonian descriptive metaphysics is implicit in one of the standard definitions for the sorts of ontologies: "A specification of a representational vocabulary for a shared domain of discourse—definitions of classes, relations, functions, and other objects—is called an ontology" (Gruber, 1993). Moreover, the justification of such a view seems close to Strawson's: Ontologies matter because they accurately describe the concepts used in scientific practice. Smith (2003; 2004), among others, has argued against this descriptive interpretation of formal ontologies on the grounds that the concepts currently in use may not be (1) internally consistent, (2) coherent with the concepts used in other disciplines, or (3) amenable to the formulation of the types of broad generalizations useful in formal reasoning.

if they are improvements on the current way of doing things—so telling.

As the above discussion suggests, these distinctions are not airtight. It would, for example, be misleading to claim that so-called "descriptive" metaphysical projects are unconcerned with the truth about laws; after all, many of these projects are tightly grounded in contemporary physical theory, which represents our best current guess at the way the world is put together. Conversely, so-called "revisionist" projects need not disregard either scientific practice or our ordinary ways of speaking; instead, they are simply less beholden to take account of certain details. My point here, however, is a quite modest one: The outcome of a philosophical analysis of "law" will depend on what one takes the point of analysis to be. If one's aim is to produce an account of laws that captures all and only those principles that scientists actually term 'laws,' one's account may be quite deflationary, as there are a large variety of sociological factors that undoubtedly affect such linguistic practices. On the other hand, if one's target is a Humean-friendly concept that can explain its instances and ground inductive inference, one's conclusions about laws might be quite surprising to practicing scientists.

Most philosophical projects undoubtedly fall somewhere between purely descriptive and purely revisionary metaphysics. This, however, creates its own set of problems. For instance, it is tempting to think that a satisfactory account of laws must (1) be faithful to contemporary scientific practice (an explicitly descriptive goal) and (2) show how laws can be reduced to some preferred ontological base (which may well require revision of this practice). These criteria may (and indeed, almost always do) conflict, however, and the mere fact that we acknowledge both criteria as legitimate does not make the relative "success" of any analysis any easier to ascertain.

This sort of tension between descriptive accuracy and theoretical virtue is not constrained to the study of laws, and in fact features prominently in many of the central debates in contemporary philosophy of science. For instance, consider the problems faced by subjective Bayesians in their attempts to offer an analysis of the concept of *probability* relevant to evaluating the evidential relationships between hypotheses and evidence. Subjective Bayesians argue that the "probability that a hypothesis is true given some evidence" can be defined in terms of the *degrees-of-belief* that some individual does, would, or should have in that hypothesis, given that evidence. The problems begin to arise

when one tries to figure out how one should characterize the notion of *degrees-of-belief* used in this definition. Insofar as the analysis aims to be reductive, degree-of-belief must mean something akin to *actual degree-of-belief*. However, actual degrees-of-belief don't obey the probability calculus, so an analysis in these terms would fail to fit the ordinary concept of probability. By contrast, rational degrees-of-belief might obey the probability calculus, but have only a tangential connection to any actually existing psychological states. Moreover, it is unclear whether they can be understood distinctly from "probable given the evidence"[7]. If the account of explication proposed above is correct, this apparent difficulty can be traced to a failure to appropriately characterize the goals of a philosophical analysis of probability. Defining probability in terms of the betting odds of a perfectly rational person might be descriptively adequate, but this tells us little of value about the concept of *probability* (or about *rational degree of belief*). Conversely, defining *probability* in terms of actual degrees-of-belief meets the (revisionary) goal of reduction to a preferred base, but sacrifices similarity to the ordinary concept.

1.2.3 The Advantages of Explication

While Strawson recognizes that there are substantial problems with interpreting certain metaphysical claims as being either purely descriptive or purely revisionary, he does not propose any evaluative criteria for such mixed claims. Carnapian explication, I claim, provides a coherent view of meta-ontology which allows for the acknowledgment of both (1) the importance that ordinary language and scientific practice play in evaluating metaphysical claims and (2) our desire (at least in certain contexts) to improve practice by introducing concepts that are more precise, simpler, and that allow for fruitful connections to be drawn with concepts drawn from other domains. In particular, I will argue that explication does a better job of this than criteria that are exclusively descriptive or exclusively revisionary.

The evaluative criteria appropriate to a purely descriptive metaphysical project can be stated quite simply: the philosophical analysis of a concept ought to be as similar to the concept actually used as possible. The problem is that such criteria seem to be of

[7]For a discussion of the problems in formulating a coherent version of subjective Bayesianism, see (Maher, 2006).

very limited applicability. For example, consider the vagueness inherent in our ordinary concepts of *cause*, *force*, or *gene*. Most philosophers would recognize these concepts as legitimate targets of analysis and numerous accounts have in fact been offered. Insofar as philosophers offer analyses according to which these concepts end up being precise, however, they do not seem to be engaged in purely descriptive metaphysics, and it would thus be inappropriate to judge them on purely descriptive criteria. Another reason for doubting the adequacy of purely descriptive criteria is that our concepts may lack the well-defined relationships with other concepts of philosophical interest (especially those from different disciplines) that are usually thought to distinguish successful metaphysical projects. This suggests that (contra Strawson) there is no guarantee that the fruits of a successful descriptive metaphysical project will be all that desirable. For example, the philosophical interest in laws is due in large part to a desire to clarify the role laws play in scientific explanation, counterfactual evaluation, and attributions of physical necessity. A purely descriptive account (even if accurate) may fail to clarify any interesting aspect of these concepts, especially if it focuses on capturing every use of the phrase 'law of nature,' at the expense of articulating an interesting concept.

The problems with formulating purely revisionary criteria are even worse. So, for instance, there is no general expectation that revisionary accounts of laws of nature will perfectly capture (or even generally approximate) the manner in which we apply our ordinary concept of *law of nature*. We are instead free to introduce concepts that are precise, simple, consistent, etc. even if their scientific analogues are not. However, once we give up fidelity to practice, we also lose any clear criteria for determining whether our analysis is successful. In particular, it is not at all obvious that the results of a revisionary project will be relevant to solving the problems that motivated the philosophical analysis in the first place. In the case of laws, for instance, much of the philosophical interest is based on roles that "laws of nature" are perceived as playing in explanation, in the possibility of free will, or in the nature of physical necessity. In each of these cases, our questions are questions about the concepts already in use; these questions will not be resolved by the specification of an entirely new concept. It is possible, of course, that these questions arise from an inconsistency in our current conceptual scheme. In this case, "revisionary metaphysics," in the form of the specification of novel concepts,

may be appropriate. However, insofar as such a proposal would involve wholesale revisions of scientific practice, one might well doubt whether this is an appropriate task for metaphysicians.

The criteria proposed by Carnapian explication, in contrast to purely descriptive or revisionary criteria, can be unproblematically applied to the results of paradigmatic philosophical accounts of laws. These criteria hold that accounts are judged on their capacity to provide precise, fruitful concepts that can be used in replacement of the vague explicandum for at least some purposes. Carnap's four criteria for the evaluation of explicata follow from this goal.

The fact that explication provides clear criteria for the evaluation of philosophical analyses of laws of nature matters only if these criteria allow us to accurately distinguish between successful and unsuccessful philosophical analyses. I think, however, that there are at least two reasons to think that these explication-based criteria do capture what is essential to the evaluation of philosophical claims. First, at least some scientific or philosophical problems arise from the fact that our ordinary concepts are vague and perhaps (in some cases) applied inconsistently. Explication, insofar as it provides us with precise replacements for these concepts, may serve to "dissolve" these problems[8]. Second, the formulation of a precise explicatum might in some cases help us to better understand certain features of the explicandum. For example, at least some of the interest in providing philosophical analyses of laws stems from the hope that a successful analysis will guide our judgments concerning laws. In many cases, we simply do not know whether our ordinary concept *law of nature* applies to a given case or not. However, if we craft a precise explicatum that agrees with the explicandum about clear cases (i.e. cases where it is clear whether or not a given principle is a law), we have some reason to trust the explicatum's verdict regarding the problem case as well[9].

[8]One example might be Einstein's explication of "simultaneity" in his formulation of special relativity. Here, the explicandum might be taken as the concept of simultaneity of either (1) ordinary language or (2) classical physics.

[9]Maher (2007) defends explication on these grounds.

1.2.4 Explication Versus Clarifying the Explicandum

If the above argument is correct, one of the primary tasks of metaphysics of science is the construction of adequate explicata for concepts of everyday or scientific interest. These explicata can then be used in place of the corresponding explicanda in specified contexts; however, they remain conceptually distinct from the explicanda and should not be conflated with them. Not every philosophical analysis of a concept aims at the construction of formal explicata, however. Instead, many philosophical arguments can be read as claims about the concepts used in ordinary language or in scientific practice. For example, much of the philosophical work on laws of nature is plausibly interpreted as attempts to *describe* such things as: (1) what types of relations are correctly called 'laws' (according to contemporary physics) (2) whether such laws must remain true under certain counterfactuals, or (3) whether it is possible for there to be laws of sciences other than fundamental physics. Insofar as such discussions are limited to describing the concept of law that is actually in use, these arguments cannot reasonably be taken as advocating the adoption of a certain explicatum. Instead, these discussions are best interpreted as clarifications of the explicandum. That is, they aim to *specify* a concept already in use and argue that it plays an important role in scientific practice. As was the case with Strawson's "descriptive metaphysics," there are good reasons for thinking such clarification cannot represent the only (or even the primary) goal of metaphysics of science.

The important thing for our purposes is to note the clear differences in the evaluative criteria appropriate to (1) discussions of the explicandum and (2) proposals for the adoption of a new explicatum. The former can be criticized only insofar as they are disloyal to some aspect of ordinary usage, either by claiming that certain principles are laws that are not laws or that some principle is not a law that actually is. It would not be appropriate to criticize such accounts for failures of vagueness, simplicity, or ontological parsimony. If these criteria are appropriate, it is only in the context of the latter category, where a new concept is being proposed. Here, the evaluative criteria laid out by Carnap are the appropriate ones. This is a point to which I will return repeatedly over the remainder of the dissertation. Consider the thesis that laws can be reduced

to non-nomic facts, for instance. I will argue that it is not plausible to claim that our current concept of *law of nature* picks out something that is reducible to the so-called "Humean base." So, such accounts fail as attempts to clarify the explicandum. However, it is not clear that they can appropriately be evaluated by any other criteria, since those defending the accounts do not offer specific examples of contexts in which it would be advantageous to adopt a revised concept of law.

1.3 CONCLUSION

Most philosophical accounts of law do not recognize the distinction between clarification of the explicandum and explication, and this shows up in confusion concerning the appropriateness of various evaluative criteria. For example, consider Lewis's oft-quoted contention that a policy of "spoils-to-the-victor" [10] provides adequate defense of some of his own proposals' failures to line up with widespread intuitions about objective chance, causation, and laws. The appeal to such a methodological principle only makes sense if one sees oneself as proposing an explicatum for "laws"; in such a case, one cannot be claiming to be providing a surprising (but accurate) account of the concept of law that is already in use. If intended as an explicatum, the success of the view hinges on the ability to provide a precise, fruitful, simple concept that can be used in place of our current concept in specific contexts. Moreover, if an analysis of law is intended as an explicatum, then we ought to expect it to render verdicts on cases where the ordinary concept of law is silent, and to have specifiable advantages (in determinate contexts) over the concept already in use.

In the next three chapters, I will turn to specific problems that arise in offering philosophical analyses of laws of nature. For the most part, I will leave aside explicit consideration of explicatum and explicandum. The take-away point here remains, however: philosophical accounts of laws of nature are beholden to take account of the role that laws play in our actual practice. If the accounts are intended to be mere clarifications of the explicandum, then we should require that they capture ordinary usage. If,

[10] For example, see Lewis (1994, 478). Armstrong says similar things about the precise range of what a philosophical theory ought to admit regard as "possible" (1989, 64).

by contrast, they are intended as explicata, it needs to be shown that they have definite advantages over the explicandum in the relevant contexts. The burdens of specifying these contexts, and of showing that a given explicatum does the work required of it, fall squarely on the shoulders of the metaphysician who proposes it.

CHAPTER 2

TWO CONCEPTS OF LAW

In the previous chapter, I argued that claims about laws of nature ought to be evaluated as either (1) clarifications of concepts already in use (i.e., as clarifications of *explicanda*) or (2) proposals for the adoption of new concepts that can be used in place of such concepts in certain contexts (i.e., as proposals for the adoption of new *explicata*). In either case, I suggested that the evaluation of such claims requires that we be able to articulate *which* concepts we are concerned with and that we have some grasp of what role these concepts play in scientific practice. In order to evaluate philosophical theories of laws of nature, then, it will be worth taking some time to examine the roles that the concept(s) of law play in scientific and philosophical discourse.

While it is probably impossible to specify the necessary and sufficient conditions for lawhood, one can make it relatively clear which concept of law we are interested in analyzing. For instance, we can provide examples of what we take to be paradigm cases of laws of nature, articulate provisional necessary conditions, and so on. In this chapter, I will do this, and will conclude that there is not a single, unified concept *law of nature*. Rather, there are (at least) two concepts of law of nature relevant to understanding scientific and philosophical discourse and these two concepts raise interestingly different philosophical problems. For the sake of clarity, I will call these concepts *strong law* and *weak law*. In what follows, I will describe these concepts in a little more detail and argue that they are, in fact, distinct concepts. While I think that these results are of independent interest, they also have a direct bearing on some of the more traditionally "philosophical" debates considered in later chapters. Specifically, the results of this chapter suggest that philosophical attempts to identify the laws of the special sciences with laws of physics (e.g., by hypothesizing the existence of hidden ceteris paribus clauses) are misguided. For similar reasons, this conclusion also suggests that it would be inap-

propriate to demand that a particular philosophical analysis of laws of nature capture every use of the phrase 'law of nature.'

2.1 ARGUING ABOUT CONCEPTS

It is worth emphasizing that the methodology described in the previous chapter does not presume that philosophical claims about laws of nature should be interpreted as claims about the concept *law of nature*. Instead, it assumes that conceptual analysis plays a vital role in providing the context for the evaluation of philosophical claims. After all, we can only in rare occasions compare (even indirectly) the results of a given philosophical analysis of laws of nature against the "real thing." Instead, we must judge philosophical theories by considering carefully both the role that concepts of law play in scientific reasoning and the ways in which these reasoning processes would be affected if we adopted the concepts resulting from the proposed philosophical analysis. This sort of conceptual focus does not preclude us from taking into account the truth of either pre-philosophical or philosophical claims about laws of nature. For example, suppose that our pre-philosophical concept *law of nature* is inapplicable to the real world (e.g., because its appropriate application logically entails the existence of some supernatural being that turns out not to exist). If this were the case, it may well be that all (or almost all) of our beliefs about which laws of nature exist would be false. If this were true, it might be advisable to adopt a more ontologically conservative philosophical analysis of *laws of nature* so that we might have a more useful concept.

In what follows, I will argue that we ought to distinguish between strong laws, which are the principles that fundamental physics aims to uncover, and weak laws, which are commonly appealed to in the special sciences and in various areas of ordinary practice. I will suggest that the former sort of law picks out principles that are manifested in strict regularities and that have distinctive relations to counterfactual truth and explanation. The latter sort of law, by contrast, encompasses a much wider range of principles that resemble these stronger laws in certain ways. These weaker laws may not correspond to strict regularities and lack the strong laws' distinctive relations to counterfactuals and explanation. I will begin by identifying the basic features of each concept and will

close by comparing my proposed distinction to others that have been proposed in the literature on laws. If my argument here is successful, then we have good reason to doubt the plausibility of any philosophical analysis of law of nature that purports to provide a unified treatment of these (quite different) sorts of laws.

In order to argue that there are two distinct concepts of law, I will suggest that certain paradigmatically correct uses of 'law of nature' are incommensurable with other, also paradigmatically correct, uses of this phrase. This argument is premised on the fact that competent users (e.g., practicing scientists) of 'law' terminology are *not* massively mistaken in their judgments (or "intuitions") about laws in cases where these errors cannot be attributed to factual ignorance. So, for example, if such speakers regularly use appeals to 'laws of nature' to support counterfactuals or to ground explanations, we have good reason to think that there is a concept of law that is closely tied up with explanation and counterfactual truth. The cogency of this argument hinges on the assumption that competent speakers are, at least to some extent, authorities on how their concepts work. I do not take this to be a controversial assumption[1]. It does not rule out speakers being massively wrong about matters of fact (e.g., they falsely believe that *F=MA is a law* in cases where this error can be attributed to some sort of factual ignorance [e.g., they are unaware of Einstein's arguments]). It merely means that, absence evidence of inconsistency, charity requires that we assume that competent speakers are using their concepts correctly. In any case, it borders on incoherence to claim that speakers of a language are *massively* mistaken about the way that their own concepts are applied. So, if we discover that competent speakers have consistently believed that *were Newtonian mechanics to have been true, then F=MA would have been a law*, we have strong *prima facie* reasons to trust their judgments.

[1]In keeping with Carnapian methodology, I will often speak of 'concepts' as being something like the meanings of terms. The premise about the authority of competent speakers is compatible with, but does not require, Bealer's (1996; 2002) and Jackson's (1994; 1998) view that people have *a priori* access to their own concepts. So, for example, this premise would also fit well with Goldman's (1999) "naturalized" defense of intuitions. Goldman argues that naturalists ought to accept that competent speakers' intuitions ("conscious, spontaneous judgments") are a reliable indicator of whether a certain *concept* can be correctly applied, where *concepts* are non-conscious psychological states closely associated with the application of certain natural-language predicates. Goldman holds that speakers' intuitions provide us with strong *a posteriori* evidence about the extensions of various concepts (21-23).

With this in mind, I will assume that if we can find clear criteria for applying phrases like 'law of nature' in one context and different (and inconsistent) criteria for applying 'law of nature' in another context, we can provisionally conclude that there are at least two concepts of law at work. The claim that the concept *weak law* is derivative (or merely metaphorical) can be dealt with in a similar way. As a matter of historical fact, it seems likely that the strong concept of law preceded the weaker one, and that the weaker concept came into use with the perception that certain other principles were like these strong laws in some way. This historical fact does not entail that the concept of weak law is illegitimate, however, nor that it can be identified with the strong concept. After all, many historians have hypothesized that the original concept of law of nature (which I have called *strong law*) originated in a metaphorical application of legal concepts to the natural world[2].

2.2 THE FIRST CONCEPT OF LAW: *STRONG LAW*

I will label the first concept of law *strong law*. I will argue that strong laws (1) correspond to strict regularities, (2) are counterfactually robust, (3) do not depend on human interests or contexts, and (4) bear distinctive relations to explanations. Strong laws are the sorts of laws that those working on cosmology (and other branches of fundamental physical theory) aim to discover, and are the types of laws relevant to philosophical debates about free will, physicalism, and induction. In a certain sense, the concept *strong law* is a relatively easy one to understand and examples of strong laws (or at least, examples of assertions about strong laws) can easily be found. For

[2]The precise relationship between the legal and scientific concepts of law has been a matter of some debate. Zilsel (1942) and Needham (1951a; 1951b) contend that the scientific concept is essentially a straightforward application (by Descartes) of the legal concept onto the natural world, where God fills the role of a legislator. More complex (but roughly congruent) stories are given by Oakley (1961) and Ott (2009), who contend that the Cartesian concept of law is closely tied up with Descartes's beliefs concerning either (a) theistic *voluntarism* (i.e., the thesis that God is entirely free to act upon the world and is not constrained by the internal "essences" of things) or (b) natural law theory, which applied legal concepts to the moral world. Other writers have contended that the relationship between the two concepts of law is much more distant; for example, Ruby (1986) contends that the scientific concept of law arose from an analogy with mathematical or logical laws, and not from any direct analogy with legal concepts.

example, in a universe governed by classical physics, Newton's laws of motion would be strong laws, as would his law of universal gravitation. Similarly, the equations of general relativity (GR) or quantum mechanics (QM) would plausibly express strong laws if true. So might the law of conservation of mass-energy and the second law of thermodynamics. Conversely, the "law of supply and demand" of economics is not a candidate for being a strong law, nor are the "laws" of folk psychology. These points should be uncontroversial; the problem facing us is to figure out what, if anything, distinguishes these two varieties of laws.

While my interest in laws of nature is not primarily historical, a bit of history may help to clarify this concept. Descartes, whose discussion of laws of nature profoundly influenced the development of the modern scientific understanding of laws, describes laws of nature as follows in *The Treatise on Light*:

> Take it then, first, by 'Nature' here I do not mean some deity or other sort of imaginary power. Rather, I use the word to signify matter itself, in so far as I am considering it taken together with the totality of qualities I have attributed to it, and on the condition that God continues to preserve it in the same way that he created it. For it necessarily follows from the mere fact that he continues to preserve it thus that there may be many changes in its parts that, it seems to me, cannot properly be attributed to the action of God. The rules by which these changes take place I call the Laws of Nature. (1998a, 25)

Descartes's descriptions of laws of nature capture many of the essential elements of strong laws. The first characteristic concerns the ontological distinction between the laws and the other parts of "Nature." Descartes clearly views the laws as *rules* governing the course of nature and not as descriptions of regularities within nature. This distinction is compatible with a second major feature of strong laws—that they correspond to (or perhaps cause) certain regularities. So, for instance, Descartes claims that it is a law of nature that "each particular part of matter always continues in the same state unless collision with others forces it to change its state" (1998a, 25). While the law is not identified with a regularity, it corresponds to a regularity in a straightforward way: if this is

a law, then there will be no examples of actual particles that disobey it. Descartes, like most of his successors, also takes laws to be objective, at least in the sense that the truth of propositions such as *it is a law that L* depend on facts about the universe and not, for instance, on the peculiar interests of the person who has asserted the proposition. A final aspect of the Cartesian concept worth noting concerns the relationship that it posits between laws, counterfactuals, and the actions of God. In the passage quoted above, Descartes seems to disavow the notion that the laws of nature are mere descriptions of the way God happens to act on nature; rather, he apparently understands the laws as being themselves an independently existing part of the natural world (albeit, a natural world that is entirely created and controlled by God). On Descartes's view, the study of laws falls within the domain of natural philosophy and not theology[3]. However, knowledge of Cartesian laws is clearly special in that allows knowledge about what would have or could have been the case.

From Descartes's descriptions of laws of nature, it is clear that he thinks of them as being necessary in some non-logical sense. However, he provides few details about the nature of this necessity. Insofar as Descartes claims that it is possible for God to have made different laws (by making different matter), he clearly thinks that the laws are not logically necessary. After all, God could have made different matter. However, Descartes also claims to have "showed what the laws of nature were ... and to [have showed] that they are such that, even if God had created many worlds, there could not be any of them in which the laws failed to be observed" (Descartes, 1998b, 24). Part of Descartes's confidence in having established this sort of necessity for laws is an effect of his heavy reliance on *a priori* methods; however, the Cartesian thesis that laws of nature mark out a special subclass of possible worlds has remained a central part of the contemporary concept of *strong law*.

[3]In a recent study, Ott (2009) has argued that Descartes was an occasionalist (i.e. he believed that God was the sole and unique cause of every event), and thus must have understood laws being something like *descriptions* of an omnipotent God's actions, and not as separately existing aspects of the natural world. Ott concurs, however, that the Cartesian notion of law is intended to be more than mere summary of regularity, that laws are intended to have a sense of necessity, and that their study is not part of theology. Ott, like many other historians of science, also emphasizes the historical importance of Descartes's "top-down" conception of laws, according to which laws do not supervene upon the properties of individual things.

While Descartes's use of law terminology may have provided the historical impetus for its widespread adoption, the paradigmatic use of law terminology (especially as it relates to philosophy) is plausibly provided by Newton. In the *Principia*, Newton follows Descartes in adopting law terminology in laying down his principles of mechanics (though he frequently refers to them as 'axioms'). In Book I, for example, Newton says the following about laws:

> It is the province of true philosophy to derive the natures of things from causes that truly exist, and to seek those laws by which the supreme artificer willed to establish this most beautiful order of the world, not those laws by which he could have, had it so pleased him. For it is in accord with reason that the same effect can arise from several causes somewhat different from one another; but the true cause will be the one from which the effect truly and actually does arise, while the rest have no place in true philosophy. (2004, 52)

Newton's discussion of laws, like Descartes's, highlights a number of features of strong laws that have come to be seen as central. Again, as was the case with Descartes, Newton sees the laws of nature as being parts of the natural world that are in some sense distinct from the phenomena that they govern. Also in agreement with Descartes are the ideas that discovery of laws is the province of natural philosophy, and that correct identification of the laws of nature allows one to discover the true *causes* of visible phenomena. Newton also explicitly dismisses the idea that the laws discovered by natural philosophy are mere mathematical tools useful only for describing or predicting observable events. Instead, Newton takes his laws to be the "true cause" of the observed phenomena that the our knowledge of the laws allows us to successfully predict. In the section of the *Principia* titled "On the System of the World," for example, Newton goes on to give an account of how the knowledge of the laws of motion and the law of gravitation can, together with knowledge about past events, be used to successfully predict the behavior of various observable phenomena, including tide patterns and the orbits of various heavenly bodies.

Newton's use of law terminology in the *Principia* also highlights two other impor-

tant aspects of strong laws. The first is that strong laws, or at least those that we know about, can often be described mathematically. Laws of nature, on Newton's view, are not the anthropocentric principles favored by his scholastic predecessors—the laws' obtaining does not require that the things they govern have desires, natures, or essences of any type. Newtonian "laws of nature" are thus distinct from the sorts of "natural laws" that characterize certain theological accounts of morality and the natural world[4]. The second is that knowledge of the laws is in some strong sense explanatory. Newton assumes that his derivation of tide patterns, of the motions of heavenly bodies, etc. serves to provide an explanation for why these phenomena occur as they do.

I take it that the concept of *strong law* is relatively clear. Moreover, it does not seem to be a concept that is of solely philosophical or historical interest, since one can easily find examples of physicists (mainly cosmologists) using law terminology in much the same manner that it is used by Newton and Descartes. So, for example:

1. For, like every other general law of nature, the law of transmission of light in *vacuo* must, according to the principle of relativity, be the same for the railway carriage as reference-body as when the rails are the body of reference" (Einstein, 1921, 23).

2. you can guess already from my introduction that I am interested not so much in the human mind as in the marvel of nature which can obey such an elegant and simple law as this law of gravitation (Feynman, 1965, 24).

3. What laws govern our universe? How shall we know them? How may this knowledge help us to comprehend the world and hence guide its actions to our advan-

[4]Dorato (2005) offers an analysis of laws that emphasizes the conceptual importance of their mathematical formulation, and of the way that knowledge of certain (dynamical) laws allows one to calculate the future state of a system from a complete description of its current state. He argues that laws can be productively seen as "algorithms" or "software" that are conceptually (but not actually) distinct from the "hardware" that they are instantiated on. Woodward (2002; 2003) concurs in emphasizing the importance of laws that mathematically describe the relationship between changes in two or more physical properties. While I agree that many strong laws have the form that Dorato and Woodward describe, a large number do not. For example, the law of conservation of energy, the law that light has a maximum velocity, and the second law of thermodynamics are all paradigmatic examples of strong laws, but do not describe the relationship between two or more physical qualities.

tage? (Penrose, 2004, 7).

When used in contexts such as these, the terms 'law' and 'law of nature' are being used to pick out some fundamental principle of the universe, knowledge of which is *explanatory*. Such authors regularly refer to the laws as "governing" the universe and refrain from offering any suggestion that the laws of nature should be understood as true relative only to some particular context or purpose. Such authors are reasonably interpreted as making claims about strong laws.

A number of prominent philosophical discussions of laws of nature can also be construed as pertaining to strong laws. In particular, philosophical discussions of laws that emphasize features such as laws' universality, their correspondence with strict regularities, and their sense of necessity can profitably be interpreted as being about strong laws. So, for example, consider Hempel and Oppenheim's (1948, 152-157) influential description of what they call *fundamental laws*. Hempel and Oppenheim state that fundamental laws are "universal" principles of "non-limited scope" that cannot themselves be derived from laws that are more fundamental. They also claim that such laws form a sort of explanatory bedrock, insofar as they can be appealed to in explanations but cannot themselves be explained. Finally, like Newton and Descartes, Hempel and Oppenheim explicitly deny the idea that lawhood might be relativized to a particular community's interests or evidence. Instead, laws are objective. Something like this conception of laws also seems to be at the heart of more recent debates about the reducibility (or irreducibility) of laws. So, for example, both Lewis (1973) and Armstrong (1983) agree with many of Hempel and Oppenheim's claims about the general characteristics of laws. In chapters 3 and 4, I will consider these debates in more detail.

2.3 CHARACTERISTICS OF STRONG LAWS

With these uses of 'law of nature' in mind, we are in a position to make some general remarks about the distinctive features of strong laws. In this section, I propose to do just that, but with one major caveat: I will not be arguing that we have any justified beliefs about what the strong laws actually are, nor will I argue that there even exist any strong laws. My goal here is much more modest—I will simply aim to articulate what

strong laws must be, if they are to be the types of things described in previous section. I will suggest that there are four distinctive characteristics of strong laws:

1. If it is a strong law that L then it is true that L.

2. If it is a strong law that L then, if it were the case that some physically contingent proposition P were true, then L would still be a strong law.

3. The truth or falsity of the claim that L *is a strong law* is *objective* and *context-independent.*

4. If it is a strong law that L then L cannot be given a *scientific* explanation.

These points will undoubtedly be controversial to some readers and I will present some reasons for accepting each. My main interest here, however, is not to definitively settle open philosophical debates about the nature of laws; rather, the idea is to identify a particular concept of law essential to understanding scientific practice. In the next section, I will suggest that weak laws are distinguished by the fact that they lack one or more of these characteristics.

2.3.1 Strong Laws and Truth

The first distinctive characteristic of strong laws concerns truth. In particular, it seems plausible that, if the proposition *it is a strong law that* L is true, then L is true as well[5]. Descartes, for instance, explicitly claims that the laws he has discovered would hold in any universe God created, which clearly implies that such laws hold in our *own* universe. This principle has also been widely accepted in the philosophical literature on laws. So, for example, Humean reductionists such as Hempel and Lewis offer analyses according to which laws are identified with true sentences or true propositions, respectively; Hempel claims that laws are simply universal generalizations that contain the appropriate types of predicates, while Lewis argues that they are extremely succinct

[5]This formulation assumes that L is a proposition. One might also express this principle in ontic terms (i.e., in terms of whatever it is that makes L true). For example, "If L is a strong law, then there are no violations of or exceptions to L."

summaries of the masses of particular facts. Similarly, the classic non-Humean accounts of Tooley, Dretske, and Armstrong conclude that propositions expressing laws of nature are in some strong sense *necessary* and thus, trivially, will be true in the actual world.

There are, of course, some well-known arguments concerning the "falsity" of laws. However, I do not think that these arguments need to concern us here. So, for example, both Cartwright (1983; 1999; 2002) and Giere (1999) have argued that many paradigmatic claims about laws of nature would, even if true, fail to entail the existence of strict, exceptionless regularities[6]. To illustrate this point, Cartwright (1983) offers an extended argument that Newton's law of universal gravitation would fail to correspond to a strict regularity *even if the facts about gravitation were exactly as Newton claimed they were*. More specifically, she claims that if Newton's law of gravitation is interpreted as entailing any claim about the actual net acceleration experienced by massive bodies, then this claim will be false at nearly every Newtonian world. This is because massive objects in Newtonian worlds are almost always subject to other, non-gravitational forces that affects their acceleration.

According to Cartwright, the "falsity" of the law of gravitation need not prevent it from counting as a law at Newtonian worlds. This is because the law of gravitation does capture something related to acceleration — i.e., it captures the *power* or *capacity* of massive bodies to attract one another. So, on Cartwright's view, the proposition *it is a law of nature that any pair of massive bodies will experience an attractive force inversely proportional to the square of their distance* will be true just in the case that massive bodies have a capacity to attract one another that varies inversely by the square of their distance. By itself, however, the law does not entail anything about the existence of any regularity, strict or otherwise. Moreover, according to Cartwright, this problem is a fully general one, and one that cannot be overcome by noting the possibility that there exist laws governing additional forces (i.e., A Newtonian could not escape the problem by noting that the acceleration due to charge difference is captured by Coulomb's law).

Cartwright (1983, 54-74) offers an extended argument against the viability of the

[6]Cartwright's and Giere's arguments focus mostly on causal laws within fundamental physics. There are, of course, a variety of laws that would correspond to strict regularities if true—e.g., the law of conservation of mass-energy or the law that light has a maximum velocity. Cartwright's and Giere's claim is that this is not a universal feature of the laws discussed in fundamental physical theory.

view that the law of gravitation and Coulomb's law describe *component forces* (e.g., that they describe *gravitational force* or *electrical force*). She has also repeatedly expressed pessimism about the very possibility of scientists' discovering a comprehensive "network" of laws that would jointly entail strict regularities. For example, in an early discussion of the covering-law model of explanation, she writes as follows: "Covering-law theorists tend to think that nature is well-regulated; in the extreme, that there is a law to cover every case. I do not. I imagine that natural objects are much like people in societies. Their behavior is constrained by some specific laws and by a handful of general principles, but it is not determined in detail, even statistically. What happens on most occasions is dictated by no law at all . . . My claim is that this picture is as plausible as the alternative. God may have written just a few laws and grown tired. We do not know whether we are in a tidy universe or an untidy one" (1983, 49). According to Cartwright, then, it is simply false that paradigmatic laws of nature entail strict regularities.

Several things can be said in response to such arguments. The first is that it is unclear whether Cartwright's arguments (even if successful) entail that (1) strong laws are far different than most people have thought they were (because they fail to entail the existence of strict regularities) or (2) there simply are no (actual) instances of strong laws. Insofar as our current aim is to describe a concept in actual use, the latter may provide a more plausible interpretation for our purposes. If this is the case, then Cartwright's arguments can safely be put aside, since (as mentioned previously) I am claiming neither that there are any strong laws, nor that we can have any knowledge of any particular strong laws. Even if Cartwright is correct in her claim that Newton's laws do not entail regularities, for instance, it seems implausible to think that Newton (and his successors) understood them in this way. In the *Principia*, for instance, Newton seems quite optimistic about the possibility of (accurately) capturing the net forces that bodies are subject to via a system of laws modeled on his law of gravitation.

While the first response is adequate for our purposes here, it is worth noting a few concerns about the cogency of Cartwright's arguments. Cartwright claims, for instance, the law of gravitation "lies"—that is, that it does not accurately describe the acceleration of actual bodies. However, this does not seem to be true. The law says that *if* a pair of massive bodies are experiencing no other forces, *then* they will accelerate toward one

another in a certain manner. But all that Cartwright has shown is the antecedent of this conditional is generally not satisfied by actual bodies [7]. Another reason for doubting the cogency of such arguments concerns the pessimism about induction that they seem to assume as a premise. It is (almost trivially) true to claim that our current physical theories fail to accurately describe certain phenomena and that they have left out various types of forces, entities, etc. However, it is also (almost as trivially) true to claim that our current physical theories can be applied to more systems and leave out less forces than did earlier theories. This suggests that Cartwright's argument might take as a premise a rather pessimistic view of induction; in particular, it seems that Cartwright assumes the history of physical theory so far cannot justify the belief that there will one day be a completed physical theory according to which laws correspond to strict regularities. But this seems to be a questionable inference[8].

2.3.2 The Necessity of Strong Laws

The second distinctive characteristic of strong laws concerns their sense of "necessity" or "invariance" under various sorts of actual and counterfactual changes. Strong laws, unlike merely accidental generalizations, hold no matter what. So, for instance, most would grant that it is impossible for humans to *break* strong laws, even if some may argue for the metaphysical possibility of supernatural (i.e., "miraculous") violations of the law. Similarly, it seems plausible that strong laws would have continued to hold, even if physically contingent matters of fact were to be different than they actually are. So, for instance, had President Bush ordered a nuclear assault in response to the 9/11 attacks, the particular matters-of-fact about our world would have been far different than they actually are; however, the strong laws would be precisely the same. This distinctive relation between strong laws and counterfactual conditionals has been widely recognized by philosophers working on both topics. So, for example, Goodman (1947), Kneale (1950) and Stalnaker (1968) all affirm the principle that the laws of nature will

[7] Elgin and Sober (2002) pursue this point at length, though they concur with Cartwright's conclusion that so-called "fundamental laws" may not be explanatory in the way that has sometimes been thought.

[8] Hoefer (2003) argues, contra Cartwright, that we have good evidence that fundamental physics can and will succeed in discovering laws that correspond to strict regularities.

be preserved under counterfactual antecedents with which they are logically consistent. The idea that a satisfactory analysis of laws should capture (something like this) relation with counterfactuals has been accepted by partisans of every stripe, including primitivists such as Carroll (1994, 7-10) and Lange (2000; 2005) and Humeans such as Urbach, Ward, and Roberts (1999; 2008).

The most commonly cited reason for denying the special relation between laws and counterfactuals involves its incompatibility with David Lewis's account of counterfactual semantics. Lewis argues (very roughly) that a counterfactual of the form *were P the case, then Q would be the case* is true just in the case that there are no worlds in which P is true and Q is false that are as similar to (or as "close to") to the actual world as worlds in which P and Q are both true. When considering the role that laws of nature ought to play in evaluating counterfactuals, Lewis considers and rejects the thesis that any world with the same laws of nature as the actual world will be more similar to the actual world than any world with different laws. In particular, Lewis argues that worlds with "small" miracles allowing the occurrence of P are more similar to the actual world than worlds with the same laws in which P holds because of differences in past particular matters of fact[9].

While a critique of Lewis's proposed semantics for counterfactuals is obviously beyond the scope of this project, we can provide a response adequate to our purpose here. Lewis's argument against the thesis that laws have a special relationship to counterfactuals is based on considerations that are quite far removed from scientific practice or ordinary use—in particular, Lewis's argument assumes that the evaluation of counterfactual conditionals requires the consideration of concrete, non-actual worlds and that

[9]Lewis's argument is as follows:

> Suppose a certain roulette wheel in a deterministic world i stops on black at a time t, and consider the counterfactual antecedent that it stopped on red. What sorts of antecedent-worlds are closest to i? On the one hand, we have antecedent-worlds where the deterministic laws of i hold without exception, but where the wheel is determined to stop on red by particular facts different from those of i. Since the laws are deterministic, the particular matters of fact must be different at all times before time t, no matter how far back ... On the other hand, we have antecedent-worlds ... where the laws of i hold *almost* without exception; but where a small, localized, inconspicuous miracle at t or just before permits the wheel to stop on red in violation of the laws (1973, 75).

such worlds can be ranked according to objective similarity matrices. Neither of these premises can be directly vindicated by appeal to specific aspects of the way people understand counterfactuals; instead, they are theory-laden assumptions whose plausibility is taken to rest on the formidable apparatus of Lewis's proposed semantics for counterfactuals. So, insofar as we are interested in describing the ordinary concept of *strong law*, the fact that certain aspects of its application seem to conflict with a technical philosophical theory need not immediately concern us[10].

In any case, if strong laws are necessary in this sense, then they cannot be mere "summaries" of occurrent facts, as many have claimed. Notably, this means that our conception of strong law is incompatible with the "best system" account of law defended by Mill, Ramsey, and Lewis. This thesis identifies the laws of nature with the consequences of the axioms of our simplest, strongest true theory of the world. [11]. Lewis describes the view as follows:

> Take all deductive systems whose theorems are true. Some are simpler, better systematized than others. Some are stronger, more informative than others. These virtues compete: an uninformative system can be very simple, an unsystematized compendium of miscellaneous information can be very informative. The best system is the one that strikes as good a balance as truth will allow between simplicity and strength ... A regularity is a law iff it is a theorem of the best system. (Lewis, 1994, p. 478)

Following Lewis, I will define the thesis as follows:

> (MRL) L is a strong law $=_{def}$ L is a theorem of the true physical theory that best balances strength and simplicity[12].

[10]Lewis (1979) suggests that, in general, we will judge that even small violations of laws render a world "less similar" to our own than do massive changes in particular matters of fact, and attempts to modify his semantics for counterfactuals to partially account for this fact. This suggests that Lewis recognizes this aspect of the ordinary concept of law, even if his proposed semantics for counterfactuals cannot entirely account for it.

[11]The view is first articulated by Mill (1874) and is later developed by Ramsey (1978). Lewis first proposes the view in his (1973, 73-74). Most contemporary formulations take Lewis's formulation as a starting-point.

[12]Lewis (1994) requires that candidate theories also be evaluated for how probable they make the ac-

Strength here is intended as a measure of the information conveyed by the theory, while simplicity measures its economy of expression in a certain language. There are a number of *prima facie* problems that arise when one tries to make these concepts more precise—for example, a good definition of *strength* should privilege useful information over mere listing of facts, and a good definition of *simplicity* should not end up being language-relative[13]. I will assume in what follows that some satisfactory precisification of these notions can be found.

The root problem with MRL is that it cannot account for the distinction between laws of nature and accidental generalizations—that is, it cannot account for the *necessity* of strong laws. To see why, consider the following propositions:

(U) There are no spheres of uranium more than a mile in diameter.

(G) There are no spheres of gold more than a mile in diameter.

It is plausible that, in the actual world, both U are G are true but that only U is a strong law. MRL can capture this distinction only if the inclusion of U, but not G, results in the deductive theory that best balances strength and simplicity. Since G's inclusion would strengthen the theory, its exclusion can only be justified by appeal to the loss of simplicity. Whatever the success of this maneuver in the actual world, there are reasons to think that the strength and simplicity criteria cannot generally track the distinction between laws and accidental generalizations. Consider, for instance, the class of worlds that are populated only by gold spheres that are slightly smaller than a mile in a diameter. It seems reasonable to think that, on any reasonable weighting of strength and simplicity, G (or something similar) will count as a law by the MRL criteria. After all, the inclusion of G in our theory would allow for considerable predictive and descriptive power at such worlds. There is no independent reason, however, to think that G is a strong law in every such world. In particular, it seems plausible to claim that at least one of these worlds has the same laws as the actual world, according to which G is not

tual distribution of non-nomic facts; Lewis calls this criterion that of *fit*. Since this is irrelevant to the discussion here, I've left it out.

[13]For examples of some of the problems that arise with Lewis's original treatment of these concepts, see Armstrong (1983) and van Fraassen (1989). In response to these worries, Earman (1986, 87-90) and Loewer (1996) both offer revised definitions of strength and/or simplicity.

physically necessary[14]. But, if this is the case, then the best system account fails as a definition of strong law.

The above example shows that being included in the best system is not a sufficient condition for a proposition's being a strong law. Other examples can be constructed to show that MRL fails to provide necessary conditions for lawhood. Consider the case of unobtainium, a hypothetical element discussed by Lewis (1994). Suppose that there are certain principles governing unobtainium's rate of decay analogous to those governing the decay-rates of other particles. For example, it may be that unobtainium has a .5 chance of decaying over 10,000 years. Further suppose, however, there only ever exists a single atom of unobtainium, and that it decays in exactly 9,520 years. Lewis's worry is that, in such scenarios, MRL may generate an incorrect law concerning unobtainium's rate of decay. There is a broader concern however: MRL may be incompatible with there existing any laws governing unobtainium at all. After all, if unobtainium is extremely rare, and if there exist no derived laws concerning its rate of decay, it is unlikely that the inclusion of a statement describing unobtainium's rate of decay in our axiomatized theory will strengthen this theory enough to outweigh the corresponding loss of simplicity. So, MRL will entail that there is no law governing unobtainium's rate of decay. Since an element's rate of decay is a paradigmatic example of a law-governed process, this shows that MRL fails to provide necessary conditions for lawhood.

2.3.3 The Objectivity of Strong Laws

The third distinctive characteristic of strong laws concerns their objectivity, where 'objectivity' means that the truth of claims such as *L is a law of nature* do not depend

[14]Roberts (2008) has criticized MRL on the related grounds that it cannot distinguish widespread regularities that are the result of nomically contingent initial conditions from those regularities that are not so contingent. Roberts argues that this distinction is at the heart of "fine-tuning" arguments and plays a central role in scientific reasoning. In particular, scientists and philosophers have usually assumed that the first type of regularities, but not the second, must be given some explanation. In many cases, this belief has served as a stimulus to productive scientific theorizing. As an example, Roberts offers the following case: Laplace's nebular hypothesis was proposed to explain the fact that all the planets orbited on approximately the same plane, a fact which Newton had claimed resulted from God's arbitrary will. Laplace, but not Newton, hypothesized that this regularity resulted from nomically contingent initial conditions and was thus explicable.

upon features such as the speaker's interest in asserting the claim or on the context in which the statement is being considered. I've already argued Descartes and Newton take the laws of nature to represent objective matters of fact. This assumption has also been shared by the majority of their scientific successors and by the scientifically literate folk when they engage in discussions about what the laws of nature are. It has also been shared by most philosophers who have written about laws. There is thus a strong *prima facie* assumption that there is a concept of law according to which the truth of *L is a law of nature* depends only on non-contextual, non-subjective aspects of the physical world.

Unsurprisingly, there are at least a few analyses of law of nature according to which paradigmatic strong laws fail to be objective in this sense. In particular, some recent presentations of MRL-style views have advocated subjectivism or contextualism in order to solve the sorts of problems mentioned previously. Many of these accounts have purported to do this by explicitly defining the notion of *best system* in terms of the theory, perspective, or context within which it is evaluated. So, for example, Roberts (1999; 2001) has argued for an "indexical" interpretation of the best-system analysis of laws, Halpin (1998; 1999; 2003) has argued for a Humean "perspectival" account of laws and physical probability, and Ward (2002; 2003; 2004; 2005; 2007) has argued for a "projectivist" account. In each case, the general move has been to reconcile the apparent differences between MRL and our normal way of thinking about strong laws by appealing to some feature of the *context* in which the laws are being considered. The consideration of context, which includes factors such as language, world, and the purpose for which a theory is being used, allows for the possibility of there existing multiple theories that best balance of strength and simplicity, and thus, for their being multiple systems of "laws of nature"[15]. For example, these authors suggest that the

[15]Halpin's characterization of perspective is representative of what I am calling "context":

> First, it determines a language for all candidate theories. This language is the basis for the second aspect of perspective: standards of strength, simplicity, and fit. These notions we saw to be language dependent. Third, a perspective involves particular scientific methodologies and preferences – i.e. a scientific culture provides an understanding of the appropriate ways that scientific theorizing may proceed. (2003, p. 151)

Context in the above sense might be thought of, very generally, as being made up of everything the agent brings to the table with her when considering which theory (and which set of laws) to endorse.

possibility of there being different laws in worlds with the same non-nomic facts is explained by the fact that which laws hold is determined partially by contextual features. One possible advantage of these approaches is that they give more determinate content to the overly vague notions of strength and simplicity. A second is that they might be able to better capture the distinction between laws and accidents.

I will not pursue objections to these specific accounts in detail, but it is worth noting a few ways in which these accounts diverge from our concept of *strong law*. First, these accounts entail that which propositions are laws of nature at a world will depend on the context in which that world is being considered. So, when considering the class of gold-sphere worlds described above, it is open for defenders of context-sensitivity to say that, depending on the context in which these worlds are being considered, G might be a law in all of these worlds, or some of them, or none of them. The same can be said for the analogous class of uranium-sphere worlds. The problem with this solution is that it locates the difference between U and G in the wrong place. The difference between the two propositions is not a matter of the context in which we consider them; instead, the difference reflects something about the objective makeup of the world—in particular, it reflects the differing atomic structures of gold and uranium. If U is a law and G is not, then this is true for everyone, in all contexts. It is true not just for actual human scientists, but for hypothetical Martian scientists, regardless of the language they adopt or the projects they choose to pursue. This what contextualist accounts fail to capture.

A second problem concerns the opacity of the contextual notions to those engaged in debates over the laws of nature. In particular, it is difficult to see how a scientist could determine whether others engaged in a certain debate with her share her context. So, for instance, suppose that Marie is in a debate with a researcher who (1) agrees with her on the results of all actual experiments that have been and will be performed but (2) disagrees with her on what the results of certain counterfactual experiments would be. For example, suppose he claims that, were a certain experiment E to be performed in a perfect vacuum, then R_1 would be the result while she claims that, were this experiment to be performed, R_2 would be the case. If both Marie and her colleague agree that perfect vacuums do not (and will not) exist, this cannot reasonably interpreted as a disagreement about any non-nomic feature of the world. If one conjoins these contextualist views

with some reasonable principle of charity, it would seem that Marie ought to interpret her disagreement with her colleague as an equivocation resulting from a difference in context. But this is false, since Marie is clearly right in interpreting her colleague as genuinely disagreeing with her. More generally, there do not seem to be any plausible examples of fundamental laws that vary according to context. Canonical descriptions of laws of nature by both classical and modern physicists (e.g., Descartes, Newton, Einstein, Feynman, or Hawking) never assign context anything like the role it plays on these sorts of views.

In any case, there is no need to present a knockdown argument against such views here. Whatever the merits of these contextualist views may be, no reasons have been provided for thinking that this lack of objectivity is a feature of the ordinary concept *strong law*. None of the previously mentioned authors argue for this claim based on appeals to current practice, however; instead, they claim that the thesis that the laws are subjective allows them to account for the laws' relation to counterfactuals without positing any problematic entities or relations[16]. Such analyses of 'law of nature' may or may not be successful as proposals for how we *should* speak about laws (i.e., they may succeed as explicata), but we have little reason to accept them as *descriptions* of concepts already in use (i.e., as clarifications of the explicandum).

2.3.4 Strong Laws and Explanation

The final distinctive characteristic of strong laws concerns their asymmetric relationship to explanation. In particular, while strong laws can often be used to provide scientific explanations of certain events or phenomena, they themselves cannot be given such explanations.

The use of strong laws in explanations has been widely recognized, so I will not argue for it here. As an example, consider Newton's *Principia*, which shows how the

[16]Ward defends the failure of projectivism to match up with our intuitions about the "factual status of laws" by noting that (1) the complexity of the projectivist analysis can explain why people may not have noticed that they are secretly projectivists and (2) "in light of that explanation, the naive intuition does not demand to be vindicated. Our judgment regarding the factual status of laws should be made in light of the respective merits and demerits of the candidate analyses *once they've been properly formulated.*" (2002, 212).

laws of Newtonian mechanics and the law of gravitation can be used to explain *why* the heavenly bodies move in the speeds and directions that they were known to move in.

More importantly for the purpose of distinguishing strong laws from weak laws, it does not seem that strong laws themselves can be given scientific explanations[17]. For example, if Newton had been *correct* in his claim that his laws of motion were the fundamental laws of the universe, then it would have been impossible to *explain* why these laws held in scientifically acceptable terms. This is reflected by the fact, when we are given a scientific explanation for why a certain 'law' holds, we will no longer regard as being a 'law' *in the same sense.* So, in explaining why Kepler's laws of planetary motion and Galileo's law of free-fall yielded correct predictions, Newton showed that they were not strong laws. This intuition is also reflected in many philosophical theories of explanation. I have already noted Hempel's and Oppenheim's (1948) "covering law" theory of explanation, according to which explanation proceeds by deduction from fundamental laws. By definition, such accounts of explanation cannot be applied to the explanation of strong laws themselves. The same phenomena arises if one attempts to explain strong laws according to unificationist, mechanist, or counterfactual views of explanation. We can cite no further natural regularity that "unites" strong laws, no "mechanism" underlying their obtaining, and no particular state of affairs the obtaining of which is counterfactually related to the strong laws in the correct way.

2.4 THE SECOND CONCEPT OF LAW: *WEAK LAW*

I have argued that there is a scientifically significant concept of law of nature that picks out propositions that are true, objective, and bear distinctive relations to counterfactuals and explanation. I called this concept *strong law.* In this section, I will argue that *strong law* is not the only concept of law of nature that is relevant to scientific prac-

[17]It is worth emphasizing the import of the *scientific* modifier here. Both Descartes and Newton thought that one could explain why the universe had the laws it did by reference to God's actions; a similar view is defended by Foster (2001; 2004). On these views, it is nevertheless the case that strong laws represent a sort of scientific explanatory bedrock—no explanation of the laws can be given without explicit mention of a God. The same might be said for explanations of complex laws that proceed by mathematically deducing them from known laws. In such cases, the explanation might appropriately be called "mathematical" as opposed to "scientific."

tice. More specifically, I will argue that certain (also paradigmatically correct) uses of phrases like 'law of nature' cannot plausibly be taken as pertaining to strong laws, since they violate at least one of the criteria laid out above. For the sake of convenience, I will call the concept associated with these other sorts of uses *weak law*. This concept approximates in certain ways the more familiar concept of *ceteris paribus law*. However, there are also significant differences between these concepts, which I will discuss in more detail later.

2.4.1 Examples of Weak Laws

Paradigmatic weak laws include many of the important principles of the special sciences. One might also include (as borderline cases) such analogical uses of law-terminology such as "Murphy's laws" or "the laws of dating." All such uses of law-terminology share some significant similarities to the strong laws described above, but these putative laws fail to qualify as strong laws by the criteria offered there. With this in mind, here are some principles that, while appropriately called 'laws' in some sense, are clearly not strong laws:

1. **Newton's second law (mechanics):** The relationship between an object's mass m, its acceleration a, and the force applied to it F is given by the formula $F = ma$.

2. **Galileo's law of free-fall:** A body in free fall near the earth will accelerate at 9.8 m/sec^2.

3. **Boyle's law (thermodynamics):** For a gas held at a fixed temperature, the product of the pressure p and volume V is equal to a fixed constant k. The laws is expressed by the formula $pV = k$.

4. **Bode's laws (astronomy):** The mean distance a of planet n from the sun is described by the formula $a = (n+4)/10$, where earth's mean distance = 1, and n = 0,3,6,12,24,....

5. **Thorndike's law of effect (psychology):** A behavior that produces a satisfying effect in a given scenario is more likely to be repeated in future scenarios of the

same type than a behavior that produces an uncomfortable effect.

6. **Mendel's law of inheritance (genetics):** The inheritance pattern shown by one trait will be independent of that shown by other traits.

7. **The law of demand (economics):** If the price of a good is reduced, demand will increase; if the price is increased, demand will decrease.

8. **The exponential law of population growth/Malthus's law (population ecology):** So long as the environment is stable, a population will grow or shrink exponentially.

9. **Dollo's law (evolutionary biology)**: Evolution is irreversible—no organism will have the exact same traits that some distant ancestor did.

10. **Fodor's law (folk psychology)**: If someone desires D, she will try to get D.

I assume that each of the uses (with the probable exception of "Fodor's law") above represents a plausible use of the word 'law', at least if uttered in an appropriate context. One can find numerous references to laws such as these in peer-reviewed scientific journals and philosophy of science textbooks. This suggests that the claim that "Newton's second law is not a law" is blatantly (if not analytically) false[18]. It only makes sense to deny that Newton's laws are laws if one already has a particular analysis of lawhood in mind. If one adopts this stance, however, once must grant that one is no longer talking about the concept of law that is relevant to the actual use of Newton's laws, and that one is instead relying on a philosophical *theory* of laws. One might also object to the lawhood of the above examples by appealing to disagreement among practicing scientists (e.g., about whether the law of demand is really a law). Like the first objection, this does not seem relevant, since the vast preponderance of such disagreements concern empirical matters (e.g., about how widely applicable the principle is), and not the meaning

[18]Some readers may think that I have overstated the case here a bit. After all, I've previously argued that Newton intended to make claims about *strong laws*; understood this way, it is clearly true that Newton's laws are not laws. However, I think that arguing this way is to miss the point. There is a perfect legitimate sense in which Newton's laws are laws—e.g., they are laws for disciplines not concerned with the sorts of scenarios where Newton's laws yield widely inaccurate predictions. Many textbooks in engineering, biomechanics, etc. regularly make reference to 'Newton's laws' and do so without any caveat.

of calling such principles 'laws'[19]. We may discover that none of the above principles are laws; nevertheless, this would fail to show any problem with the concept of *weak law*. Rather, such discoveries would reflect that the world was different than we had previously thought it was.

It is worth noting that all of the propositions listed above are false and hence, cannot be strong laws according to the criteria proposed in the previous section. Consider Newton's laws, for instance. General relativity shows that these laws, if taken as descriptions of actual systems (e.g., the orbits of the planets around the sun, or of the moon around the earth.) are false. General relativity also shows, of course, why and to what extent these laws can be used to provide approximately accurate predictions—they are accurate of mid-sized bodies moving at speeds significantly less than the speed of light that are not subject to strong gravitational forces. Even in these cases, however, Newton's laws are literally false; the ontology it presumes simply does not exist in our world. The case with Boyle's law is similar. The formula, which describes the relationship between pressure, volume, and temperature is false. As was the case with Newton's laws, however, it can be used to provide accurate predictions concerning certain types of systems—namely, those at relatively low temperatures and pressures. However, since its derivation depends on the assumption that gas particles have no volume, are subject to no intermolecular forces, etc., Boyle's law does not actually describe any actual gas. The same can be said of each of the other principles—in each case, they are literally false if taken as descriptions of the real world systems to which they are commonly applied[20]. Nevertheless, we have no problem in saying that these principles are 'laws' when considered in the correct contexts.

[19]This is not to say, of course, that scientists never have semantic disputes about what ought to be called a 'law.' In most such cases, however, I suspect the underlying concerns are empirical—i.e., they concern the scope or usefulness of the principle in question. I suspect that few, if any, economists would take seriously the claim that the law of demand was not a law for the simple reason that *no principle of economics is a law*. To argue in such a manner would be to misconstrue the meaning of calling something a 'law" in the context of economics.

[20]Importantly, they are not all false for the same reason. Some weak laws are idealizations or abstractions that are false because they fail to account for other forces, while others are more fundamentally mistaken. Moreover, as I will argue in the next section, it is possible for weak laws to be true.

Earlier, I argued that strong laws share at least four distinctive characteristics: their correspondence with certain strict regularities, their objectivity and context independence, their preservation under certain counterfactual suppositions, and their asymmetric relation to explanation. I will suggest that weak laws are characterized by lacking *some but not all* of the distinctive characteristics of strong laws, and by the way they approximate meeting other criteria. So, while there are no necessary or sufficient conditions for any particular principle counting as a weak law, there is a clear sort of "family resemblance" between strong laws and weak laws.

2.4.2 Some Weak Laws are False

I noted above that each of the sample weak laws described in the previous section is false. This suggests that the truth of propositions such as *it is a weak law that L* are compatible with *L*'s being false. The immediate objection to this, perhaps inspired by Fodor (1991) and Pietroski and Rey (1995), might go as follows: while the sentences listed above are false, one can produce true sentences by appending a *ceteris paribus* (CP) clause of the form "other things being equal." Once this (already implicit) clause is tacked on, according to this story, one can see that the weak laws are true and do correspond to strict regularities in precisely the same way that strong laws do[21]. So, for example, we should not understand what I've called "Fodor's law" to be disproved by a person who forgoes satisfying his or her own desires in order to aid a family member; instead we should say that people will try to satisfy their desires only "other things being equal," and note that a family member in distress counts as a case in which other things are not equal. If this were true, then Fodor's law does correspond to a strict regularity—its obtaining rules out the possibility of there being a case in which (1) a person did not seek to satisfy her own desires and (2) other things were equal (i.e., there were no interfering factors that could be blamed).

[21] More importantly, at least for the defenders of CP-clauses, claims about laws in the social sciences can be confirmed or disconfirmed in the same manner as claims about the laws of physics. For instance, we can disprove claims about strong laws by discovering that the entailed regularities do not hold. Defenders of CP-clauses are committed to the idea that claims about "laws" in the social sciences can be disconfirmed in just the same manner. Laws of both sorts entail strict regularities; if these regularities are observed not to hold, then the purported law is not a law.

There are a number of reasons for doubting that the laws explicitly discussed in the special sciences can be made true by appending CP clauses. So, for example, it seems that CP clauses may render a generalization vacuous (Earman *et al.*, 2002; Woodward, 2002) and that this solution may be incompatible with the way that the principles in questions are actually applied (Schiffer, 1991; Mitchell, 2002). While I am sympathetic to these points, I think there is a simpler reason for denying the adequacy of such a solution: namely, many of the laws of the special sciences cannot be made true simply by appending ceteris paribus clauses. Consider the laws of Newtonian mechanics, for instance, which appear to be paradigmatic of the sorts of laws used in the special sciences. There is no simply sense in which these statements of such laws are true "other things being equal." According to GR, the ontology of mass and force they presuppose is simply false; it is not merely that Newton's laws have left out relevant forces (of the type that could be accounted for by a ceteris paribus clause).

The case is even worse for laws such as Galileo's law of free-fall, the predictions of which will always be inaccurate. After all, Newton showed that bodies do *not* fall at $9.8\ m/sec^2$, but that the acceleration due to gravity will change with distance. Again, one could deny that Newton's laws and Galileo laws are 'laws' at all (even weak laws), but there is no evident reason to do so. Such laws are typical of the sorts of things that are often called "laws" in disciplines outside of fundamental physics. Such laws can be used to successfully predict the behavior of falling bodies and support counterfactuals (had I dropped a stone off the roof, it would have fallen in accordance with Galileo's law of free fall). They thus share certain other characteristics of strong laws; it is these similarities (and not a hidden CP clause) that makes them weak laws.

It is worth considering (especially given the examples offered above) whether weak laws are always false. If this were so, we might be able to offer a succinct explanation of the difference between strong laws and weak laws: the former are true and the latter are false. On closer inspection, however, it does not seem obvious that weak laws must be false. One could, with some effort, cook up a close relation of Bode's law that is literally true, and could be used to offer precisely accurate predictions. One might plausibly do something similar with Dollo's law, which was for a time thought to correspond to a strict regularity. Even if one succeeded in reformulating these principles to guarantee

their truth, however, it does not seem that one would have succeeded in uncovering a new strong law. Instead, one would merely have discovered a more accurate (and probably more unwieldy) version of an extant weak law. I think a better explanation for the failure of such true weak laws to be strong laws cites their failure to meet the other criteria relevant to strong lawhood. It is to these criteria we will now look.

2.4.3 The Context-Dependence of Weak Laws

Some weak laws also violate the second criterion for strong lawhood—that is, claims about what is or is not a weak law do not depend solely on objective, context-independent matters of fact. To be more specific: it seems plausible that there is a proposition WL and pair of contexts C_1 and C_2 such that

1. In C_1, it is true that WL *is a weak law*, and

2. In C_2, it is false that WL *is a weak law*.

One can easily see this sort of context-dependence of weak laws reflected in scientific discussions of paradigmatic weak laws. For example, in contexts where we are concerned with objects in free-fall near the surface of the earth, it seems appropriate to call Galileo's law of free-fall a law, even though it obviously fails to be a strong law. Conversely, in contexts where we are concerned with objects in free-fall near the surface of the moon, Galileo's law cannot appropriately be called a law of any type. Similarly, while Mendel's law of inheritance plausibly counts as a weak law in a great many contexts, it would not count as a weak law for a research team interested in examining whether the inheritances of two specific alleles was actually independent. This sort of context-dependence does not occur with strong laws; so, for instance, there are no plausible examples of contexts in which *no information can be transmitted faster than the speed of light* does not count as a strong law[22].

[22]Lange (2000) offers various examples of laws that are laws only relative to some discipline or context. For example, he considers the case of Boyle's law and van der Waal's equation, both of which deal with the relation between pressure, volume, and temperature in ideal gases, and both of plausibly express laws (2000, p.211-220). Since these laws generate inconsistent predictions, Lange claims that cannot *both* be laws in a single context.

As another way of illustrating this point, consider how our claims about strong and weak laws might appear to the intelligent, but nonhuman, inhabitants of a different planet. Our claims about strong laws would presumably be relatively straightforward: whatever claims about strong laws that are true for the inhabitants of such a planet would also be true for us, and vice versa. It seems highly plausible that, were we to encounter alien life, one of the major areas of mutual interest would concern finding out what the other party knew about the strong laws. The principles I have called "weak laws," by contrast, might very well be of no interest to such beings. If the planet's mass differed from that of earth, for instance, Galileo's free-fall equations would fail to provide reasonably accurate predictions of the behavior of falling bodies; if the inhabitants' neurology and psychology differently sufficiently from that of humans, then their system of economic exchanges might massively violate our law of supply and demand. If this were case, it would be inaccurate to claim that these principles were weak laws *for these beings*.

This context dependence of weak laws suggests that weak lawhood is tied closely to usefulness. Galileo's free-fall principle, for instance, counts as a weak law just in those contexts in which it is expedient to use it for prediction. Similarly, the weak lawhood of folk-psychological, economic, or biological principles will be manifested only in contexts where there is an interest in predicting the behavior of the various life-forms (human and non-human) that inhabit our planet. If this claim about weak laws is correct, then the truth of claims like *it is a weak law that L* may be in some sense subjective or context-dependent, even if the truth or falsity of *L* is entirely objective. Of course, it does not follow that one can make arbitrary propositions into weak laws merely by manipulating features of the context in which they are discussed. So, for example, one cannot by fiat make wildly inaccurate principles into weak laws simply by treating them as such. Similarly, there is a perfectly objective sense in which, given a particular context, something is either a weak law or it is not. Aristotle's principle that massive bodies seek the center of the earth, for instance, represents an intuitive principle that many children (and uneducated adults) still appeal to on a regular basis. It is not, however, a plausible candidate for weak lawhood in any scientific context. This is because, in any context in which Aristotle's principle might be used to issue predictions,

there are a wide variety of other principles—Galileo's law of free-fall, Newton's law of gravitation, etc.—that fill the predictive role better than Aristotle's principle does. The contrast here is not between truth and falsity—all three principles are false—but between those principles that have legitimate scientific uses and those that do not[23].

2.4.4 Weak Laws and Counterfactuals

In the previous section, I suggested that strong laws are characterized by a certain distinctive relationship to counterfactuals. In particular, I argued that a strong law would remain a strong law under any physically possible counterfactual supposition[24]. This is plausibly false of all weak laws. For example, it does not seem that the law of demand would remain a law were human psychology to have evolved in a radically different way than it actually has. This dependence upon physically contingent matters-of-fact is characteristic of weak laws. Many (if not all) of the laws of biology, psychology, economics, medicine, etc. depend upon the fact that life on earth evolved in a certain way; similarly, Newton's laws serve the needs of engineers so well only because of certain (physically contingent) facts about the types of things we care about building. It is important to note that the capacity to support counterfactuals is independent of the truth of the weak law in question. The problem is rather that, had certain physically contingent facts been different than they actually are, most weak laws would fail to provide even approximately correct predictions.

For an illustration of what I am talking about, consider the following counterfactual claims, all of which I take to be correct:

1. Were the curvature of space to be much more extreme than it actually is, Newton's laws would not be weak laws.

[23] I think it would be overly strong to claim that there are no contexts in which Aristotle's principle is a weak law. It seems plausible, for instance, that this principle had legitimate claim to being a weak law in the time before early modern physics made it obsolete. This is, again, in sharp contrast to strong lawhood; Aristotle's principle is not and never was a strong law, even though it was widely believed to be so.

[24] That is, if a counterfactual supposition is logically compossible with the strong laws remaining the laws, then the strong laws would continue to be the strong laws, were that counterfactual supposition to be the case. It is, of course, logically possible for the strong laws to be different; the only point here is that it isn't physically (or nomologically) possible for them to be different.

2. If it were the case that humans wanted all and only goods that were in wide supply, the law of demand would not be a weak law.

3. If it were the case that there were a super-powerful being who periodically manipulated organisms's DNA to resemble their ancestors, Dollo's law would not be a weak law.

The important point here is not that there are some counterfactual antecedents under which weak laws would cease to be weak laws (that is true of the strong laws, too), but that they would cease to be laws under physically possible counterfactual antecedents—i.e., under antecedents that are logically compossible with all the strong laws remaining strong laws.

While the weak laws fail to support counterfactuals in the distinctive manner that strong laws do, it is important to note the weak laws do support counterfactuals and that this support in some ways resembles the support to counterfactuals provided by strong laws. So, for instance, consider each of the following counterfactuals, along with the weak law that supports it:

1. Were a pencil to fall off of my desk, it would accelerate at a rate of around $9.8m/sec^2$. (Galileo's law)

2. Were Donald Trump to desire to acquire a 2004 Ford Taurus, he would undoubtedly own one. (Fodor's law)

3. If demand for tulips were to be much higher than it currently is, the price of tulips would also be much higher. (Law of demand)

Our confidence that such counterfactuals are correct suggests that we are committed to weak laws supporting at least certain sorts of counterfactuals. Some paradigmatic weak laws, such as Newton's laws of motion, seem to be distinguished by the fact that their accuracy is preserved under the types of counterfactual antecedents that are of special interest to us[25]. It may be that there is no specific relation to counterfactuals

[25]A number of authors have written on the relationship between counterfactuals and the principles appealed to in the special sciences. Lange (1999; 2000), for instance, argues that the laws of the special

possessed by all and only weak laws; instead, it seems that many weak laws support counterfactuals in a manner that is somewhat analogous to the way that strong laws do.

2.4.5 Weak Laws and Explanation

I previously argued that strong laws can be used in explanations but cannot themselves be (scientifically) explained. Weak laws, conversely, may meet neither of these criteria. Many weak laws, including all of those offered as examples, can be explained. For example, GR can explain why Newton's laws hold for certain types of systems and facts about cellular biology and genetics underlie the truth of Dollo's law. Even in those cases where we cannot currently explain a weak law's obtaining, this seems to be merely a feature of our ignorance about the underlying mechanisms and not a fact about the nature of the world. Conversely, weak laws may themselves be of only limited explanatory value. In particular, many weak laws fail to explain their instances. So, for instance, one need not (and should not) appeal to Bode's law to explain *why* a particular planet has the orbit that it does, nor does it seem profitable to cite Dollo's law when one is asked why the traits of a particular organism differ from those possessed by a long-distant ancestor. This explanatory deficit obtains despite the fact that these principles are both predictively accurate and are relatively counterfactually invariant. Instead, the problem seems to be that such laws are best understood as *effects* that are to be explained and not as fundamental facts that are to be appealed to in the explanation of other phenomena[26].

Again, it should be stressed that appeals to weak laws can and do feature prominently in scientific explanations. The law of supply and demand, for instance, can reasonably be cited in the explanation of many economic systems, and Newton's laws can be cited in the explanations of a wide variety of phenomena. The point here is merely that weak laws are the types of principles that can be explained, and that not *every* weak law

sciences have a relationship to the non-nomic facts of the their domain that precisely mirrors the relationship of the laws of physics to domain of physics. Skyrms (1980; 1995), Mitchell (2000), and Woodward (2003) have also offered accounts that, while not agreeing with Lange's on specific details, concur in the conclusion that the principles of the special sciences relate to counterfactuals in a manner that resembles, but does not duplicate, the relationship between the laws of physics and counterfactuals.

[26]For a discussion of the way that the laws of psychology are often understood as effects, see Cummins (2000).

cannot be used to generate informative explanations.

2.5 CONTRAST WITH OTHER VIEWS

I have argued that there are two concepts of law of nature that need to be distinguished: *strong laws* and *weak laws*. More specifically, I have argued that (1) there is a concept of *strong law* which applies to principles with a number of distinctive characteristics; (2) there are numerous, plausible uses of 'law' that do not commit speakers to use of this concept and (3) there is therefore a second, broader concept of law I have called *weak law*. In this section, I will attempt to bring out the import of this distinction by contrasting it with two other recent distinctions between types of laws of nature. In particular, I will consider Swartz's (2003) distinction between "scientific laws" and "physical laws" and Earman's and Roberts (1999) distinction between "ceteris paribus laws" and "laws of nature."

2.5.1 Contrast with Swartz

Swartz (2003) argues that there are distinct concepts of *scientific law* and *physical law* (or *natural law*). In some significant ways, this distinction mirrors the one I have drawn; however, there are also some key differences. Scientific laws, according to Swartz, are "the pronouncements of scientists" and "are what they invoke in their explanations of physical phenomena" (2003, 4). He points out, as I have with respect to weak laws, that many of the principles appealed to in scientific explanations are known to be false. He defines scientific laws to be the principles, true or false, that feature in these explanations. He offers as examples of scientific laws many of the laws of physics and of the special sciences.

Swartz claims that physical laws, by contrast, must be literally true (2003, 1). The fact that we have a concept of *physical law*, argues Swartz, can be inferred from the fact that we must understand the approximate truth of scientific laws as being approximate to something. Similarly, Swartz argues that physical laws are what govern the universe (if anything does) and are the laws relevant to debates about free will and determinism. Along these lines, Swartz writes that

> 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 [i.e., physical laws cannot be the same thing as scientific laws]. (2003, 14)

Swartz's contrast here is a useful one and a corresponding distinction holds between strong laws and weak laws: the existence and nature of strong laws, but not weak laws, is relevant to the such things as the truth of determinism and the possibility of miracles.

Swartz's concept of *scientific law* resembles, in some ways, the concept of *weak law* that I have identified. The primary difference between the two involves the fact that the definition Swartz provides of *scientific law* is, depending on how one reads it, either overly inclusive or too vague. On the one hand, by Swartz's explicit definition of *scientific law*, nearly every principle that has (or could?) be used by scientists to explain anything will count as a scientific law. On this reading, Swartz's *scientific laws* do not pick out any concept that scientists care about: not every principle that features in a scientific explanation qualifies as a law of any type. On the other hand, many of Swartz's comments suggest that this simplistic interpretation is not the end of the story. For example, Swartz dismisses the thesis that scientific laws must be "close to true" on the grounds that "sometimes the truth is so complex that a proposition that approximated closely to it would be so unwieldy as to be useless." He instead suggests that scientific laws are characterized by possessing "the imperfectly understood property that confers suitability for use in explanation [, which] must be some complex property involving a weighted mixture of closeness-to-the-truth along with tractability and human comprehensibility" (2003, 10). If Swartz's analysis is to amount to anything more than the vague idea that the laws are whatever features in scientific explanations, more will need to be said about these factors are to be weighed against one another. In any case, however, Swartz's characterization of *scientific law* does not suggest that such laws have anything like the family resemblance to strong laws that I have argued is essential to weak laws.

A second distinction between strong laws and weak laws involves the former's overly

tight connection to the epistemic powers of the agent who is considering them. This follows from Swartz's requirement that scientific laws be capable of featuring in comprehensible explanations. Swartz suggests that the reason that certain principles count as laws (but others do not) depends largely on the cognitive abilities of the agents involved in offering explanations. Given the requirement that principles be tractable for the agents that use them, for instance, it is possible for an equation or proposition that is not currently a scientific law to become a law merely through the introduction of greater computing power or by the scientific community that is considering it becoming more skilled at its application. On Swartz's view, the laws of GR or QM risk not being laws for those agents who don't have the relevant mathematical expertise. But this seems straightforwardly false; such laws are laws even if particular agents fail to grasp them. This sort of relativization to the cognitive abilities of the agent goes beyond the discipline-specific context-sensitivity that I have argued characterizes weak laws; instead, it seems to risk creating a new set of laws tailored to each agent's cognitive abilities and each scenario's predictive and explanatory demands.

The concept of *weak law*, in contrast to *scientific law*, is not identified with any equation that can be used in scientific explanation. Instead, the concept of weak law carries with it a number of other substantive requirements. Specifically, weak laws have some but not all of the characteristics that strong laws have. This includes not only explanatory value and closeness to truth, but also counterfactual invariance, which does not relate to an agent's cognitive abilities at all. For instance, consider such paradigmatic weak laws as Newton's laws or Mendel's laws. Both principles feature prominently in the explanatory and predictive work of their respective disciplines. Similarly, in these domains, we have every reason to think these principles would continue to be highly accurate under a wide variety of counterfactual suppositions. So, for example, Newton's laws would continue to hold under the sorts of variations in speed, atmosphere, and acceleration that are of interest in mechanical engineering. Similarly, the counterfactual invariance of Mendel's laws can be all but taken for granted in certain important contexts. Conversely, we do not think of highly specific equations (e.g., equations describing the gas mileage of a particular model of car) as being laws even though they may be useful for some applications. This suggests that weak laws, insofar as they can

properly be said to resemble strong laws, must be more than mere equations that play some role in explanation; instead, they are perceived as playing relatively central roles in certain types of scientific theories.

There are also significant differences between the concept of *strong law* and Swartz's *physical law*; in particular, his characterization of physical laws as a sort of "limiting point" of scientific laws fails to capture what is distinctive about such laws. Swartz argues that, insofar as we view scientific laws as being approximately true, we must be committed to their being approximations to something. This something, on Swartz's view, is physical law—laws which correspond to strict regularities. It is our commitment to the existence of such laws that grounds our faith that the physical world can be described (even in an approximate way) by scientific laws. In short, while the specification of physical laws may not be an explicit goal of scientific inquiry, science is committed to their existence insofar as "science itself has an objective focus and a court of appeal beyond mere consensual favor among learned practitioners" (2003, 11). According to Swartz, the claim *physical laws exist* is simply a short, simple way of stating *there are strict regularities in nature*.

Strong laws, unlike physical laws, are not identified with strict regularities or with the propositions that describe them. In particular, Swartz's specification of *physical law* identifies them too closely with general truths and ignores the other distinctive characteristics of such laws. In short, Swartz's concept of *physical law* seems to be closer to the (revisionary) concepts of law described by reductionist theories of lawhood rather than any concept of scientific practice. In fact, Swartz seems to grant precisely this point in his discussion of physical necessity (Swartz, 2003, 58-60) where he suggests that this ordinary sense of lawhood (which includes a certain sort of necessity) is in some way mistaken or impossible. In making this argument, however, Swartz must forfeit his claim that *physical law* captures the concept of law relevant to describing actual practice. Again, this is not to say that revisionary claims are necessarily mistaken; however, their discussion goes beyond the bounds of this chapter, which is concerned only to characterize concepts actually in use. In chapter 3, I will consider the Humean reductionist view in much more detail.

2.5.2 Contrast with Earman and Roberts

Earman and Roberts (1999) have also argued for a distinction between different concepts of law. On their view, at least some of the fundamental laws of physics are understood (by physicists) as something like strong laws. Earman and Roberts label these laws "strict laws." Strict laws are picked out by the equations of fundamental physical theories and they state strict, exceptionless regularities. Earman and Roberts note, as I have above, that the generalizations of the special sciences do not fit this criterion. They then go on to argue that there is a second (derivative) concept of law of nature that encompasses certain principles discussed outside of fundamental physics. Following the literature, they label such laws "ceteris paribus laws." Unlike most defenders of ceteris paribus laws, however, they argue that this derivative concept of law extends only to generalizations whose domain and approximation to truth can be well specified (e.g., by deduction from strict laws and initial conditions) and thus does not apply to the generalizations of the special sciences. They conclude that the generalizations of the special sciences, at least in many cases, are no more than summaries of statistical data, and share none of the distinctive characteristics of strict laws.

For my purposes here, the distinction between strict laws and strong laws can be safely set aside. Earman and Roberts's discussion of strict laws is relatively unproblematic, and their argument that statements of strict laws are (or at least, purport to be) strictly true seems cogent. There are, however, significant differences between the description of weak laws given above and Earman and Roberts's characterization of ceteris paribus laws. As was the case with Swartz's distinction, however, there are good reasons for doubting that Earman and Robert's descriptions map onto to any concept that is actually in use. To see why this is, consider their definition of *ceteris paribus law* as

> a generalization that plays some of the roles of laws in the science at issue, and that is not strictly true but that, nevertheless, is approximately true in most of its intended applications; extraordinary situations may render the generalization false, but such situations are not among the intended applications, and in most of the intended applications, the generalization is reliable. (1999, 463)

Earman and Roberts go on to argue that such laws have content only if we can specify (with a fair amount of precision) what is meant by "approximately true" and "most of the intended applications." If we cannot do this, they contend that such "laws" are merely empty or, at best, are elliptical ways of pointing to various statistical correlation[27]. Hence, they argue that the paradigmatic examples of ceteris paribus laws are the laws of phenomenological thermodynamics, whose range of application and approximation to truth in a given situation can be derived from the strict laws of fundamental physics. Generalizations of the special sciences, almost never admit of such derivation; hence, they are not laws of any type. Earman and Roberts go on to suggest that the primary value of such special science "laws" belongs entirely to the context of discovery and the role they play in helping scientists formulate "embryonic theories." They conclude that, if such laws are currently perceived as playing more substantive roles in the special sciences, this calls not for "logical analysis" but for "better science" (1999, 466).

The problem with this proposal is that it is far too strong in what it requires of the laws of the special sciences. On Earman's and Robert's own account, the laws of phenomenological thermodynamics are laws because "the reduction of thermodynamics to statistical mechanics makes it possible to give a clear sense to the crucial phrase 'in most of the intended applications"' (1999, 464). Since such reductions are impossible in the case of, e.g., cognitive psychology, there can be no laws of such sciences. One way in which this claim is too strong can be seen in terms of Earman and Robert's own example. As they note, the laws of phenomenological thermodynamics were originally called 'laws' because it was felt that they were true—that is, it was thought they were strong laws. On this account of laws, however, not only were the nineteenth century scientists mistaken in believing that their formulation of the laws of thermodynamics stated strict laws, they were mistaken in their belief that they had expressed anything substantive at all in their formulation of these principles, since they were not in a position to specify the precise situations in which these principles would have a reasonable degree of accuracy. This does not correspond with ordinary usage, under which we would say that nineteenth century scientists discovered the laws of thermodynamics, but were

[27]Earman and Roberts read causal statements in a similar way. So, for example, they argue that "Smoking causes cancer" is an elliptical way of expressing/referring to various statistical claims.

not yet in a position to specify with accuracy the boundaries of the domain to which they could be applied accurately. This same conclusion would hold for *any* "law" of physics which was later to discovered to have limits to its application—Newton's laws, Maxwell's equations, the laws of GR, etc.

If this is the case, then we have at least some reason to be charitable toward the social-scientific principles I've identified as weak laws; the mere fact that we cannot currently specify the appropriate domain does not entail that a principle cannot function in explanations or predictions, or cannot support counterfactuals. Earman's and Roberts's concerns about the testability of social-scientific generalizations are, of course, legitimate ones; however, the history of science does not seem to support the pessimistic view that a failure to be able to explicitly specify a principle's domain of application entails its untestability or explanatory vacuousness.

2.6 CONCLUSION

In the introduction, I suggested that, once one recognizes there are two concepts of law, some progress might be made on settling some of the outstanding philosophical debates about laws. Much of the remainder of the dissertation will be devoted to arguing for this claim in more detail. In general, I will proceed upon the assumption that philosophical accounts of laws of nature are intended as descriptions of the ordinary concept of *strong law*, and will thus drop discussion of weak laws except where this becomes relevant. Where the philosophical account diverges from ordinary usage, I will consider what reasons, if any, we have for replacing our ordinary concept with the one resulting from the proposed philosophical account.

CHAPTER 3

THE LAWS DO NOT REDUCE TO HISTORY

In a recent paper, Schaffer (2008) has argued that the *history* of a world—the totality of that world's non-nomic, non-modal happenings—entirely determines what that world's laws of nature are. Schaffer's reductionist thesis, or something close to it, has also been defended by advocates of Humean Supervenience[1]. In this chapter, I will argue that reductionism about laws of nature is false. My argument will proceed in several stages. In the remainder of this section, I will consider some of the major statements of reductionism about laws and sketch their relations to one another. I will then articulate the strong *prima facie* case against reductionism, paying special attention to the role played by Carroll's (1994) "mirror argument." In the remainder of the chapter, I will consider recent attempts by reductionists, Schaffer among them, to avoid the anti-reductionist conclusion of the prima facie argument. I will conclude by suggesting that *nomic primitivism*—the thesis that the laws cannot be reduced to the non-nomic facts about the entities they govern—remains a viable and attractive thesis.

3.1 REDUCTIONISM ABOUT LAWS AND HUMEAN SUPERVENIENCE

Schaffer (2008, 93) defends the following reductionist thesis, which I will label RL ("Reductionism about laws"):

RL: Laws reduce to history.

[1]Lewis (1986; 1994) offers the canonical formulation and defense of Humean Supervenience, while Lewis (1973; 1983) defends reductionism about laws via a "best-system" approach. Other contemporary defenses of Humean Supervenience and/or reductionism about laws include Earman (1984; 1993; 2005a; 2005b), Loewer (1996), Roberts (1998; 2001; 2008), Halpin (1998; 1999; 2003), and Beebee (2000).

In order for this thesis to have definite content, a little more needs to be said about lawhood, reductionism, and history. Lawhood is left undefined, but is said to be "the sort of thing expressed by the equations of scientists, such as Newton's laws of motion and Schrödinger's law of wavefunction evolution" (2008, 83)[2]. History is described as the "fusion of all events throughout spacetime" (2008, 83), where events are understood to be concrete particulars that occur at specific places and times. History does not include any irreducibly modal facts—i.e., no fundamental facts that concern laws, counterfactuals, dispositions, or their ilk. Finally, Schaffer argues that reductionism ought to be understood as an *ontological* thesis about the types of things that exist and not merely as a "theoretical" or "definitional" thesis concerning the relations between theories, concepts, or terms. If reductionism is true, then this is a necessary relation that holds at all worlds, and not simply a description of the way that laws and history are related in our own world.

I will tentatively define *nomic primitivism* (or *primitivism about laws*) to be the thesis that RL is false. This definition of primitivism assumes that there are no plausible versions of reductionism about laws that do not have RL as a consequence. In the remainder of this section, I will provide some reasons for thinking that this is so. In the next chapter, I will suggest that this picture is a bit more complicated, as nomic primitivism is also incompatible with law necessitarianism, according to which the laws of nature are metaphysically necessary.

While Schaffer claims that RL is an "ontological" and not a "conceptual" thesis, one can formulate similar theses in terms of the relations between certain *concepts.* So, for example, Carroll (1994, 8-12) articulates reductionism about laws in terms of *nomic concepts*. Nomic concepts are those that explicitly involve things such as laws, causes, dispositions, or physical probabilities. *Non-nomic concepts*, by contrast, are those that do not *explicitly* involve such problematic entities. However, non-nomic concepts might still have implicit nomic commitments—e.g., it might be the case that a non-nomic concept (such as *justified induction*) can be applied correctly only in worlds in which there is at least one law. The set of *concepts free of nomic commitments*, a proper subset of the non-nomic concepts, includes only those concepts whose conditions of correct application

[2]In a previous chapter, I called this concept of law *strong law.*

are entirely independent of the nomic concepts[3]. Finally, we might take a world's *nomic features* and its *features free of nomic commitments* to be, respectively, the facts about that world that can be expressed in terms of nomic concepts and concepts free of nomic commitments. Reductionism about laws is the claim that a worlds' nomic features reduce to its non-nomic features. While both 'history' and 'concept free of nomic commitments' are left intentionally vague, it seems plausible that Carroll's conceptual definition of reductionism agrees with RL. This characterization of reductionism is consistent with the above-mentioned caveat that RL's truth need not guarantee our ability to conceptually or theoretically reduce particular claims about laws to claims about history.

A more difficult question concerns the relation between RL and the various theses of *Humean Supervenience* (HS) that have been defended in recent years. Those who advance such theses usually take them to be incompatible with nomic primitivism; there are thus good reasons for articulating a definition of *nomic primitivism* (and HS, respectively) on which this incompatibility is respected. HS theses might plausibly be thought to diverge from RL in at least two ways, however: first, in the constraints placed on the *reductive base* and second, in the specific *modal character* of the relations that are claimed to hold between the laws and this base. While I cannot hope to definitively settle this matter, I will offer a few reasons for thinking that any adequate formulation of HS ought to entail RL.

3.1.1 Characterizing the Reductive Base

The first potential area of divergence between RL and HS concerns the characterization of the facts that constitute the reductive (or "Humean") base. RL states the base can contain only non-modal facts. Thus characterized, it appears to be more permissive than certain prominent characterizations of the Humean base. Lewis, for example, defines HS as follows:

> Humean Supervenience is yet another speculative addition to the thesis that truth supervenes on being. It says that in a world like ours, the funda-

[3]Carroll grants that this final set of concepts includes the logical concepts (e.g., the concept of *logical entailment*). Carroll contends, however (and I agree), that it is much more difficult to identify any non-logical concepts that are obviously free of nomic commitments.

> mental relations are exactly the spatiotemporal relations: distance relations, both spacetime and timelike, and perhaps also occupancy relations between point-sized things and spacetime points. And it says that in a world like ours, the fundamental properties are local qualities: perfectly natural intrinsic properties of points, or of point-sized occupants of points. Therefore it says that all else supervenes on the spatiotemporal arrangement of local qualities throughout all of history, past and present and future. (1984, 474)

Lewis's "fundamental relations" and "fundamental properties" here play a role roughly analogous to "history." However, Lewisian Humean Supervenience (HSL) is in at least one respect a *stronger* thesis than RL is, in that HSL (unlike RL) requires that the fundamental relations and properties be the spatiotemporal relations and local qualities, respectively. Therefore, RL can be true without HSL being true. This might be the case, for instance, if the occurrences of particular events (e.g., the quantum entanglement of a given pair of particles) could not be reduced to what happened at particular space-time points (e.g., the events concerning each particle taken individually)[4].

RL's characterization of the base is also sufficiently permissive to capture Earman and Roberts's (2005a; 2005b) recent proposal that HS be understood as the thesis that the nomic facts of the world are determined by the facts that are, given the laws of that world, *measurable*. More specifically, they claim that the Humean base of a world can be characterized as follows:

> A fact F at world w belongs to the Humean base at w if and only if there is a spatiotemporally finite observation or measurement procedure P which is nomologically possible at w, such that at w, P is a nomologically reliable method for detecting whether F. (2005a, 17)

This "measurability" definition of Humean Supervenience (HSM) requires that the laws of a world be reducible to facts that these laws themselves entail are measurable—for

[4]In fact, Lewis's supervenience thesis has been criticized by fellow reductionists on the grounds that it is inconsistent with plausible interpretations of quantum mechanics (Loewer, 1996; Oppy, 2000; Earman & Roberts, 2005a).

example, by facts concerning the pattern of instantiation of properties whose particular instantiations could have been detected by exploiting their nomic connections with other properties in some manner. This allows for the possibility of spatially separated particles being entangled, so long as this entanglement makes some nomologically detectable *difference*. Such a base excludes any fact that does not make this sort of difference. Just as was the case with HSL, HSM can reasonably be interpreted as a *stronger* thesis than RL, and one which entails it. Whether or not the entailment holds will depend on how one understands the notion of *measurement* used to formulate HSM. In particular, HSM will entail the truth of RL only if a fact's *measurability* by a spatiotemporally finite procedure guarantees its non-modal character (and thus, qualifies it for inclusion in that world's history). While Earman and Roberts do not offer an explicit definition of measurement, I think it is plausible that they intend HSM to exclude primitive modal facts. The thesis that the measurable facts are among the non-modal facts is further supported by Earman and Roberts's contention that the Humean base consists of "the set of all facts that could serve as initial or boundary conditions" (2005a, 16).

Not every extant characterization of HS entails RL. However, I think that there are good reasons for doubting the adequacy of those that do not. Loewer, for example, has proposed that the Humean base of a world be understood as consisting of all and only the properties instantiated at points in the "fundamental space" of that world (1996). Insofar as Loewer places no modal or nomic constraints on the qualities that might be instantiated at the "fundamental spaces" of worlds, it is not clear that his version of HS entails RL. However, as Earman and Roberts have pointed out, this definition also fails to delineate a plausible *Humean* position[5]. This is because certain blatantly non-Humean worlds (e.g., a world containing contingent necessitation relations between universals) can be coherently thought of as having their own "fundamental spaces." It seems plausible that any version of HS that does not entail RL will have similar difficulties. With this in mind, I will assume that any satisfactory characterization of the Humean base ought to ensure that it is a subset of history.

[5]In the philosophical debates about laws, causes, and probabilities, so-called "Humeans" defend the Hume-inspired position that there are "no necessary connections" (either metaphysical or physical) between distinct events. They are in particular opposed to both nomic primitivism and law necessitarianism. It is unclear whether Hume himself either did endorse or would have endorsed such a position.

3.1.2 The Modal Character of Reductionism about Laws

A second area of potential divergence between RL and HS concerns the *modal character* of the reductionist thesis. RL, as formulated by Schaffer, is a claim about metaphysical necessity—i.e., as the claim that the laws are determined by history *at all possible worlds.* By contrast, at least some authors purport to defend contingent varieties of HS. So, for example, HSL holds that the laws of nature supervene on the Humean base *at worlds like ours*; so, if HSL is true, then the *actual world* is not a counterexample to RL. However, HSL does not specify whether this sort of relation holds at worlds that are *not* like ours. In particular, it seems to leave open the metaphysical possibility that HSL is false at worlds in which so-called "alien properties" (i.e., properties that are not instantiated in the actual world) are instantiated.

There is a closely related generalization of HSL that is both metaphysically necessary and suggested by Lewis's own characterization of the thesis, however: the laws of nature supervene upon *particular matters of fact* (i.e., facts that concern individuals or particular instantiations of properties; facts that are not global in nature) at all possible worlds[6]. This more general thesis is plausibly entailed by RL, so long as 'particular matters of fact' are taken to be among the non-nomic, non-modal facts. On this understanding of the thesis, the contingent aspect of HSL can be traced to its claim that, in worlds like ours, the particular matters of fact concern the spatiotemporal relations and local qualities. This characterization of HSL agrees with the reading given by Beebee (2000, 572) and fits well with Robert's (2001) defense of a "metaphysically necessary" version of HSL. This also fits well with Lewis's demand that satisfactory analyses of chance or laws must show how such things "supervene upon being" even if they fail to supervene upon Lewis's favored characterization of the Humean base. In any case, I think there are good independent reasons to define reductionism about laws as a *metaphysically necessary thesis*, and to define nomic primitivism as its negation. In particular, a contingent reductionist would invite many of the same types of ontological and epistemic criticisms that are often leveled against primitivism: e.g., that it allows for the possibility of

[6]Armstrong (2004, 125, ft. 1) notes that is unclear whether Lewis believed in this thesis, but remarks that if Lewis did hold that non-Humean worlds were possible "he would, however, then have condemned worlds where the supervenience fails to hold as rather unsatisfactory worlds!"

"ungrounded facts" and that it is incompatible with our having knowledge of the laws.

A contingent reductionism would also violate what many (e.g., (Earman & Roberts, 2005a)) take to be a minimal commitment of any Humean Supervenience thesis—that it be incompatible with the standard "counterexamples" that have been proposed. It is to these counterexamples I will now turn.

3.2 THE PRIMA FACIE ARGUMENT AGAINST RL

If the argument in the previous section is correct, then RL represents a *minimal* reductionist view. That is, any view that can appropriately be called reductionist ought to entail it, and *nomic primitivism* can be thought of its negation[7]. In this section, I will offer a preliminary argument that RL is false.

If RL is necessarily true, as Schaffer claims that it is, then so is the following global supervenience thesis (the "supervenience of laws on history"):

SLH: Any two worlds with identical histories will have identical laws.

One might equivalently express this in terms of a logical entailment: difference in laws entails difference in history. Insofar as SLH makes no mention of an ontological "reduction" of laws to history, it is weaker than RL. However, proponents of RL are obviously committed to SLH—if the laws reduce to history, then it should not be possible for there to exist two worlds with identical histories and distinct laws.

Supervenience theses such as SLH have been the primary target for anti-reductionist arguments. In general, these arguments can be construed as offering purported counterexamples to the above supervenience thesis. These counterexamples present pairs of

[7]In the next chapter, I will suggest that this picture is a bit more complicated then it seems here. I will argue that Law Necessitarian is also a reductive view, and that nomic primitivism ought to exclude it. My suspicion, though, is that this view is (trivially) compatible with RL. According to most varieties of law necessitarianism, the individuals of our world are individuated in part by the modal properties they bear (e.g., part of being an electron is being disposed to respond in certain ways to protons). On this view, it may not make sense to compare two worlds *just* in terms of occurrent facts, since paradigmatic occurrent facts (e.g., an individual instantiating a property at a particular place and time) will end up having a whole bunch of modal commitments following from the identity of the individuals that we are discussing. If this is the case, then RL may well be true, but only because it turns out that it is impossible to specify the "history" of a world in a way that leaves out the sorts of modal facts that bother Schaffer-style reductionists.

worlds that have identical histories but which are claimed to have different laws. Two of these purported counterexamples are as follows:

1. W_1 consists solely of a single particle moving at a uniform velocity. This world is governed by the laws of Newtonian mechanics. W_2, like W_1, consists solely of a single particle moving at a constant velocity. It, however, has only one law of nature: all particles move at uniform velocities. (Earman, 1984, 212)

2. W_1 is a world that contains only 10 types of fundamental particles and thus, allows for exactly 55 types of two-particle interactions. 54 of these interactions have been observed and the laws governing them have been determined. The 55th type of interaction, that of X-particles with Y-particles, has never and will never occur. This is not due to any law preventing their interaction, but to a mere accident of the places the particles are located. Nevertheless, W_1 has a law governing the interaction of X-particles with Y-particles. W_2 is just like W_1 in all respects but one: it has a different law governing the interaction of X-particles with Y-particles. (Tooley, 1977, 667)

If these pairs of worlds represent genuine possibilities, then SLH (and thus RL) is false. Their existence entails that it is possible to vary the laws while holding the history constant[8].

3.2.1 The "Mirror" Argument

The most developed and convincing example of worlds that agree on history but differ on laws is provided by Carroll's (1994, 60-62) "mirror" argument. He begins by describing two worlds, U_1 and U_2, each of which contains exactly five X-particles and five Y-fields and nothing else. Each X-particle has been moving at a constant velocity toward a unique Y-field since the beginning of time. When each X-particle in U_1 crosses a Y-field (each will cross at a different time), the particle acquires spin up. U_2 agrees with

[8]Note that reductionists cannot appeal to facts about counterfactuals or dispositions to defuse these counterexamples—i.e., they cannot claim that the particle that obeys Newton's laws is a *different sort of* particle than the one that obeys the uniform velocity laws. Facts about counterfactuals or dispositions, like facts about laws, cannot belong to a world's history.

U_1 in all respects except that the final X-particle to cross a Y-field does not acquire spin-up. Carroll now stipulates the following facts about laws: (1) In U_1 it is both true and a law that *X-particles acquire spin-up when crossing Y-fields* and (2) In U_2, this is neither true nor a law. In both U_1 and U_2 there is a mirror on a swivel near the final Y-field. If this mirror were turned 90 degrees, the final X-particle would never enter the Y-field.

After describing U_1 and U_2, Carroll offers an argument that the laws do not supervene upon history. The argument can be expressed as follows:

M1. U_1 and U_2 are possible worlds with distinct laws L_1 and L_2.

M2. If U_1 is a possible world, then so is the world obtained by swiveling the mirror in U_1 to prevent the final X-particle from entering the final Y-field. Call this world U_3.

M3. If U_2 is a possible world, then so is the world obtained by swiveling the mirror in U_2 to prevent the final X-particle from entering the final Y-field. Call this world U_4.

M4. U_3 and U_4 have identical histories.

M5. U_3 has laws L_1 and U_4 has laws L_2.

M6. So, since U_3 and U_4 have identical histories and distinct laws, laws do not supervene upon history.

In recent years, a large literature has sprung up concerning these arguments (and, in particular, concerning Carroll's mirror argument). Many reductionist critics (Roberts, 1998; Beebee, 2000; Schaffer, 2008) have focused on what I have labeled M5—i.e., on the claim that U_3 and U_4 have distinct laws (and thus, that they are distinct worlds at all). Carroll himself defends M5 on something like the following considerations:

M7. It is physically possible to change the position of the mirror in both U_1 and U_2.

M8. If a proposition is physically possible, then the laws would be the same, were that proposition to be true.

Carroll's idea is, roughly, that it is implausible to think that the laws of U_1 or U_2 would be different, were the position of a mirror to be different (in a physically possibly way) from what it actually was. Carroll goes on to offer an alternative formulation of the mirror argument that relies on the explicit premise that the laws will remain true under any and all physically possible counterfactual suppositions. Carroll formulates this principle (SC) as follows: "if a proposition P is physically possible and physically necessitates proposition Q, then Q would be the case if P were the case." From this, it follows trivially that "if P is physically possible and Q is a law, then Q would (still) be a law if P were the case" (1994, 51), which can be appealed to in defense of M5.

While I have already argued (in chapter 2) that something like SC characterizes the relationship between laws of nature and counterfactuals, the move from M1–M4 to M5 can be defended without appeal to any specific principle of physical possibility[9]. Instead, one need merely note that, since U_3 was obtained from U_1 by the mere movement of a mirror, no competent user of the concept *law of nature* would hold that it has different laws than U_1. The same holds true for U_2 and U_4. It is simply a brute fact about our concept of law that small changes in particular matters of fact do not change what the laws are. This is particularly true when the changes do not directly involve the properties to which the relevant law pertains—for example, when the change concerns mirrors and the law concerns X-particles and Y-fields.

In any case, it is important to note that the success of such counterexamples depends upon on the premise that the descriptions pick out pairs of worlds that are genuinely possible. In the face of relatively simple counterexamples, such as the first two given above, it might be thought that the reductionist can simply deny that they describe genuine possibilities. After all, the scenarios described are far removed from the types of situations encountered in scientific practice or everyday life. Our judgments concerning such counterfactual cases may simply be inaccurate. Given these facts, and in the absence of an explicit explanation of why the judgment reflected in these counterfactual cases is essential to our concept of laws, it may appear that the reductionist can simply reply

[9]Even if SC is not needed to defend M5, it might be relevant to the truth of RL in the following way: insofar as SC captures our intuitions about physical possibility and RL is incompatible with it, we have some (additional) reason to think that RL is false.

that such cases are question begging.

While such a response cannot simply be dismissed, I think that it misunderstands the relevance of the cases that are described. The judgments that the descriptions above pick out genuinely possible pairs of worlds reveals something about our concept of *law of nature*—specifically, they reveal that we judge it possible for the laws to differ without history differing. It is not question-begging to demand that a satisfactory analysis of law capture ordinary usage where this is possible. A better reductionist response would be to note that ordinary use is often contradictory or confused, and that it would thus be question-begging to demand that any satisfactory analysis of law capture *these specific intuitions* without further consideration of the relevant factors. However, in order for this response to succeed, it would need to be shown that ordinary use is actually contradictory or confused, and that we are thus justified in ignoring certain aspects of it. I will consider variations of this response throughout this chapter.

3.2.2 Formulating the Prima Facie Argument

With the above-mentioned caveats in mind, I will call the following argument the *Prima Facie Argument* against reductionism:

PF1. Competent users[10] of the concept *law of nature*, when presented with descriptions of possible worlds, judge that it is possible for the laws to differ without the history being different.

PF2. In cases where errors cannot be blamed on insufficient evidence, competent users of a concept are generally accurate judges of whether or not a property, relation, individual, etc. corresponding to that concept is instantiated or exists.

PF3. So, there are pairs of metaphysically possible worlds with identical histories and different laws.

[10] I understand "competent users" to be something like typical or paradigmatic users of the concept of law. This certainly includes applications of laws of nature in scientific articles and textbooks. It also plausibly includes judgments made by scientists and scientifically literate "folk." I certainly do not intend to claim that every competent user of the law concept makes such judgments. However, I take this usage to be typical enough that an adequate philosophical analysis of law ought to capture it.

PF4. So, RL is false.

The reason I call this the 'prima facie argument' is to allow for the possibility that competent users of the concept *law of nature* are incorrect about their judgments about laws in counterfactual scenarios. On further reflection, we may simply find that they have misreported their responses to the thought experiments (e.g., because the thought experiment had not been specified in sufficient detail) or that their claims about laws in these specific scenarios are inconsistent with other ontological or methodological commitments that they have. However, absent specific evidence that competent users are mistaken in this way, one must presume that their use accurately reflects their competence.

It is worth noting that the success or failure of the prima facie argument does not depend on whether the competent speakers appealed to have any true beliefs about what the actual laws are. Knowledge of the actual laws requires substantive, non-conceptual knowledge of the actual world—i.e., it requires both that one be a competent user of the concept *law of nature* and that one be in a position to ascertain which particular properties/relations/etc. in the actual world fall under this concept. In the cases we are concerned about, the relevant facts about the world are stipulated and the speaker is required merely to make a judgment based on these. It is hard to see how a speaker could legitimately be considered a competent user of a concept in the case that she regularly made mistakes in applying it in such conditions.

This concludes my presentation of the prima facie argument against reductionism. The argument can be summarized as follows: if reductionism is true, then it is not possible for there to exist worlds that agree on history but disagree on laws. But, competent users speak as if it is possible for such worlds to exist. Therefore, it is possible for worlds to have identical histories but different laws. In the next two sections, I will survey the two main lines of reductionist response to the prima facie argument. The first line of response claims that the "intuitions" underlying the prima facie argument provide little or no evidence against reductionism. The second line of response grants that the intuitions underlying the PFA may be evidentially relevant, but claims that primitivism leads to unacceptable epistemic consequences.

3.3 REDUCTIONIST RESPONSES TO THE PRIMA FACIE ARGUMENT

In this section, I will consider the plausibility of rejecting the intuitions that underlie the PFA. Reductionists have offered at least three reasons for thinking that these particular intuitions are either evidentially irrelevant or are counterbalanced by competing intuitions. The first reason is that the intuitions underlying the PFA have a "suspect source" and can thus be ignored. The second reason is that the intuitions conflict with our commitment to naturalism. The final reason is that the intuitions conflict with equally strong intuitions that support reductionism. I will argue that all of these responses fail.

Before moving on to the reductionist responses, it is worth saying a word about the role I take *intuition* to play in the PFA. By 'intuitions' I mean simply the judgments (by philosophers, ordinary speakers, scientists, etc.) that pairs of worlds such as those described in Carroll's mirror thought experiment are genuinely possible—i.e., the judgment that laws could differ without the history differing. This use of 'intuition' differs from the uses described in Bealer (1996) and Sosa (2007), according to which the faculty of 'intuition' is analogous to that of perception, and delivers contents ('intuitions') that can be the *object* of judgments. I will remain agnostic as to whether there is a faculty of intuition that is separate from our capacity to judge whether our concepts can be applied in actual and counterfactual cases. In any case, I have formulated the PFA in terms of the judgments of competent users of the concept *law of nature*, and will assume that a "mere" intuition in the Bealer-Sosa sense (without a corresponding judgment) is evidentially irrelevant. I do this in order to avoid begging substantive methodological questions against the reductionist. If Bealer and Sosa are correct in claiming that "intuitions" are evidentially relevant in this stronger sense, things are plausibly even worse for the reductionist than I argue here.

A final caveat concerns the role played by PF1, which is the claim that competent speakers actually have the anti-reductionist intuitions in question. It is, of course, a matter for empirical investigation as to whether competent speakers actually make the judgments that PF1 claims that they do. In the context of the philosophical literature on laws, however, this is largely irrelevant, since the point has been conceded by nearly

everyone who writes on the subject. The reductionists that have explicitly addressed the various purported counterexamples to reductionism have generally granted that they are intuitive and have purported to offer responses that show that reductionism should be accepted despite this. The fact that even those who are theoretically committed to reductionism share such intuitions provides strong evidence that these intuitions are not merely a matter of theoretical bias.

3.3.1 Response 1: The Intuitions Come From a Suspect source

One straightforward response to the PFA contends that the specific intuitions supporting PF1 can be dismissed because they can be explained away by reference to the religious beliefs of early users of the concept *law of nature*. This response is consistent with the idea that other intuitions about laws—e.g., that laws hold at all times and places, or that they are somehow related to explanations—are of genuine evidential import, and ought to be accounted for. While this argument is rarely spelled out in any detail, Schaffer's statement here seems to be representative

> For the notion of lawhood in use is a direct descendant of the theological views of Descartes, Newton, and Leibniz, who viewed laws as divine decrees concerning the clockwork of the world. The idea of laws as divine decrees seems to engender the intuitions of distinct possibilities. Here one is intuiting God acting in different ways. But if one rejects the view of laws as divine decrees, it is not clear why one should continue to hold onto the intuitions it engenders ... So I conclude that there is good reason to reject the intuitions involved, as remnants of a dubious theology. (2008, 95)

Similar criticisms are suggested, though not always explicitly endorsed, by van Fraassen (1989, 1-8), Loewer (1996), Beebee (2000), Halpin (2003), and others.

Beebee in particular offers an extended argument that purports to show how what she calls the "governing intuition"—the judgment that the laws *govern* the non-nomic facts and do not reduce to them—is entirely independent of competence with the concept *law of nature*. She concludes that is not *conceptually true* of laws that they govern.

After noting that the governing intuition does make sense if one believes in a divine lawmaker, Beebee offers the following argument:

> I take it to be just plainly true that belief in laws of nature does not conceptually presuppose belief in a divine lawgiver; hence if it is supposed to be a conceptual truth that laws govern, then we must look elsewhere for a vindication of the alleged concept. (2000, 581)

Beebee goes on to consider whether the intuition concerning governance can be "vindicated" by analogy with prescriptive principles such as moral laws or legal laws. She argues that the so-called "governing" nature of these prescriptive laws is tied to the fact that they can be violated in the actual world and thus conceptually require the existence of penalties and consequences attached to their (perhaps counterfactual) violation. If there were no penalties or consequences attached to violation, Beebee claims, something cannot coherently be thought to be a law of this type at all. Laws of nature, unlike these prescriptive laws, cannot be broken (or, at least, cannot be broken in a world that does not allow for divine "miracles"). Beebee concludes that, insofar as laws of nature do not share this aspect of prescriptive laws, we have no conceptual reason for holding that they must govern.

Neither Beebee nor Schaffer offer any extended argument for their claim that the problematic intuitions are the result of the religious history of the concept of law. Nor do they articulate how this supposed feature of our intuitions is supposed to undermine their value as evidence. I take it, however, that the argument might be formulated as follows:

> R1. The judgment that the laws could differ without history differing is caused by the fact that competence with the concept *law of nature* is closely correlated with the belief that laws govern the world.
>
> R2. This correlation holds because of contingent, historical facts relating to religious belief that are independent of the scientific value of our having a concept *law of nature*.

R3. Judgments about laws are evidentially relevant to determining the nature of the concept *law of nature* only if these judgments reflect the scientific value of our having this concept.

R4. So, the judgment that laws could differ without history differing is evidentially irrelevant to determining the nature of the concept *law of nature*.

The idea here is fairly simple: what is conceptually true of laws depends solely on the role that the concept plays in contemporary scientific reasoning. So, if the judgment that laws govern is caused by religious factors completely independent of scientific practice, we can safely ignore this judgment when attempting to determine what is conceptually true of laws.

The problem for the reductionist is to establish that R2 is, in fact, true. This burden cannot be met merely by showing that early users of *law of nature* had religious beliefs, or even that these beliefs influenced the way in which they thought about laws. After all, there are numerous examples from the history of science demonstrating the potential of religious beliefs to causally influence the formulation of various hypotheses. For example, it is plausible to think that physically respectable notions such as force and gravity were originally understood by Newton and others to have theological connotations. After all, Newtonian gravity might seem to be just the sort of mysterious "action at a distance" the existence of which might seem to presuppose the existence of a divine force-applier (whose actions the laws of gravitation might describe). In the end, of course, it was the empirical success of Newton's theory that seemed to justify belief in his theory, however, and not the supposition (explicit or otherwise) that there existed some divine power pushing massive bodies together. The theological beliefs of Newton are simply irrelevant in determining the nature of the concept of *gravity*. This is the case *even if* Newton's theological beliefs play an explanatory role in accounting for why and how he formulated his hypotheses concerning gravity. So, even if it *is* the case that Descartes et. al. thought that laws governed because of their theological beliefs, this by itself does not provide reason to accept R2.

I take it that one potential argument for R2 might go as follows:

R5: Early users of the concept *law of nature* believed that the laws could differ without history differing because they believed in God and for no other reason.

R6: Given the truth of R5, and absent some reason for thinking that current users of the concept have a *different* reason for believing that history could differ without laws differing, the best explanation for why we have primitivist intuitions is R2.

For the time being, I will set aside consideration of R6. I do so primarily because the considerations that might be advanced in R6 are presumably similar to those that have been offered in favor of the claims that primitivism is incompatible with *naturalism* or scientific realism. I will consider these charges in more detail later in the chapter.

In what follows, I will argue that there are good reasons to think that R5 is false. The first reason is that, as a matter of actual intellectual history, it is unclear whether or how much the *specific* intuition that the laws could differ without the history differing can be traced to the supposition that an omnipotent creator was imposing his or her will upon the universe. Instead, it seems that the belief in God was tied to almost *every* aspect of the concept of law, or that it was tied to almost none of them. The second reason is that it is doubtful that the anti-reductionist intuitions can be meaningfully separated (even for those who originally proposed the concept) from the concept's recognizably legitimate scientific applications.

A cursory examination of the historical literature on laws of nature reveals little, if any, agreement as to the original source(s) of the modern concept, and even less agreement on its relation to theological beliefs. Early scholarship places the wide use of the term 'law of nature' (or its analogues) in scientific and philosophical writing at or around the time of Descartes (Zilsel, 1942); however, more recent historians and philosophers have argued that uses of closely related concepts can be found at least three centuries earlier (Ruby, 1986), or that such concepts were widely held and used across many cultures in the premodern period (which might suggest that they are "innate" concepts) (Lehoux, 2006). Similarly, while there is general agreement that the concept is based on some type of analogy with other types of laws, there is little agreement as to what par-

ticular aspects were originally taken to be distinctive. So, for example, both Zilsel and Needham (1951a; 1951b) hold that laws were originally intended to pick out widespread regularities that could be described in simple terms. It was simply that, absent the belief in an omnipotent creator who controlled all aspects of the universe, no one had thought it plausible that there might be such widespread regularities. Ruby, by contrast, has argued that the early uses of 'law' terminology owed more to an analogy with mathematical or logical principles than they did to direct analogy with the prescriptive laws discussed by Beebee. Finally, both Oakley (1961) and Ott (2009) have argued that the central innovation of the early modern conception of law of nature was to recognize that laws were *external* to the objects they governed.

None of the sources just mentioned provide much support for R5. Consider, for instance, the account given by Zilsel, according to which belief in an omnipotent God served to motivate the belief that there existed simple, mathematically describable regularities[11]. It might seem that this sort of God-centered account is congenial to the reductionist's claim that contemporary intuitions have been influenced by previous generations' belief in God. The problem, however, is that Zilsel's account would seem to imply too much if conjoined with a "suspicious source" type argument. The reason is as follows: Many reductionists think that it is conceptually true that laws correspond to strict regularities. However, if Zilsel is correct, it was precisely *this* aspect of the concept of law that was tied up to belief in God. It would obviously be a mistake to conclude that, since belief in simple regularities is causally linked to a belief in God, the judgement that laws correspond to regularities is evidentially irrelevant to determining the nature of the concept *law of nature*. We should thus be careful in using history in this way.

[11] Zilsel describes Descartes' innovations as follows:

> The Cartesian world view combined the basic ideas of the Bible and the new physics. By the same combination of ideas [Descartes] became the most important pioneer of the concept of natural law which influenced the modern era as strongly as his dualism. Like Galileo, he took over the idea of physical regularities and quantitative rules of operation from the superior artisans of the period. And from the Bible he took the idea of God's legislation. By combining both he created the modern concept of natural law. (1942, 26)

Where Zilsel's history suggests that belief in an omnipotent God infected far more than the "suspect" uses of law of nature, the histories given by Ruby and Oakley suggest that the counterfactual commitments of the concept were far more central to its perceived scientific importance than the reductionist history suggests. Ruby, for instance, argues that laws of nature were descriptive principles modeled directly on the laws of logic or mathematics and were not intended to be in the least "metaphorical"[12]. If they are indeed modeled after such principles, this would provide a reason, contra Beebee, for thinking that laws might (as a matter of conceptual necessity) carry counterfactual commitments of the type appealed to by the PFA. Laws of logic and mathematics are paradigms of necessary truths that support counterfactuals in robust manner. One can easily imagine how an analogy with such principles would lead to the concept of a counterfactual-supporting *law of nature*.

Oakley, in contrast with Zilsel and Ruby, concurs with the reductionist in concluding that primitivist aspects of the early-modern concept *law of nature* were closely tied to a certain conception of God. More specifically, he argues that the Cartesian conception of law of nature was distinguished from superficially similar Thomist conceptions by the fact that the former, but not the latter, was understood to be *external* to the objects and properties that it pertained to (1961, 38). This externalism might plausibly be taken to account for the anti-reductionist intuition that the laws could differ without the history (which concerns objects) differing. The problem, however, is that Oakley goes on to argue that it was precisely this type of externalist commitment that led to the scientific breakthroughs of the period. According to the Cartesian concept, scientific explanation proceeded by articulating general principles governing the *relations* between various properties and not by stating the *internal* principles governing individual objects. For Descartes, the nature of these relations was determined entirely by God's will

[12]She argues that Roger Bacon, a thirteenth century innovator in optics, had something like the modern conception of law. She writes:

> In sum, Bacon's law was not in the least metaphorical. From the start it meant regularity pure and simple. It was used in the context of an approach in optics that, apart from the exclusion of a single physical phenomenon from the realm of natural law, resembled that of modern science ... in regarding nature as a set of intelligible, measurable, and predictable regularities. (Ruby, 1986, 350)

and was thus divorced from any inherent "essence" of the objects. This change of focus (from internal to external), according to Oakley, is what made plausible the idea that there might be fully general, mathematically describable "laws" governing the collisions and motions of massive bodies that were independent of the natures of the particular bodies involved. Again, there is no way of neatly separating the "illegitimate" religiously motivated aspects or applications of *law of nature* from those that seem scientifically respectable.

The point here, in any case, is not to advocate for any particular historical account of law. Instead, the goal has been a much more modest one—to indicate that the actual conceptual history of the concept of law is far more complex than the "suspect source" argument posits. There is simply no reason to think that the particular intuitions appealed to by the PFA have any more or less to do with the concept's theological history than the types of intuitions about laws that most reductionists endorse (e.g., that they are true, hold throughout time and space, etc.). A second, and much shorter, response to the "suspicious source" objection might go as follows: historical psychology is, at least for the purposes of determining what is "conceptually necessary" (to use Beebee's phrase), simply irrelevant. Reductionists seem prepared to grant that the intuitions appealed to are legitimate descriptions of the way people actually make judgments about laws. Absent some compelling reason to suppose that such judgments conflict with other commitments the speakers have, we ought to suppose that such intuitions accurately reflect the speakers' conceptual competence. In the next two sections, I will consider the plausibility of claims that there are conflicts of this sort[13].

3.3.2 Response 2: The Intuitions Conflict with Naturalism

One commonly cited motivation for defending reductionism about laws, and for thus dismissing primitivist intuitions, is that primitivism is somehow incompatible with methodological naturalism. *Methodological naturalism* is, roughly, the idea that philo-

[13]If suspicious arguments of this type *were* cogent, the philosophical causalities might well be catastrophic. For instance, insofar as many of our central ethical intuitions have a long (and well-documented) religious history, they may well be vulnerable to this same sort of argument. It seems absurd to suggest that intuitions aren't relevant to ethical debate, however.

sophical methodology and conclusions ought to be commensurable with those of institutional science (i.e., the science published in mainstream journals and funded by respected academic and extra-academic institutions). So, for example, Lewis famously describes his motivation for defending Humean Supervenience as being "not to defend reactionary physics, but rather to resist philosophical arguments that there are more things in heaven and earth than physics has dreamt of" (1994, 474). Lewis's (*prima facie* plausible) thesis seems to be that novel ontological commitments ought to spring from new empirical research and not merely from the consideration of philosophical thought experiments[14].

The problem, of course, is to determine whether adopting primitivism about laws violates this vaguely specified sort of methodological naturalism. There are a wide variety of ontological commitments that methodological naturalism might reasonably be taken to rule out; for example, it seems plausible that one cannot reconcile mind-body dualism or ethical Platonism with the respect for scientific methodology that methodological naturalism entails. More recently, Ladyman et. al. have argued that a fairly weak version of methodological naturalism rules out the acceptability of wide swaths of analytic metaphysics, including debates about composition (2007, 1-66). In the case of laws, however, the problem is complicated by the fact that both sides of the reductionist debate acknowledge that, unlike in the case of ethics or composition, scientific practice has at least surface commitments to possibilities that are not countenanced by RL. Maudlin (2007), for example, argues that physicists (both classical and contemporary) (1) never purport to offer reductive analyses of fundamental laws and (2) regularly make the primitivist assumption that it is possible for two different sets of laws to have models which are physically indistinguishable from one another[15] . Because of this aspect of

[14]There is, of course, considerable controversy as to the proper way of defining methodological naturalism. Papineau (2007) provides a good description of the thesis. He states that methodological naturalists "see philosophy and science as engaged in essentially the same enterprise, pursuing similar ends and using similar methods" while anti-naturalists "see philosophy as disjoint from science, with distinct ends and methods."

[15]Maudlin's example is as follows: Minkowski spacetime is a model of General Relativity (GR) spacetime. So, some worlds with Minkowski spacetimes have the laws of GR. However, there are theories other than GR of which Minkowski spacetime is *also* a model. One such rival theory might be, e.g., special relativity conjoined with a non-GR account of gravity (2007, 67). This shows that worlds with identical non-nomic facts (i.e., two worlds with Minkowski spacetimes) may have different laws (GR-laws

practice, it is not open to the reductionist to simply dismiss the PFA on the bare ground that the possibilities appealed to have no relevance to scientific practice.

Even if scientific practice does have some commitments to primitivism, it is of course possible that these commitments are either purely superfluous or can be eliminated with relative ease. That is, it might be the case that such commitments have little or nothing to with the practice qua *scientific* practice. If such commitments are irrelevant to (or in conflict with) the general principles governing scientific methodology, we might thus have good naturalistic reasons to reject them (even if practicing scientists may not realize this). This seems to be Schaffer's strategy. He states his argument as follows:

> Sound methodological principles (such as theoretical fathomability and ontological economy) support reducing laws to history.
> If sound methodological principles support reducing lawhood to history, then, unless sufficiently countervailing considerations can be adduced, laws reduce to history.
> Unless sufficiently countervailing considerations can be adduced, laws reduce to history. (2008, 97)

Schaffer's argument is obviously valid; I will assume for the sake of the argument that the second premise is true. This leaves just two things for Schaffer to demonstrate. The first is that sound methodological principles (i.e., principles that empirical science would recognize as legitimate) actually do support reducing laws to history. The second is that there are not "sufficiently countervailing considerations" that support primitivism against reductionism. I will consider each of these points in turn.

Schaffer offers only a limited defense of his claim that "sound methodological principles" support RL, and the range of principles he appeals to are quite narrow. In particular, he explicitly foreswears the cogency of broader "epistemic" arguments purporting to show that primitivism is incompatible with empirical methodology granting us knowledge of the laws. With regards to theoretical fathomability, he claims that "necessary connections have an air of the occult, in the sense that they imply inexplicable necessary connections between distinct existents"; with regards to ontological economy, he

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or other-than-GR laws).

claims that "it is not necessary to introduce irreducible nomic relations" (2008, 91). Insofar as Schaffer's characterization of "theoretical fathomability" is tightly tied to his general requirement that modal facts be "grounded", I leave off consideration of the claim that primitivist laws are unacceptably "inexplicable" or "occult" until the next section, where I will argue that this does not denote a methodological principle recognized by ordinary or scientific practice. In this section, I will consider only Schaffer's final claim: that the criterion of "ontological economy" supports RL, and that there are no sufficiently countervailing considerations.

Occam's razor, at least in the sense that it is appealed to here, might be understood as the principle that *parsimonious* theories are more likely to be true than their less parsimonious rivals, at least provided that all the theories fit the data equally well[16]. So, if two theories have both successfully accounted for all known evidence, the one that posits less entities, types of entities, and/or properties is the one that is more likely to be true. The application to the debate concerning laws might appear to be fairly straightforward. RL, after all, holds that there are no irreducible facts about laws; primitivism, by contrast, holds that there are (or at least, that it is possible for there to be) some such facts. All things being equal, then, Occam's razor states that RL is more likely to be true than not; insofar as Occam's razor is a methodological principle recognized by institutional science (and there are no sufficiently countervailing considerations), naturalism will also support RL over primitivism. Or so the argument might go.

On reflection, however, it is not at all clear that this apparently simple appeal to Occam's razor is consonant with methodological naturalism. This is because the metaphysical use to which Occam's razor is being put is very different from the uses that it is put to in the contexts of institutional science. One way that this difference shows up is in the way that the use of this principle might be justified. So, for example, one might justify the use of Occam's razor in physical theory by noting that, as a matter of his-

[16]Occam's razor might alternatively be understood as the principle that we ought to adopt more parsimonious theories for purely pragmatic reasons. More parsimonious theories might be more tractable, for instance. It is not clear how (or if) this version of the principle might bear on the current debate, since the usual reasons for valuing tractability are absent. For example, philosophers do not rely on their accounts of laws to make predictions regarding the behavior of specific real-world system, do not need to ensure their theories can be computed quickly, etc.

torical fact, theories that were known to be more parsimonious have generally turned out to be more accurate (and to yield better predictions) than their less parsimonious rivals. The success of this appeal requires that one (1) be able point to specific examples of parsimonious theories (e.g., Copernicus's theory) and their less parsimonious rivals (e.g., Ptolemy's theory) about which there was genuine debate and (2) be able to independently establish that the more parsimonious theory turned out to be the more accurate of the pair. Appeals to parsimony in metaphysics cannot be justified in this way, since there is no general agreement on the relative "accuracy" of historically important metaphysical theories. Instead, philosophers routinely disagree on how accurate historically important metaphysical theories are, and thus prevent us from meeting this second requirement. The problem, in essence, is that justifying the use of parsimony considerations requires that we have some parsimony-independent way of assessing theories' accuracy; in metaphysics, we simply cannot do this.

One answer to the above worry might go as follows: the use of Occam's razor on behalf of reductionism can be justified by the same evidence that justifies its use in empirical science—i.e., by appeal to its successful use in choosing between theories in empirical science. The success of this response, however, depends on whether metaphysical appeals to parsimony are relevantly similar to the appeals to it made in scientific contexts. There is at least one notable difference between the reductionist's appeal to parsimony and the uses in empirical sciences, however: namely, that the reductionist is appealing to Occam's razor to decide between *empirically equivalent theories*. By 'empirically equivalent' I mean that "no possible observation could provide us with reason to favor one over the other." This is because reductionism and primitivism disagree only about the truth of modal claims and such claims are (by definition) not about anything that could directly or indirectly be observed. The use of Occam's razor to decide between empirically equivalent theories is very different from its use in scientific contexts, where it has been used to decide between theories that disagree on some prediction or retrodiction.

The question now becomes: is Occam's razor relevant to deciding which of two empirically equivalent theories are true? There are some reasons for thinking that it is not. Forster and Sober (1994) and Sober (1996; 2002), for instance, have argued that

Akaike's theorem[17] shows that families of equations with smaller number of adjustable parameters (i.e., "simpler theories") are likely to be more predictively accurate than families of equations that have larger number of adjustable parameters in the case that *such theories fit the data obtained up to that point equally well*. This provides an explanation for why parsimony is relevant to choosing between theories that are not empirically equivalent—more parsimonious theories will generally yield more accurate predictions. Sober argues that this explains *why* it was reasonable to prefer Copernicus's theory to Ptolemy's, even though each theory fit all the data that was available at the time Copernicus proposed his theory. This explanation importantly does not apply to empirically equivalent theories. Sober concludes that "a difference in simplicity between predictively equivalent theories counts as a merely aesthetic or pragmatic consideration; it is not a ground for thinking that one theory is true and the other is false" (1996, 191). More recently, Huemer (2009) has surveyed a variety of other possible justifications for scientific appeals to parsimony, and has concluded that none provide any reason for thinking that parsimony considerations are relevant to determining the truth of metaphysical claims[18].

If the arguments above are correct, then Occam's razor is ill-suited to cut reductionist mustard. Even if these arguments fail to convince one of this relatively strong conclusion, however, there are two things to keep in mind. The first is that, even if metaphysical appeals to parsimony are cogent, this cogency cannot be explained by reference

[17]Sober talks about the simplicity of *families* of curves, where a family is specified by an equation with adjustable parameters. So, $y = a + bx$ specifies a family of straight lines, with a and b as the adjustable parameters the fixing of which will yield the specific straight lines that are members of the family. Sober states the theorem as follows: "An unbiased estimate of the predictive inaccuracy of family F, given data set D, is provided by the quantity SOS[Best-fit(F, D)] + $2k\alpha^2$ + constant" (1996, 173) where Best-fit(F,D) denotes the member of the family F that best fits data set D and SOS(C,D) denotes the sum-of-squares that curve C has with respect to data set D. k denotes the number of adjustable parameters; thus, increasing the number of parameters (increasing "complexity") will lower the predictive accuracy of the family (at least in the case that SOS is constant).

[18]Huemer favors a likelihood-based account of parsimony, according to which "complex models typically have lower likelihoods relative to a given set of data, because complex models have more parameters which can be used to accommodate the data" (2009, 231). Huemer argues that parsimony so understood is irrelevant to resolving debates such as those between physicalism and dualism or that between nominalism and realism; this is because the relevant theories do not differ on likelihoods. His reasoning can presumably be exported to the debate between RL and nomic primitivism.

to the (legitimate) uses of parsimony in other contexts. Occam's razor, insofar as it is a recognizable principle of empirical science, simply does not apply to metaphysical debates. One can, contra Schaffer, recognize that Occam's razor is a "sound methodological principle" without conceding that it supports RL. Second, there are some purely pragmatic reasons to be wary of the use Occam's razor in philosophical contexts. In particular, it seems that a wide variety of implausible positions—phenomenalism, instrumentalism, and behaviorism among them—might similarly be supported by appeals to parsimony. Moreover, depending on how these positions were characterized, scientists (or their philosophical defenders) might not be capable of pointing to the particular "countervailing considerations" that would allow them to justifiably reject such positions. Nevertheless, it would be inappropriate to characterize those refusing to adopt these positions as being "anti-naturalist." Realism about the physical world, about unobservable entities, and about dispositions are paradigmatically naturalistic positions; we ought to be wary of arguments that would make it seem otherwise. The point here is that Occam's razor must be applied with care, and especially so in contexts where we are purporting to show that scientific practice ought to be revised. This is so even if we are unable to point out which *precise* aspect of scientific practice should count as a "countervailing consideration."

For the reasons presented previously, I do not think that any "sound methodological principles"—and in particular, Occam's razor—support reductionism over primitivism. So, Schaffer's argument in favor of reductionism fails. This does not entail, however, that there are any methodological considerations in favor of primitivism. While I will not pursue the project of presenting such positive considerations in any depth, it is worth briefly noting two aspects of scientific practice that plausibly favor primitivism over reductionism: The acceptance of certain explanations over others and the sanctioning of certain inductive practices.

The first aspect of scientific practice that suggests a commitment to nomic primitivism is the acceptance and rejection of certain sorts of *explanations*[19]. To see why this

[19]That reductionist accounts laws are incompatible with the laws being explanatory has been noticed by a number of different thinkers, and has been argued for in a wide variety of ways. Armstrong (1983, 40) has contended that, insofar as reductionist "laws" are partially constituted by their instances, they cannot then be used to explain these same instances. So, if laws are to be explanatory, RL must be

is, consider U_3 and U_4, two of Carroll's mirror worlds discussed previously. In each of these worlds, a mirror has been positioned to deflect a particular X-particle (which we'll call X_5) from entering a Y-field. In both worlds, X_5 currently has and always will have spin down. Particles X_1 through X_4, by contrast, acquire spin up when passing through their respective Y-fields. Since U_3 was obtained by rotating a mirror in U_1, it is a law in U_3 that *X-particles entering Y-fields acquire spin up*. In U_4, which was obtained from U_2, this is not a law. U_3 and U_4 are completely identical in terms of history; their only area of divergence concerns which laws hold.

With this setup in mind, consider the following explanandum: *There is a difference in spin between X_5 and the other X-particles.* If reductionism were true, then the explanation for this should be the same in both U_3 and U_4. But, on reflection, we can see that this is not true. In U_3, we can explain our explanandum by citing explanans such as *X_5, unlike X_1 to X_4, never entered a Y-field* or *there is a mirror that prevented X_5 from entering a Y-field*. In U_4, by contrast, these sorts of explanations fail. This is because, in U_4 (unlike in U_3), the position of the mirror makes no difference to the spin that X_5 has—X_5 would have had a different spin than then the other X-particles regardless of how the mirror had been positioned. In fact, it is unclear that we will be able to find *any* satisfactory explanans for this explanandum in U_4; instead, the differences between the X-particles seem to be a matter of brute, unexplainable fact. It is important that this difference in correct explanations does not (and cannot) hinge on any merely epistemological issues—each world is entirely barren except for these Y-fields and X-particles and neither have any laws except those specified. In short, we (the explainers) know everything there is to know about both worlds. It is not open for us to claim, for instance, that U_4 has a probabilistic law governing X-field and Y-field interactions, and that X_5 is merely one of the rare exceptions. Rather, which explanans is appropriate depends crucially on what the laws are (and, correspondingly, on which counterfactuals are correct), and this sort of difference is not something that is compatible with reductionism.

A second aspect of scientific practice plausibly linked to belief in primitivist laws

false. Bird (2007, 86-90) offers a structurally similar criticism, and argues that reductionist laws lack the "ontological content" that would allow them to fulfill their explanatory role. Ward (2002; 2007), on the other hand, has rejected Humean Supervenience on the grounds that it cannot account for the role that certain *counterfactuals* play in offering and assessing law-based explanations.

involves the sanctioning of certain *inductive practices* and the rejection of others. By 'inductive practices,' I mean (roughly) the practice of forming and justifying beliefs about cases that we have not observed using evidence gleaned from cases we have observed. In particular, it seems plausible that the confirmation of certain sorts of universal generalizations makes sense only against the background belief that these generalizations might hold because of primitivist laws.

To see why this sort of relation between induction and primitivism might hold, consider again the case of U_2. By stipulation, it is neither true nor a law at this world that *X-particles entering Y-fields acquire spin up*. On this, both the primitivist and reductionist can agree. Both groups can also agree that the (false) proposition *the mirror is positioned to block the fifth X-particle from entering a Y-field* is physically possible. They disagree, however, on whether there would be a new law concerning Y-fields and X-particles, were the position of the mirror to change in this physically possible way. According to the reductionist (but not the primitivist), changing the position of the mirror results in a change in laws[20]. The disagreement about this particular case reflects a deep-rooted disagreement about the fundamental relation between laws and non-nomic facts. The primitivist claims that which laws hold is independent of which physical contingencies happen to be realized; the reductionist claims that this is false. In a certain intuitive sense, then, the reductionist (but not the primitivist) holds that the laws are *coincidences*. The reductionist holds that the laws could have been different (and perhaps much different) than they actually are, had certain physical contingencies come to pass.

Now, consider the case of a scientist who inhabits a world with a great number (perhaps even an infinite number) of X-particles and Y-fields. She has observed thousands of interactions between X-particles and Y-fields, and in a wide variety of circumstances. In each case, the X-particle has acquired spin up when entering a Y-field. She now considers whether it is rational to project this observed regularity to all unobserved cases. If the scientist is a reductionist, this sort of projection seems unwarranted, since there are an infinite number of X-particle/Y-field interactions she has not observed yet and hence, an infinite number of hypotheses concerning X-particle/Y-field interactions that she

[20]To be more precise, the reductionist is committed to the claim that rotating the mirrors in both U_1 and U_2 will result in a change in at least one of these world's laws.

must rule out. Moreover, it would be unreasonable for her (as a reductionist) to favor any of these hypotheses over others. For instance, she has no apparent reason to assign non-zero prior probabilities to propositions such as *it is a law that X-particles entering Y-fields acquire spin up*; after all, laws themselves are (on the reductionist view) merely a variety of coincidence. If our scientist is a primitivist, however, this sort of favoritism toward hypotheses about potential laws (and the universal generalizations they entail) might well be justified. Primitivist laws, unlike reductionist laws, are not merely coincidences, and it may make good sense to treat hypotheses about potential laws differently when considering how to project observed regularities onto unobserved instances. The scientist who wishes to project an observed regularity onto all actual unobserved cases would thus be well-advised to adopt primitivism.

Tooley (1987), Woodward (1992), and Lange (1997; 1999; 2000) have offered detailed defenses of the thesis that our actual inductive practices require primitivism, and have shown how this sort of inductive practice can be represented using Bayesian terminology. While these accounts differ on the details of presentation, Lange's is both simple and representative. He argues that, in cases where we have no relevant background evidence about the reason that all observed Fs are Gs, we cannot justify the "projection" of this regularity onto any *particular type* of unobserved F, since any choice would be arbitrary. In such a case, Lange argues that scientists have often treated the projection to (something like) *all physically possible Fs* as a sort of inductive "free move"[21] So, for example, consider Newton's confirmation of the claim "All massive bodies are subject to gravitational force." Newton could observe only a tiny subset of massive bodies and he had little evidence regarding the influence that other properties played in mediating the relation between mass and gravitational force. On Lange's account, given such (relatively impoverished) background evidence, Newton was justified in projecting his hypothesized relationship between mass and gravitational force *to all physically possible massive bodies* and hence (by logical implication) to all actually existing massive bodies. Lange claims that the prevalence of these sorts of inductive strategies accounts for the perceived importance that discovering laws play in scientific practice—in many cases,

[21] In Bayesian terms, they are "free" to assign this hypothesis a non-zero prior probability (and to forgo distributing prior probabilities evenly among the infinite hypotheses).

confirming a law is the most efficient means of confirming the corresponding universal generalization.[22].

The intuitive idea behind these strategies is that it is rational to project potentially law-governed regularities to unobserved cases because these regularities will hold regardless of the physically contingent features of these unobserved cases. In short, law-based induction does not require that we know the details about each and every case that we project observed regularities onto. According to nomic primitivism, this sort of practice makes sense, since no physically possible transformation can alter the laws; the laws hold *no matter what*. According to RL, by contrast, the laws are not invariant in this way; we can, by a series of physically possible transformations, produce massive violations of the laws, and reach worlds far different from our own. This is because RL allows for certain physically possible transformations (e.g., the changing of a position of a mirror, the removal of all massive objects from the world, or the population of a world with 1,000,000,000 gold spheres with diameters of a bit less than a mile) to produce worlds with far different laws than the actual world. So, if we attempted to project a regularity across "all physically possible changes" this may (in effect) entail projecting this regularity to all *logically possible* cases, which would obviously be absurd. This feature of reductionist views should not be surprising upon reflection, however; after all, such views hold that it is the regularities that are primitive, and that it is the laws that supervene upon these. Changes in particular matters of fact can thus easily lead to

[22]A number of writers (Kneale, 1949; Dretske, 1977; Goodman, 1983) have suggested that one cannot simultaneously, e.g., (1) believe that an oft-observed regularity between properties F and G is not physically necessary and (2) take these past observations to confirm the hypothesis that the *next* F observed will be G. This seems to me obviously false, even when one considers the examples that are usually given. Suppose, for instance, that one finds oneself in a hotel with one thousand men. After talking to 50 of the 1000, you find that each has a handle bar mustache. It seems perfectly reasonable, given this evidence, to have considerable confidence that (1) the next person one talks to will have a handle bar mustache and (2) that this regularity can be partially explained by citing some accidental truth (e.g., the hotel is hosting a mustache contest). Carroll (1994, 98-99) argues that the more plausible conclusion to draw from such cases might be that, in cases where one has little background evidence, one cannot consistently believe that (1) a given induction is justified and (2) there is *no* probability of a non-accidental connection between one's evidence and the cases about which one is making predictions. Woodward suggests something similar, and argues "[which] uniformities the observer is warranted to infer to (which features of his local situation he is entitled to extrapolate) will depend very much on he takes the nomological facts responsible for those uniformities to look like" (1992, 182). The comments of both seem roughly consonant with Lange's more elaborate defense (see above).

massive changes in laws.

3.3.3 Response 3: Conflicts with the Grounding Intuition

In this section, I will consider what I take to be a common (though often implicit) motivation for defending RL—namely, that many find it "counterintuitive" to suppose that modal facts (of which the nomic facts are a subset) could "float free" of the non-modal facts. I will call the intuition that modality must be grounded in this way the *grounding intuition*. If it could be shown that (1) the grounding intuition contradicted the intuitions appealed to in the PFA and (2) the grounding intuition was at least as important to scientific practice as these intuitions, one could reasonably conclude that the PFA fails.

To begin, it will help to get clear about what I mean by the 'grounding intuition.' The grounding intuition is, at the most general level, the intuition that facts about the way things *could be* (e.g., which events are metaphysically or physically possible) or the way things *would be* (e.g., which counterfactuals are true) must be grounded in the ways things *actually are* (e.g., in the instantiation of non-dispositional properties by spatially-located concrete individuals). Sider (2001, 36) notes two distinct ways that this intuition might be made precise: (1) by a principle stating that all truths have truth-makers or (2) by a principle stating that truth supervenes on being. In whatever form it takes, this principle often seems to serve as something like a reductionist side-constraint on satisfactory analyses of laws of nature, chance, counterfactual truth, and other nomic and modal entities and concepts. So, for example, Lewis requires (without argument) that any satisfactory analysis of chance must show how truths about chance "supervene on being" (1994, 484). Armstrong (2004, 5), by contrast, begins his *Truth and Truth-makers* with the (stronger) hypothesis that all truths have truthmakers, and that such truthmakers must *necessitate* the relevant truths.

For the sake of clarity, and in order to clearly capture the requirement that primitive counterfactuals are disallowed, I will express the grounding intuition as follows:

GI : The modal facts reduce to the non-modal facts.

This is closely related both to Schaffer's claim that "modal existents reduce to occurrent

existents" and to Sider's claims that primitive counterfactuals or dispositions violates the intuitive "no-cheaters" requirement that can be gleaned from the principle that truth supervenes on being. It also clearly entails that nomic primitivism is false since (1) nomic primitivism holds that the nomic facts cannot be reduced to the non-nomic facts and (2) the nomic facts are a subset of the modal facts[23].

In this section, I will be considering the following argument (the "Grounding Argument"):

GA1. Competent users of the concept *law of nature* judge that the modal facts reduce to the non-modal ones. That is, they judge that GI is true.

GA2. So, the modal facts reduce to the non-modal facts.

GA3. So, the facts about laws (a subset of modal facts) reduce to the non-modal facts (i.e., to history).

This argument is modeled after the PFA, except that the judgment that "it is possible for the laws to differ without the history being different" is replaced by the judgment that "the modal facts reduce to the non-modal ones." If both GA1 and PF1 are true, the judgments of competent users of the concept *law of nature* are inconsistent. They at once judge that it is possible for the laws to differ without history differing while also holding that the totality of non-modal facts (i.e., the history) determines what all the modal facts are. If this is the case, then at most one of GA2 or PF3 is true. This would present a significant objection to the PFA, since there is no obvious criterion for choosing between which of two equally strong but inconsistent intuitions a philosophical analysis ought to capture.

I think, however, that there is little reason to think that GA1 is either true or will be judged to be so by the relevant community. As I have presented it, PF1 is backed up by appeal to judgments concerning particular cases (such as that presented by Carroll's mirror thought experiment) about which there seems to be general agreement. The question is whether GA1 can be given a similar defense. This task is complicated by the fact that explicit defenses or explanations of GI (or similar principles) are rare. Schaffer,

[23]While it is difficult to formulate precise definitions of *modal existent* or *modal fact*, I take it that the basic idea is clear. They are just those facts that Schaffer wishes to exclude from his notion of "history."

for example, offers three "motivations" for asserting that the modal entities must be reducible to the occurrent ones. Insofar as he goes on to use this assertion as a premise in an argument intended to show that laws are reducible to history, I think it is reasonable to evaluate whether any or all of these motivations could serve as premises in a cogent argument for GI. The three motivations are as follows:

1. It is "intrinsically plausible."

2. It is consistent with a "plausible" principle of possibility—the principle of Humean recombination.

3. It can be used to rule out views that are "widely regarded as implausible." (Schaffer, 2008, 86)

I will consider whether any or all of these propositions might serve as a plausible premise in an argument for the GI.

The first claim—that GI is intrinsically plausible—strikes me as either blatantly false or irrelevant. It is blatantly false if intended as a description of the intuitions of non-philosophers. GI, in almost all of its formulations, contains a variety of technical vocabulary intended to pick out concepts foreign to both scientific practice and ordinary language use—'modal', 'nomic', 'occurrent,' and so on. Because of this, it is unlikely that most competent users of the law concept could even understand the principle without considerable exposition. And it is unclear how, if at all, this exposition could be presented in any way that did not result in a theoretical bias[24]. So, the claim cannot be that most competent users of the concept of law find the general principle intuitive. Conversely, if the claim is that those who do understand it (such as analytic metaphysicians) find it to be intuitive, then it is both contentious and irrelevant. It is plausibly true that reductionists about laws find it intuitive and might even (as a contingent matter of fact) base their reductionism about laws on this intuition. However, no prominent anti-reductionist has claimed to find this principle intuitive. This contrast suggests (though

[24]This is, again, an empirical claim. However, I take it to be a relatively modest one. The claim is not that GI is counter-intuitive, but merely that its very status as a technical claim about analytic metaphysics renders it the type of principle that people are unlikely to be able to understand.

certainly does not prove) that having or lacking the intuition that GI is true is a matter of prior theoretical commitments. The problem with this, at least from the reductionist standpoint, is that the intuitions appealed to in the PFA do not appear to have produced this contrast in reported intuitions. It is generally agreed by all participants in the debate that Carroll's mirror argument, for example, produces fairly uniform responses. For these reasons, I think that Schaffer's first "motivation" cannot serve as strong evidence for the truth of GI.

It is worth taking a moment to emphasize the difference between GI, which says that there are no primitive modal properties, from the (intuitive and correct) view that we ought not posit primitive modal properties without some good reason. So, to take a classic example, it is obviously unwarranted to explain opium's dispositional capacity "to put people to sleep" by positing the existence of a primitive *dormative power* that exists just to ground this disposition. Instead, we explain opium's effects by reference to its chemical properties and the way they interact with human biochemistry. In short, we offer a mechanical explanation of this modally-loaded disposition in terms of a number of more basic (modal and non-modal) facts. This sort of picture is far removed from what the advocates of GI are talking about. The question here, for both Sider and Schaffer, concerns the modal or non-modal nature of the *primitive* facts of our universe—i.e., those facts whose obtaining does not depend on any other facts' obtaining. The very fundamental nature of these facts, whether or non-modal, will prevent us (even in principle) from offering these sorts of mechanical explanations; thus, our tendency to offer explanations for garden-variety modal facts is simply irrelevant. A case that is closer to what we are talking about might be the task of explaining low-level dispositional facts such as the capacity of positively charged particles to attract negatively charged particles or the tendency of protons in a nucleus to adhere together. In these cases, it seems much more tempting to offer either modal explanation ("It is a law" or "That's just the nature of those particles") or to offer no explanation at all ("That's just the way it is"). All of these explanations violate GI.

Even if the GI is not itself intuitive, we may have other, related, intuitions that are best explained by a tacit commitment to GI. Schaffer's final two motivations provide potential examples of these sorts of intuitions. First, he claims that Lewis's (1986)

principle of "Humean recombination" captures an intuitively plausible view of what is possible. This principle, according to Lewis, holds that it is possible for "anything to exist with anything else, at least provided they occupy distinct spatio-temporal positions". Schaffer claims that views violating this principle "entail implausible limits on recombination" (2008, 86)[25]. Again, there are several things wrong with this defense. First, as Wilson (2005, 441-442) points out, it is implausible to claim that this principle accurately describes the way scientists talk about possibility—they are likely to deny the possibility of many things countenanced by this principle, e.g., the possibility of worlds where disembodied heads can talk for hours after being separated from their bodies[26]. Second, the principle of Humean recombination is perfectly compatible with numerous views on which reductionism is false. For example, the Dretske-Tooley-Armstrong view of laws holds that laws are constituted by "necessitation" relations between universals. These relations imply no "implausible limitations" on which properties can co-exist with which other properties (though they do entail limitations on which laws can coexist with which regularities). Finally, the principle of Humean recombination explicitly endorsed by Schaffer and Lewis is very likely incompatible with our best physical theory. Some interpretations of QM hold that the properties of spatio-temporally separated but entangled particles cannot be reduced to the properties instantiated by the relevant particles. So, contra Lewis's principle of recombination, science may require that we posit primitive properties or relations that cannot be instantiated at single space-time points.

Both Sider (2001, 39-41) and Armstrong (2004, 1-4) offer arguments for GI that closely resemble Schaffer's third motivation—that it can be used to rule out implausible metaphysical views. Sider does so in the context of formulating an argument against

[25] While Schaffer does not give an example, law necessitarianism is a frequent target of such criticisms. According to law-necessitarianism, no property that is law-governed can be instantiated in any world where this law does not hold. For example, on the law-necessitarian view, it is (metaphysically) impossible for electrons to exist in a world where all particles are positively charged (since it is a law of nature in the actual world that electrons are negatively charged). On Lewis's view, by contrast, it is (metaphysically) possible for electrons to be positively charged, to be as large as elephants, or perhaps even to be sentient.

[26] As Wilson notes, the principle of Humean recombination is incompatible with the thesis that the laws are metaphysically necessary. I concur with Wilson in her judgment that the principle of Humean recombination may be too strong. However, as I argue in Chapter 4, law necessitarianism also fails to track our intuitions about what is possible.

presentism, which is (roughly) the thesis that the only facts are those that obtain at the present time. Specifically, he wants to contend that it is illicit to posit primitive truths of the form *there used to exist dinosaurs* to serve as the "grounds" for sentences of the form 'There used to be dinosaurs.' Sider appeals to a form of GI as a *premise* in this argument, and then offers the following explanation of why he feels it is appropriate to appeal to GI:

> The point of the truth maker principle and the principle that truth supervenes on being is to rule out dubious ontologies. Let us consider some. First, brute dispositions. Many would insist that the fragility of a wine glass—its disposition to shatter if dropped—must be grounded in the non-dispositional properties of the glass, plus perhaps the laws of nature. It would be illegitimate to claim that the glass's disposition to shatter is completely brute or unfounded. Second example: brute counterfactuals. Most would say that when a counterfactual is true, for example 'this match would light if struck,' its truth must be grounded in the actual occurrent properties of the match and its surrounding. (2001, 40)

He goes on to list other examples, including the view that it is a law that Fs are Gs just in case every object in the world has a brute property such that all Fs are lawfully Gs. After offering these examples, he offers the succinct argument that presentism is false because it "seems of a kind with the dubious ontological cheats of the previous paragraph" (2001, 41). Schaffer offers a similar argument in defense of GI, noting that GI is "theoretically useful, in ruling out certain metaphysical views that are now widely regarded as implausible" (2008, 86). Schaffer, Sider, and Armstrong all use the implausibility of certain views that violate GI as *evidence* that GI is true.

This argument is most plausibly interpreted as an inference to the best explanation. It is claimed (as a premise) that a number of "implausible" metaphysical views violate GI and it is concluded that the best explanation for these views being false is that they violate GI. One might alternatively conclude (in a manner more congenial to GA 1) that the best explanation for why people find these views counterintuitive is that they are implicitly committed to GI. The plausibility of this argument might in part be attributed

to the structure it shares with arguments concerned with scientific methodology. So, for example, consider the issue of parsimony discussed earlier. Many scientific realists would contend that simpler, more ontologically conservative theories are more likely to be true than complex, more ontologically extravagant ones in the case that the theories agree on extant evidence. This judgment might in turn be supported by appeal to the history of science—i.e., theories that have posited complex supernatural explanations for events have, in general, produced poorer empirical results than their simpler, non-supernatural competitors. Scientific realists can thus claim that the history of science provides evidence that simplicity is relevant to truth.

The success or failure of this toy argument on behalf of scientific realists goes beyond the scope of this project. The important thing for our purposes lies in the differences between this argument and the argument for GI. The data appealed to the by the scientific realist concerns the rejection or acceptance of scientific theories by particular communities of researchers, often on the basis of specifiable experimental successes or failures. If a significant portion of those working in the relevant areas continued to favor complex, ontologically extravagant theories then this argument would have little or no force. The data appealed to by the reductionist, in contrast, concerns the fact that certain metaphysical hypotheses are implausible. Just as was the case with the claim that GI is plausible, the relevant question here is: Implausible to whom? Again, it seems unlikely that non-philosophers have any particular opinion concerning the plausibility or implausibility of the specific hypotheses in question. Furthermore, it seems contentious to claim that a majority of those who do understand the relevant hypotheses find them implausible. Many of the hypotheses that Sider picks out as "cheaters"—i.e., the existence of primitive dispositions, the truth of ungrounded counterfactuals, presentism—have prominent contemporary defenders. If the claim is, by contrast, simply that some philosophers find such views implausible, then there is no reason to suppose that this has any evidential bearing on whether GI is true.

This concludes my discussion of reductionist attempts to answer the PFA by attacking the "intuitions" behind it. In particular, I have argued that these intuitions cannot be ignored on the grounds that they have a "suspicious source," that they do not conflict with naturalism, and that reductionists have not shown that there is a competing intu-

ition of the relevant type. I also have tried to show that the debate between primitivism and reductionism is not simply a matter of different people having different intuitions. The intuitions appealed to in PFA are evidentially relevant in virtue of the fact that (1) they are widely reported and (2) there are reasons to think the judgments reflected play some role in scientific practice. The intuitions appealed to by reductionists, by contrast, are held only by a small group of philosophers and play no evident role in scientific practice. They are thus not evidentially relevant.

3.4 UNDERDETERMINATION ARGUMENTS FOR RL

In the previous section, I discussed reductionist responses that purported to show that our intuitions, once they have been fully spelled out, do not support primitivism in the way that the PFA contends that they do. I argued that all of these responses failed, and that we thus have some reason to believe that primitivism is true. In this section, I will consider a different line of response. This line of response contends that, whatever our current intuitions happen to be, we have very good reason to adopt a revised (and reductionist) concept of *law of nature*. In particular, some have argued that we can have no epistemic access to primitive laws and thus, that theories positing them are open to a particularly vicious form of underdetermination argument.

The purported problem arises from primitivism's commitment to the possibility of pairs of worlds with the same non-nomic facts but different laws. Some have argued that these possibilities are incompatible with us having any knowledge of what the laws are. A simple argument for reductionism can now be formulated: since it is possible for us to have knowledge of the laws of nature, and since primitivism is incompatible with the possibility of such knowledge, primitivism must be false. The most recent and sophisticated version of this argument is presented in Earman and Roberts (2005b) and Roberts (2008). In this section, I will consider in some detail the ramifications of this argument for the primitivist/reductionist debate, and will argue that primitivism remains a viable thesis.

3.4.1 Earman and Roberts's Argument Against Primitivism

Earman and Roberts offer an underdetermination argument in defense of their favored version of HS (which I earlier called HSM). If HSM entails RL (as I previously argued that it did), then this can also be taken as an argument for RL.

The argument's structure is fairly straightforward. Earman and Roberts begin by defining *nomic realism* to be the conjunction of two claims: (1) the laws of nature are made true by mind-independent, objective facts (*semantic realism*) and (2) it is possible to have knowledge of the laws of nature (*epistemic realism*). Earman and Robert's argument purports to show that nomic realism entails RL. To do so, they offer a specialized variety of underdetermination argument: Specifically, they argue that *if RL is false and semantic realism is true, then claims about laws are underdetermined by all possible evidence and hence, epistemic realism is false.* Since I am committed to the truth of nomic realism, this result is obviously unwelcome. In what follows, I will lay out the underdetermination argument in a bit more detail and will consider some possible responses to it.

Earman and Roberts formulate their underdetermination argument using two theories, T and T*, that are defined as follows:

T: L is a law of nature and X.

T*: L is true but not a law and X.

Here, L and X are arbitrary propositions; the theories disagree *only* on the truth of L. To see how this might work, it may help to consider specific, concrete instances of L and X. Suppose that L is $G = 6.74 \times\ 10^{-11}\ N(m/kg)^2$ and that X consists of the other nomic and non-nomic propositions that make up standard Newtonian mechanics (though excluding anything that implies the lawhood or non-lawhood of L). So, for example, X might include nomic propositions such as *it is a law that* $F = MA$ and non-nomic propositions such as *the acceleration due to gravity near the earth's surface is around* $9.8\ m/sec^2$. So defined, T and T* would disagree on precisely one thing—whether or not it was a law of nature that the gravitational constant has the value that it does. T* claims that L's truth is physically contingent on *something*, though it doesn't specify on what (since this physical contingency can't be a consequence of anything in X); T claims

that L is an underived, primitive law (if it were a derived law, its lawhood would have to be a consequence of something in X, which is explicitly forbidden).

Once they have defined T and T* Earman and Roberts go on to present the following argument:

ER1. If HSM is false, then no empirical evidence can favor T or T* over the other.

ER2. If semantic realism about laws is true, and no empirical evidence can favor T or T* over the other, then we cannot be epistemically justified on empirical grounds in believing that T is true.

ER3. So, if nomic realism (the conjunction of semantic realism and epistemic realism) is true, so is HSM.

Earman and Roberts (2005b) contend that this argument supports HSM on the grounds that nomic realism is both independently plausible and widely shared by their primitivist opponents; because he eventually concludes that orthodox reductionism (and hence HSM) is untenable, Roberts (2008) concludes that nomic realism is false. Since I wish to assert both the objectivity of laws and the falsity of reductionism, neither of these options is available to me. So, I need to find a way to split the horns of this dilemma.

Before considering the possibility of a satisfactory primitivist response, it is worth noting one caveat, which concerns the purported conclusion of the above argument. Earman and Roberts explicitly disavow having established (1) something that is true about our *current* concept of law or (2) something "deep" about the structure of the universe. Instead, they contend that their underdetermination argument shows that HSM ought to be adopted as a constraint on scientific and philosophical theorizing, on the grounds that "one of the valuable things about the activity of science, which we ought to strive to preserve, is the commitment to taking only worthy epistemic risks" (2005b, 83). It is precisely because they think theories violating HSM take *pointless* epistemic risks that Earman and Roberts think they ought to be rejected. They thus advocate the adoption of HSM as a sort of philosophical "stance," even while recognizing that it may not be an accurate description of the way that the concept *law of nature* is

currently used[27]. I have little wish to engage Earman and Roberts on the (presumably far-reaching) pragmatic advantages or disadvantages of adopting a novel concept of law; with this in mind, I will consider whether their argument shows that it is in some strong sense *irrational* to hold onto a primitivist conception of law, on the assumption that this is the concept currently in use. My argument will be that it is not irrational, or at least is no more irrational than it is to hold onto realism about the external world, theoretical entities, or other minds in the face of structurally similar underdetermination arguments.

A number of authors (Woodward, 1992; Carroll, 1994; Schaffer, 2005) have claimed that epistemic arguments for RL can (with minor modification) be used to establish far more implausible forms of skepticism (e.g., external world skepticism, skepticism about induction). Insofar as one holds that these varieties of skepticism are untenable, this provides a strong reason to reject the reductionist argument. This remains a powerful rebuttal to the reductionist even in the absence of an explanation as to what in particular is wrong with these stronger varieties of skepticism; after all, it effectively shows that belief in primitivism about laws is no more problematic than many other types of beliefs. Earman and Roberts, who are well aware of this potential response, offer a number of reasons for thinking that their formulation of the argument avoids the pitfalls shared by the more problematic versions of underdetermination arguments. I will contend that these reasons are inadequate, and that we can reject the underdetermination argument for RL for the same sorts of reasons we reject these other (obviously uncogent) underdetermination arguments.

3.4.2 Reply to Earman and Roberts

My reply will consist of two parts. First, I will construct an underdetermination argument for an implausibly strong conclusion, which I will call *Strong Humean Supervenience* (SHS). Second, I will argue that, if Earman and Roberts's underdetermination

[27]Roberts, in a slightly different mood, suggests that the predicate 'law of nature' isn't sufficiently precise to settle most of the difficult issues and argues that "we face a choice: not a choice between competing theories about what the objective matter of fact about the nature of laws of nature is, but a choice about how we are going to use the term 'law of nature' (2008, 140).

argument for RL is cogent, then so is the argument for SHS. On this basis, I will conclude that we ought to reject the underdetermination argument for RL.

Let *Strong Humean Supervenience* (SHS) be the thesis that the laws reduce to the outcomes of *actual* (past, present, and future) measurements performed by the scientific community entertaining the underdetermination argument. We can now construct an argument for SHS that is parallel to Earman's and Robert's argument for HSM. We begin by constructing the following pairs of theories:

G : L is true, and X.

G* : L is false but is consistent with the outcomes of all actual measurements (past, present, and future), and X.

Again, L and X are simply propositions, where the truth of L represents the only area of divergence between the theories. L might be, for instance, a claim about some physical quantity that will never be measured or a universal generalization with consequences about events falling outside the light cone of the relevant community of measurers. G and G* are analogous to T and T* insofar as they are (1) logically incompatible but (2) empirically equivalent[28]. We can now offer the following argument for SHS:

SH1. If SHS is false, then no empirical evidence can favor G or G* over the other.

SH2. If semantic realism about laws is true, and no empirical evidence can favor G or G* over the other, then we cannot be epistemically justified on empirical grounds in believing that G is true.

SH3. So, if nomic realism is true, so is SHS.

[28]Roberts explicates the relevant sense of empirical equivalence as follows: "For any pair of mutually inconsistent theories T_1 and T_2, if every possible body of evidence that is consistent with T_1 is also consistent with T_2 and vice versa, then no body of empirical evidence can provide any epistemic justification for believing one of them rather than the other" (2008, 169). Roberts claims that T and T* are a pair of such theories; I claim that if this is so, so are G and G*. In both cases, of course, there is a sense in which some "possible" evidence might decide the issue. For T and T*, such evidence would be the result of measuring physically possible, though non-actual, events (such as Carroll's turning of the mirror.). For G and G*, such evidence would be the result of measuring actual, though by hypothesis not actually measured, events.

The immediate conclusion of this argument is that nomic realism requires SHS; so, to be a nomic realist, one must hold that the laws of nature supervene upon the outcomes of our actual measurements. This is precisely what Earman and Roberts's argument purported to show about HSM (and hence, about RL). However, since SHS entails that we cannot be justified in making claims about any quantity that we will never (as a matter of fact) measure, something has obviously gone wrong. Scientific theories regularly make claims about the values of quantities about which no observer has or will ever measure. Such claims may, for instance, concern the values of quantities falling outside of the light cone of any actually existing observer. Even worse, SHS entails that which laws hold will depend on which measurements we happen to carry out; after all, it is the act of measuring something that makes it "count" for determining what the laws are.

In what follows, I will assume both that SHS is false and that the argument just given for it is not cogent. With this in mind, I will survey several possible criticisms of this sort of underdetermination argument. Because of interests of space, I will not be arguing that any of these responses succeed in disarming the underdetermination argument; I will, however, argue that *if* we are convinced by any of these criticisms, we must also grant that they apply to the underdetermination argument for RL. In short, insofar as one is committed to the uncogency of my toy argument for SHS, one should also be committed to the uncogency of Earman and Roberts's underdetermination argument.

With this goal in mind, the question becomes whether reductionists can identify a fault with the underdetermination argument for SHS that is not *also* a fault with the underdetermination argument for RL. Both Earman and Roberts (2005b, 279-280) and Roberts (2008, 147-152) address the "objection from bad company" in some detail, and this will prove a good starting place. So, for example, they claim that one reason that the type of underdetermination arguments that are usually offered for phenomenalism, behaviorism, or instrumentalism are uncogent is because they place unreasonably severe constraints on what can count as *evidence*. Phenomenalists, for instance, require that evidence be limited to facts concerning sense data, behaviorists require that it be limited to non-mental facts, and instrumentalists that it consist only of facts concerning observable entities. The argument for RL, they contend, presumes no unreasonable

restrictions on what can count as evidence. After all, it allow for facts about entities that cannot be directly observed to count as evidence, so long as these facts are tied in nomically regular ways to possible means of measurement.

If the argument for SHS is premised on unreasonable restrictions on evidence and the argument for RL is not, then my argument here would fail—Earman and Roberts would have shown that one can reasonably hold that the argument for SHS is uncogent without thereby being committed to the uncogency of the argument for RL. However, there are good reasons for thinking that this simple response fails. First, while limitations on evidence clearly play significant roles in certain underdetermination arguments, it is not clear that they play such roles in the argument for SHS. The argument for SHS is consistent with our being realists about the external world and entities that we haven't yet observed. It simply prohibits our using evidence about things that have been measured to justify claims about *things that will never be measured.* This is precisely parallel to the sort of restriction that Earman and Roberts place on admissible evidence; they prohibit the use of evidence concerning *actual* things to justify claims about *physically possible but non-actual things.*

A related worry, and one that I will not pursue at length here, concerns the possibility that we can directly measure nomic facts. If this were so, then we would have a positive reason for thinking that the underdetermination argument for RL is uncogent, since it would unreasonably prohibit us from considering this sort of evidence. That we have access to this sort of evidence is not as implausible as it may seem. Most of us, after all, are not external-world skeptics; we hold that many observations provide us with non-inferential (i.e., direct) evidence concerning externally existing physical objects and the properties they instantiate. We are not, as the Cartesian skeptic would have it, stuck in our own heads. One could, for similar reasons, hold that we can have direct evidence concerning nomic matters such as *causes*. For example, consider the act of throwing a baseball and the evidence this provides to the thrower. One might reasonably hold that the thrower will have evidence not only that (1) there was a sense-datum of a baseball, but also that (2) a baseball exists (as an external object) and (3) she caused the baseball to move. In any case, insofar as it is reasonable to suppose that we can have direct evidence of externally existing physical objects at all, it also appears reasonable to suppose that

we can have direct evidence concerning the nomically loaded counterfactuals pertaining to such objects. So, our non-inferential evidence base includes not only propositions about the baseball's status as a mind-independent physical object but also propositions concerning its nomic properties (e.g., the proposition that *were I to reach out and touch the baseball, my hands would not go through it*).

A second common reason for rejecting underdetermination arguments is to note that one of the "underdetermining" theories is somehow "illegitimate" and thus, does not represent a viable theory. The most detailed version of this response is provided by Leplin and Laudan (1991). Leplin and Laudan argue that a wide range of underdetermination arguments can be dismissed on the grounds that the alternative "theories" used to generate these arguments are not really "theories" at all, but are in some strong sense mere notational rip-offs of legitimate theories. Leplin (2004, 121-123) expands upon this point, and argues that theories such as G* have components that are designed to be "unconfirmable", and for this reason do not represent genuine scientific theories. In G*'s case, this component seems to be something like "L is false of some case that will never be measured." Leplin concludes from this that theories such as G* do not represent legitimate rivals to theories like G, which do not make these sorts of claims[29].

This response can be used to defend HSM only if it can be shown that T*, unlike G*, *is* a legitimate theory. The best reason for thinking this might be so is that T*, unlike G*, makes no "grue"-some references to actual "measurements" or "observations." However, this appearance is deceptive. After all, one way of formulating T and T* is as follows:

[29]Leplin explains his point as follows:

> More generally, it is a reasonable constraint on theories that the thesis of underdetermination invokes as rivals that these be at least amenable to evidential support. A propositional structure that could not in principle be confirmed violates this constraint. Such a structure, crafted solely for logical consistency with the observational consequences of an existing theory, will not be entertained as an alternative to the existing theory because there is nothing to be done with it; it is rightly dismissed as dead in the water because its only possible support is derivative from some further, independent theory that would itself be the proper object of confirmation. Once theories are required to be defensible by ampliation, conditions such as confirmability in principle, explanatory power, and generality—conditions that standard forms of ampliative inference select for – become reasonable constraints on theoretical status. (2004, 121)

T: L is true at all physically possible worlds and X.

T*: L is false at some physically possible worlds but is consistent with the events in the actual world, and X.

This formulation is equivalent to the one provided by Earman and Roberts; however, the parallels between G* and T* are now much more apparent. Both theories are formulated to ensure that any differences they have from the "baseline" theories will be logically incapable of having any bearing on the likelihood that the theories have on any possible empirical evidence. In the case of G*, the (unspecified) exceptions to G are located in the realm of the actual but unobserved; in the case of T*, the (unspecified) exceptions to T are located in the realm of the physically possible but non-actual. If Leplin's response provides us with a reason to reject G*, then it provides us with an equally cogent reason to reject T*. A legitimate rival theory to T would be one that makes a concrete claim about some particular (actual or merely possible) event; T* does not do this.

It is worth noting that adopting Leplin's response here is consistent with the possibility of our confirming theories containing claims of the form *L is true but not a law*. After all, we take ourselves to have well-confirmed beliefs about a great number of such regularities. For example, we believe that is true (but not a law) that all the U.S. presidents elected before 2010 were male, that there are no spheres of gold more than a mile in diameter, and so on. The point is merely that we ought to reject theories that claim that some actually true generalizations fail in some physically possible worlds *without any specification of what types of worlds these are*. We can reject such theories for the same reasons that we can reject theories that posit mysterious, unexplained discontinuities between the types of actually existing things we will observe and the actually existing things we will not actually observe. In both cases, the problem is that we have no evident *reason* to constrain a theories' claims in such a manner. This is also consistent with the (logical truth) that a law can never be better confirmed than the generalization that it entails; it is simply that (in certain cases), it will be more probable that the generalization holds *and the law does as well* than that the generalization holds and the law does not.

A final reason for rejecting underdetermination arguments might be termed "respect for science"; in essence, the idea here is that the positions that many underdetermination arguments are purported to support—such as phenomenalism or behaviorism—could not be adopted without abandoning much of contemporary scientific practice. This is not, strictly speaking, an argument that anything in particular is wrong with standard underdetermination arguments; rather, it is a (perhaps merely pragmatic) reason for rejecting their conclusions.

Earman and Roberts appeal to something like this principle in rejecting the restriction of the Humean base to facts that it is physically possible for us (given our anatomy, spatiotemporal location, etc.) to observe. They reject this characterization of the Humean base on the ground that, given our ignorance of what is physically possible for us to measure, such a principle might unjustifiably constrain science from making claims about facts that would be possible for us to observe. They write: "Since we have no way of telling in advance which epistemic risks will in fact come to grief, it would not be possible to implement this principle, so it would be unreasonable to impose it" (2005b, 281). They thus conclude that, insofar as we have no good way of determining what is (and what is not) physically possible for us to measure, it is OK for scientists to take the "risk" of making claims about *every* actual case, so long as these risks are related in relevant ways to the scientist's making predictions that do risk conflict with experience.

I think that this response does, in fact, provide us with a principled reason for rejecting SHS, even in the face of an apparently cogent underdetermination argument for it. I think, however, that the same sort of response provides good reason to reject RL, even in the face of an apparently cogent underdetermination argument. Earman and Roberts reject positions such as SHS on the grounds that, given our actual epistemic situation, they place unreasonably strong constraints on scientific theorizing. So, for example, if we adopted SHS, we would have to somehow constrain our scientific theories so that they made claims only about things that would *actually* be observed, even though we have no obvious way of determining what will be, and what will not be, observed. It seems to me that the nomic primitivist can, in precisely the same manner, point out that we are imperfect judges of which of our theory's predictions risk conflict with the *actual*

world and which will turn out to have potential conflict only with *physically possible* situations. It would thus be unreasonable for us to constrain our theorizing to predictions concerning the actual world, as RL requires. Rather, scientific theories should be free to make claims about all physically possible situations, even if some of these claims turn out to concern non-actual situations. The epistemic risks countenanced by nomic primitivism are thus not "pointless" at all, but are rather part and parcel of science's project of using past evidence to make predictions about novel cases.

Even if the above response is correct, however, one may be left unsatisfied; after all, this response says nothing about *why* the underdetermination arguments for RL and SHS fail. Instead, we have merely noted that the conclusions are incompatible with the scientifically valuable practice of taking worthwhile epistemic "risks." I suspect, though cannot prove, that the uncogency of the underdetermination arguments for RL and SHS hinges on differences between the context in which the arguments are offered and the contexts in which we are being asked to adopt their conclusions. So, for example, consider the context in which the underdetermination argument for SHS occurs: we are asked to choose between two theories in a context where we are *guaranteed* that these theories will agree on all actual observations. In such a context it seems (at least to me) that we may very well be at an impasse and the underdetermination argument for SHS goes through. Problems occurs, however, when we attempt to export this conclusion to our *own* context, in which we have no good way of determining whether the divergences between a pair of theories concerns the results of *actual* (though perhaps future) observations or merely *possible* observations (which will never be carried out).

I think that the underdetermination argument for RL has something of this same character. The reader is asked to consider the prospects of two competing theories in a context where it is abundantly clear that the theories' differences are constrained to worlds other than the actual one. The problem is that our own epistemic situation is nothing like this; we have no clear way of telling whether or not a given prediction pertains to the actual world or merely to some physically possible one. The theories that we are asked to decide between, unlike T and T*, do not explicitly mark for us which of their disagreements concern mere counterfactuals, and which might make a difference to some future measurement. In any case, it would be bad inductive practice

to project our decisions in these highly idiosyncratic cases to *all* cases of theory choice.

3.5 CONCLUSION

If the argument of this chapter has been correct, then we have some good reasons to reject RL and accept nomic primitivism. In particular, I have argued that our ordinary concept of law is one on which it is possible for the laws to differ without history differing. The best evidence in favor of this is the wide-spread acceptance that the pairs of worlds described by Tooley, Carroll, and others are "intuitively" possible. I have also argued that we have no countervailing intuitions or commitments that might make us lean toward RL. Nomic primitivism does not commit us to a dubious theology and does not violate naturalism; moreover, the "grounding" principle that it may violate is not one that is either obviously true or generally accepted. Finally, I have argued that nomic primitivism incurs no *special* epistemic problems, and that the confirmation of laws might thus proceed analogously to how we confirm a wide variety of other claims that once seemed problematic.

I have, of course, provided no explicit account of how we *do* confirm laws, however. It seems to me that such an adequate account must involve several factors. First, it seems plausible that laws are confirmed against background evidence that itself contains a wide variety of irreducible modal and nomic claims—e.g., claims about singular causes, about the truths of various counterfactuals, and about what the other laws are or might be. Second, it seems that laws are the type of things that *cannot* (in some strong sense of the word) be broken. We can confirm that a generalization is a law, at least in some cases, by seeing whether it (or some closely related generalization) can be broken. To take a paradigmatic case: we believe that it is an accidental generalization that all spheres of gold are less than a mile in diameter. We might come to believe that this was a law, however, if repeated attempts to construct such spheres failed, despite adequate amounts of gold and plenty of technological expertise. Perhaps, for instance, we find that when gold is gathered in sufficient mass, it simply spontaneously turns to lead. Such events would undoubtedly lead for a search for alternate explanations; eventually, however, it seems plausible that we would conclude that we were incorrect about what the laws

were, and revise our beliefs accordingly. In any case, such issues go beyond the scope of this chapter, and represent a potential area of future research.

CHAPTER 4

THE LAWS ARE NOT METAPHYSICALLY NECESSARY

Any philosophical theory of laws of nature must account for the sense in which the laws are necessary. This sense of necessity, sometimes called "physical necessity" or "nomological necessity", can be roughly characterized as the type of necessity relevant to distinguishing the laws and their logical consequences from "accidental" regularities. For instance, it is physically necessary that there are no spheres of uranium more than a mile in diameter. This is because, according to the laws of nature, the critical mass of uranium would prevent it from being accumulated into such spheres (van Fraassen, 1989, 27). By contrast, it is not physically necessary (even if true) that there are no spheres of gold more than a mile in diameter.

The sense in which laws are necessary might also be clarified by considering the relation between which laws hold and which counterfactuals are true. For example, consider the (presumably counterfactual) supposition that there currently exists on some distant planet a technologically advanced alien civilization that has long been dedicated to the construction of more-than-one-mile-wide spheres of every naturally occurring element. Suppose also that we know that their technology has been sufficient for them to construct of elements such as iron and copper. Insofar as we believe that the proposition concerning uranium is physically necessary and the one concerning gold is not, it seems reasonable for us to believe that were such a society to exist, there would exist a gold sphere of more than a mile in diameter. Conversely, it does not seem plausible that there would exist such a sphere of uranium. Thus it is the law, and not the accidental generalization, that would continue to be true on this counterfactual supposition.

In the previous chapter, I argued that the facts about the laws of nature do not reduce

to non-nomic, non-modal facts concerning "history." If my argument there was correct, then the necessity of laws requires that laws are something more than mere descriptions or summaries of non-nomic facts. In this chapter, I will argue that it is metaphysically possible for laws of nature to be false and thus, that the physical necessity of laws is not identical to (and does not reduce to) metaphysical or logical necessity. My conclusion will be that the facts about the laws of nature and physical necessity are in some strong sense primitive. I will begin the chapter by offering some preliminary remarks aimed at clarifying the notion of *physical necessity* and that of a *reductive definition*. The remainder of this chapter will be dedicated to the consideration of *law necessitarianism*, the thesis that the laws of nature are *metaphysically necessary* and are thus true in every possible world.

4.1 THE CONCEPT OF PHYSICAL NECESSITY

In order to establish my claim that physical necessity is not a variety of metaphysical necessity, I will rely on certain assumptions about physical necessity. These assumptions include the following:

1. Some propositions would be physically necessary if true, and we are able to identify some such propositions.

2. Some propositions that describe widespread regularities would fail to be physically necessary even if true, and we are able to identify some such propositions.

3. Every logical consequence of a physically necessary proposition is itself physically necessary.

4. The truth of physically necessary propositions is preserved under a wider and/or more important range of counterfactual suppositions than that of physically contingent ones.

I will not offer extended arguments for any of these claims, but will provide some brief remarks aimed at motivating their provisional acceptance.

4.1.1 Assumptions about Physical Necessity

First, I will assume that paradigmatic examples of laws of nature (what I earlier called "strong laws") are physically necessary. These laws of nature plausibly include the core claims of a true, fundamental physical theory. For instance, the field equations of GR or the equations of QM would, if true, express physically necessary propositions. For the purposes of simplicity, I will sometimes assume that we live in a Newtonian world. In such a world, the laws of nature include at least Newton's three laws of motion and his law of gravitation. If a philosophical analysis of physical necessity entails that paradigmatic laws of nature are not physically necessary, it can be rejected for this reason.

Second, and for similar reasons, I will assume that we are correct in supposing that not every widespread regularity is physically necessary. Moreover, as the example of the gold spheres shows, a generalization can fail to be physically necessary even in the case that it does not pertain to (1) any particular times or places and (2) merely artificial or unnatural predicates or properties[1]. Any philosophical analysis of physical necessity that implies that paradigmatic accidental generalizations are physically necessary can thus also be rejected.

Third, I will follow philosophical convention in offering the following definition of physical necessity:

> (PN) Proposition P is *physically necessary* $=_{def}$ P is true at every world with the same laws of nature as the actual world.[2]

This definition of physical necessity entails that every logical consequence of a law of nature is physically necessary. It does not entail, however, that every physically neces-

[1] According to Hempel's (once widely-accepted account of laws, laws are true universal generalizations (i.e., sentences) that contain only certain sorts of predicates, and accidental generalizations are distinguished by the fact they contain other sorts of predicates. The gold sphere case is an example of an accidental generalization that Hempel's account incorrectly classifies as a law.

[2] 'Actual' should be read non-rigidly so that it doesn't always refer to our world; i.e., a proposition is physically necessary at an arbitrary world w just in case it holds in every world with the laws of w. One might alternatively take P to be physically necessary in w if and only if P is true at all worlds accessible from w. Here, y is *accessible* from w if and only if w and y have exactly the same laws. This definition will be equivalent to the one given, as Carroll (1994, 177) notes, though the wording incorrectly suggests that physical necessity is relational.

sary proposition can properly be called a 'law of nature.' Some philosophers have argue that a distinction ought to be maintained between the two terms. Marc Lange, for example, has argued that the laws can be distinguished from the other physical necessities in virtue of the way in which laws can be confirmed (2000, 201-210). PN also does not rule out the existence of multiple "grades" of physical possibility; in particular, it leaves open the possibility that some laws (such as "symmetry principles") will be true at a strictly larger range of worlds than other laws. While I think there is some plausibility to these claims, they are tangential to the focus of this chapter. For the purposes of simplicity, I will use 'law' or 'law of nature' to refer to any physically necessary proposition[3]

Fourth, I have assumed that there is *some* distinctive relation between physical necessity and counterfactual truth. I have left this requirement intentionally vague in order to avoid begging substantive questions concerning the precise relation between physical necessity and the evaluations of counterfactuals. However, I will assume that any account which entails that the laws of nature bear *no* distinctive relation to counterfactuals is false. I will generally speak of counterfactuals being "true" or "false," but nothing of substance will hinge on this—one might equally well speak of them being "correct" or "incorrect." In this same vein, I will say that P is *preserved* under the supposition S just in the case that "Were it the case that S, it would be the case that P" is true or correct. I will consider the relation between laws and counterfactuals in a bit more detail in the concluding chapter.

4.1.2 Reductive Definitions of Physical Necessity

In specifying the concept of physical necessity, I appealed freely to both laws of nature and to the truth of various counterfactuals. If the arguments of this chapter and

[3]There are certain awkward consequences to identifying all logical consequences of laws as being physically necessary, as I have here. In particular, this view entails that every logical or mathematical truth is physically necessary. One way of dealing with this would be to require that every law of nature be logically contingent. My suspicion is that any clause of this type risks leaving out certain legitimate laws of nature: in particular, it rules out the possibility that certain laws serve as stipulative definitions of the terms they contain and are thus analytically true. In any case, there is a plausible alternative explanation for our reluctance in most contexts to identify logical truths as laws of nature. If we were to claim that such truths were "laws of nature", we might be taken as (falsely) implying that they were *merely* physically necessary and not logically necessary.

the preceding chapter are correct, then this is to be expected— the facts about laws of nature (and the sense in which they are necessary) cannot be reduced to other types of facts. This thesis disagrees with *reductive accounts* of physical necessity that purport to offer definitions or analyses that do *not* appeal to such notions. The ultimate goal of these reductive projects has generally been to show that the facts about physical necessity are determined by other, more tractable, better-understood types of facts. A successful reductive account might, in theory, be able to solve such problems as (1) identifying what the "truth-makers" for laws of nature are, (2) showing how it is possible to have knowledge of laws, and (3) explaining why laws matter to scientific explanation.

While it may be impossible to formulate a precise rule for determining what ought to count as a "reductive account," I take it that each of the following toy accounts are reductive in the relevant sense:

1. L is a law of nature $=_{def}$ L is an informative, concise summary of the facts about our world.

2. L is a law of nature for agent A $=_{def}$ Agent A would continue to believe in L under admissible changes to A's evidence.

3. L is a law of nature $=_{def}$ L is true in every possible world and L is not analytically true.

The first toy account is a simplified version of David Lewis's "Best-System Account" of laws (1973, 72-73), according to which the laws of nature are the contents of the true deductive system that best balances strength with simplicity. I considered this account in more detail in chapter 2. The second is a simple version of an account according to which the laws are defined by the epistemic roles they play. Versions of it are defended by Skyrms (1980; 1995) and Urbach (1988; 1992). Both of these reductive accounts purport to show that laws of nature reduce to history; as such, they fall within the scope of the previous chapter's argument. The final account is a very simple necessitarian view, more complex versions of which will be considered at length in this chapter.

As was the case with the previous chapter, I will say that a *reductive definition of physical necessity* is one on which the *nomic* facts—those relating to laws, causes, coun-

terfactuals, and the like—are shown to be entirely dependent upon the non-nomic facts. If this reduction holds, then a proposition stating the complete non-nomic facts about the world should entail a proposition stating the nomic facts[4]. While it is in many cases difficult to determine whether or not a given account is reductive, I do think it is possible to give *necessary* conditions for a given account's counting as reductive. On the basis of criteria proposed by John Carroll, I will say that a reductive account of physical necessity must hold that the facts about physical necessity reduce to (or supervene upon, determined by, etc.) facts that are expressible without using any *explicitly* nomic concepts. So, an account is reductive just in the case that the facts about laws are determined by facts that *do not* pertain to causes, probabilities, dispositions, or the like. According to Carroll, the nomic concepts are those "that have been universally recognized as having a modal character and as inappropriate for a use in a definition intended to tame the modality of lawhood" (1994, 7). Carroll's 'nomic concepts' do not exhaust the list of concepts with nomic commitments; rather, they merely provide an (incomplete) list of some concepts whose comprehension is too tightly and obviously tied to physical necessity to be of any use in providing a reductive definition. Carroll goes on to argue that the facts cannot be reduced to the non-nomic facts (e.g., the facts expressible in terms of non-nomic concepts); it follows trivially from this that the laws are not determined by the facts *free* from nomic commitments.

While Carroll's definition of 'non-nomic' works well enough for his purposes, I will adopt a slightly more permissive version of it. In particular, I will say that a concept is *nomic* if its "modal character" cannot be understood independently of the *physical modality relevant to distinguishing laws of nature from accidental generalizations*. This proposed modification allows for modal notions to be used in reductive accounts of physical necessity but requires that these modalities be understandable independently of the distinction they make between laws and accidental generalizations. For example, an analysis of physical necessity that appeals to some independently characterizable sense of logical or epistemological necessity could still be genuinely reductive. A definition could not be reductive, however, if the concepts appealed to cannot be characterized independently of the role they play with regards to distinguishing laws from accidents.

[4]For an explanation of why this entailment holds, see Jackson (1994).

So, for example, a reductive account cannot appeal to physical probabilities, causes, or the truth of counterfactuals.

4.1.3 Is Physical Necessity a Type of Metaphysical Necessity?

One way of accounting for the difference between laws and accidents would be to show that physical necessity is a special case of logical or metaphysical necessity. I will call the thesis that physical necessity can be so reduced *law necessitarianism* (LN). LN has been defended by a number of recent authors[5]. Aside from a general concern for ontological parsimony (the less primitive "necessitation" relations, the better), such a reduction might be hoped to have at least two advantages. First, insofar as the ontology and epistemology of metaphysical necessity are (perhaps) better-understood than that of physical necessity, such an account could help explain the ontological status of and our epistemic access to claims about physical necessities. Second, this identification might provide an explanation for the ability of laws to support counterfactuals not supported by accidental generalizations.

There are two strong prima facie reasons for thinking that LN is false, however. First, many paradigmatic metaphysical necessities, including the truths traditionally classified as "logical" or "analytic", look to be knowable *a priori*. By contrast, almost every paradigmatic physical necessity—the fundamental laws of physics, for instance—seems to be discoverable only by empirical investigation. Defenders of LN have generally responded that laws are a variety of the "a posteriori necessities" identified by Kripke. Later in the chapter, I will argue that their are significant disanalogies between Kripke's arguments and the arguments given for LN, and that defenses of LN require stronger (and different) conceptions of metaphysical necessity and/or modal epistemology than those relied on by Kripke.

The second, related, problem is that laws of nature have an appearance of contingency that paradigmatic metaphysical necessities do not. One indication of this appar-

[5]For example, Swoyer argues that "if ϕ is a true statement of law, then it is [metaphysically] necessarily true" (1982, 221), Shoemaker claims that "causal necessity is just a special case of metaphysical necessity"(1998, 61), and Bird defends the thesis that "All the laws of nature are metaphysically necessary" (2005b, 442). Anderson, following Bird, also offers a limited defense of the "scientific essentialist" view that "all the laws of nature are metaphysically necessary" (2005, 373).

ent contingency is the relative coherence of considering the denial of a law of nature versus the denial of a logical or conceptual truth. It seems perfectly coherent to consider what would result if, contra the actual laws of nature, the maximum speed of light were to be slightly greater than it actually is[6]. On the other hand, it is difficult to make sense of suppositions such as "were it the case that triangles did not have three sides" or "were $1+1$ to equal 3." In general, there appears to be a distinction between the way we understand counterfactuals with antecedents that we know to be *physically impossible* and those with with antecedents we know to be *metaphysically impossible*. We can and do consider counterfactuals of the former type whenever we consider what the world would have been like, had Newtonian metaphysics been true or whenever we read a novel by Kurt Vonnegut or J.K. Rowling. Such exercises of the imagination can be both scientifically productive and enjoyable. Conversely, it is difficult to think of a case in which we deliberately entertain the possibility that a known metaphysically impossibility is true. We do consider metaphysically impossible hypotheses, of course—the history of philosophy and mathematics provides ample evidence of this—but we rarely continue to entertain these sorts of hypotheses once their impossibility becomes evident. Once we have discovered, for instance, there is no final digit of π, there is little sense to made of the counterfactual antecedent *were there to be a final digit of* π. It might be interesting to see the proof that this impossible, of course, but there is little sense to be made of the proposition itself.

I think that these intuitive differences between physical and metaphysical necessity provide a powerful argument against LN. It is not the end of the story, however, as defenders of LN have produced powerful defenses of their view. There have been at least two ways in which defenders of LN have argued for their position. The first line of argumentation might be called the *Best Explanation Argument for LN* and suggestions of it are found in various forms in Swoyer (1982, 208-210), Ellis and Lierse (1994, 42-44), Smith (2001), and Wilson (2005, 440-441). This argument, at least in some of its versions, contends that we are justified in believing that LN is true *despite* its highly

[6]For example, "Were the speed of light in a vacuum to be one-tenth greater than it actually is, electrons would still be negatively charged" seems to be true.

counterintuitive consequences[7]. This argument involves an explicit or implicit appeal to the best explanation. In this case, the explanandum includes certain well-recognized features of laws and physical necessities. Among other things, these facts include (1) that the laws are preserved under a wide and important range of counterfactual suppositions, (2) that the distinction between laws and accidental generalizations is an objective fact, (3) that laws imply the truth of certain regularities, and (4) that we have some epistemic access to what the laws are. Smith, for instance, claims that LN can successfully explain these feature of laws but that no competing reductive account can. In particular, he claims that LN provides a better explanation of the nature of laws than either Lewis's Humean account or the Dretske-Tooley-Armstrong "contingent necessitation" approaches. Moreover, Smith argues that LN does no worse on other features of the explanandum (e.g., the apparent possibility that the laws are false) than do the other views he considers.

I will not consider this line of argumentation in any detail, as its primary target seems to be other reductionists (such as those discussed in the last chapter) and not nomic primitivists. For primitivists, the argument is a non-starter, since it assumes (as a premise) that the physical necessity of laws can be reduced, which is precisely what primitivists deny. In essence, the argument from the best explanation assumes as a premise that some variety of reductionism is true, then concludes that LN is true since it is better than all competing reductive accounts[8]. I will remain agnostic as to the success of LN vis a vis other reductive accounts. In any case, the primitivist need not claim that laws are completely opaque or mysterious; rather, the claim is simply that since the necessity of laws represents a primitive, irreducible fact about our world, we should not expect that it can be reductively explained in the way that Humean or law

[7]Smith, before beginning his positive argument for LN, writes that "Why should what it is metaphysically possible, impossible or necessary be determined by what can or cannot be conceived or imagined by the species of organisms to which we belong? The thesis that metaphysical modalities are determined by the mental capacities of the human species is only slightly less implausible than the thesis that metaphysical modalities are determined by the mental capacities of chimpanzees" (2001, 904).

[8]Many defenders of LN reject primitivism for the same reasons that the reductionists mentioned in the last chapter do—i.e., that the intuitions supporting it are suspect, that it is unparsimonious, that it entails the laws are "ungrounded", and that it cannot account for our knowledge of laws. In the previous chapter, I argued that these sorts of responses fail.

necessitarian accounts entail.

The second line of argumentation, which I will call the *Semantic Argument for LN*, contends that an examination of the way that we talk about laws and physical necessities reveals that we are already committed to LN. This line of argumentation has been advanced by Swoyer (1982), Bigelow (Bigelow *et al.*, 1992), Shoemaker (1998), Bird (Bird, 2004), and Anderson (Anderson, 2005). The semantic argument motivates LN by identifying laws of nature as a variety of the *a posteriori* metaphysical necessities discussed by Saul Kripke (1980); this identification also goes part of the way toward providing an answer to some of the worries mentioned earlier. In the remainder of this chapter, I will argue that the semantic argument fails as a defense of LN and hence, that the prima facie reasons for dismissing LN are cogent ones. My argument will proceed in several stages. First, I will outline an account of modal epistemology that can account for both (a) the cogency of the Kripkean arguments that defenders of LN often cite as models and (b) the evidential importance of the intuitions of competent language users in determining which sentences express physically necessary propositions. Second, I will argue that there (for the reasons relating to those mentioned above) is little reason to think that competent speakers treat sentences expressing physically necessities in the way they treat ones expressing metaphysical necessities. Finally, I will consider and reject two proposals intended to defuse the apparent "counterintuitive" aspects of LN.

4.2 LAW NECESSITARIANISM

Before considering the cogency of the semantic argument for LN, the thesis needs to be clarified in at least two respects. The first respect concerns the concept of necessity that is to be used in the analysis of physical necessity. Defenders of LN sometimes claim that the laws are necessary in the same sense as logical truths are. In order for this claim to have any definite content, however, something more will need to be said about the concept of "logical" or "metaphysical" necessity. The second respect concerns the intended *scope* of LN—i.e. the question of precisely which propositions defenders of LN are committed to the necessity of, and which they are not. My goal here will be to articulate precise definitions of 'metaphysical necessity' and 'law necessitarianism' that

are (1) weak enough to be implied by most of the prominent necessitarian accounts but (2) strong enough to be incompatible with accounts on which the laws are contingent.

Many defenders of LN, and all of the ones discussed in this chapter, explicitly acknowledge that the concept of *metaphysical necessity* relevant to defining LN is the one characterized by Kripke, so his explicit remarks are a good place to start. Kripke generally uses the term 'metaphysical possibility' to denote an objective sense of possibility that contrasts with the merely 'epistemic' possibilities that are relative to a given agent's background knowledge. He also explicitly identifies the "metaphysical" sense of possibility as the "broadest" concept of possibility (1980, ft. 18) and suggests that necessity proper is a "metaphysical" notion (1980, 45). At other places, he apparently uses 'logically possible' as a synonym for 'metaphysically possible' (1980, 143).

Taken together, Kripke's remarks suggest the following (non-reductive) definition of metaphysical necessity:

MN P is metaphysically necessary $=_{def}$ P is true at *every* (logically, metaphysically, etc.) possible world[9].

According to MN, a metaphysically necessary statement is a statement that could not (in the strongest possible sense) be false—it is true in every possible world. Accepting MN does not entail, however, that one cannot recognize interesting and relevant differences between the logical truths and other metaphysically necessary propositions. For instance, some philosophers have thought it plausible that logically true sentences contain only logical terms essentially[10], or that logically necessary propositions are characterized by certain epistemic features (for instance, that they are knowable *a priori*).

[9]This definition accords with George Bealer's claim that 'metaphysical necessity' "is a term which Kripke stipulatively introduced, solely for heuristic purposes, as a synonym of his term 'logically possible'—that is, of 'possible"'(2002, 78). It also accords with the explicit characterizations of metaphysical necessity given by prominent law necessitarians such as Shoemaker (1998), Smith (2001), and Wilson (2005).

[10]I have in mind here Quine's definition of logical truth: "The logical truths, then, are those true sentences which involve only logical words essentially. What this means is that any other words, though they may also occur in a logical truth (as witness 'Brutus', 'kill', and 'Caesar' in 'Brutus killed or did not kill Caesar'), can be varied at will without engendering falsity" (1960, 352). One can accept something like Quine's definition of logical truth while still accepting the existence of other types of non-logical metaphysically necessary truths (e.g. analytic truths).

So, for example the proposition *water is* H_2O is sometimes held to be metaphysically, but not logically, necessary. I have no quibbles with such talk except to note that, if this proposition is metaphysically necessary, then there is *no* logical possible "world" in which it is false.

MN is consonant with many, though not all[11] of the philosophical glosses that have been given to 'metaphysical necessity.' Kit Fine, for example, states that the metaphysically necessary propositions are those that are "true in virtue of the nature of all objects whatever" where objects include both individuals and concepts (1994, 9). He argues that other varieties of necessity, such as logical or nomological necessity, are to be understood as involving only the individuals and concepts of the relevant domain. In a somewhat similar characterization, Gideon Rosen states that "[metaphysical necessity] is the strictest real necessity and metaphysical possibility is the least restrictive sort of real possibility in the following sense: if P is metaphysically necessary, it is necessary in every real sense; if P is really possible in any sense, then it is possible in the metaphysical sense" (2006, 16). Here, a *real modality* is a non-epistemic modality knowledge of which can sometimes be substantive and synthetic. This proviso concerning "substantive" and "synthetic" truths is intended to mark a contrast with definitions of 'logical necessity' or 'conceptual necessity' that definitionally require that any proposition satisfying them be knowable a priori.

If the above definition of metaphysical necessity is correct, *law necessitarianism* (LN) can defined as the conjunction of the following two propositions:

LN1: P is physically necessary $\Rightarrow$ P is metaphysically necessary.

LN2: P is metaphysically necessary and X $\Rightarrow$ P is physically necessary.

[11] There are certain uses of 'metaphysical possibility' MN is incompatible with. In particular, it is in compatible of views on which any logically consistent sentence must be true in some possible world. For example, Salmon argues that "metaphysical notion of possibility restricts the logical notion of possibility" (1989, 14) and that the concept of metaphysical necessity used by Kripke can be understood as corresponding to our ordinary notion of a *possible* "total way things might have been" (1989, 18). One of Salmon's arguments for this is premised on the idea that there are certain logically possible ways things might have been that are nevertheless not *possible* in any ordinary sense of the word 'possible.' None of the argument of this chapter will depend on the existence (or non-existence) of Salmon's merely logically possible worlds; with this in mind, I will presume that Salmon's characterization of metaphysical necessity is irrelevant to the truth of LN.

LN1 states that physically necessary propositions are true in every metaphysically possible world; LN2 states that the physically necessary propositions form a subset (proper or otherwise) of the propositions that are true in every world. Most of the work in defending and criticizing the plausibility of LN has, with reason, focused on LN1. However, varying the value of X in LN2 can result in versions of LN that are distinct in interesting ways. For instance, X might require that the physical necessities not be knowable a priori or that physically necessary propositions at a world pertain to properties that are actually instantiated at that world. This latter requirement would allow for the possibility of there existing worlds with distinct laws, so long as these world contained different properties. On this characterization of physical necessity, the physical necessary propositions would be true at every world, but would not be physically necessary at every world. In general, I will assume that LN does *not* imply that the actual laws are laws in every possible world—i.e. it does not imply that propositions of the form *P is a law* are metaphysically necessary. Rather, it implies only that, if P is a law, then P will be *true* in every world[12].

LN is distinct from several philosophical positions that have sometimes been called 'necessitarian.' First, it is stronger than the claim that only *some* physical necessities are metaphysically necessary. I think this weaker thesis, even if true, is philosophically uninteresting. First, it would be true in the case that some competent speakers, in some contexts, treated expressions of laws as stipulative definitions of the terms contained[13]. If any such propositions were analytic in this sense, they would be metaphysically necessary. Even the strongest critic of LN might be prepared to grant this possibility. Second, one primary attraction of law necessitarianism is its claim to provide a reduction of physical necessity to metaphysical necessity. The weaker claim does not ground such a reduction.

Second, LN is distinct from the claim that laws *entail* certain metaphysical neces-

[12]Alexander Bird defends the "strong necessitarian" thesis according to which "there is no difference between possible worlds as regards their laws; nomologically, they are identical" (2004, 259). On this thesis, true propositions of the form *P is a law* are true in every world. If my arguments in this chapter are successful, they will apply to this stronger thesis as well.

[13]Sober (1997) argues that many principles called 'laws' by biologists, such as Mendel's laws, are generally understood to be analytically true.

sities but are not themselves metaphysically necessary. Leckey and Bigelow (1995), for example, have presented a version of necessitarianism on which it is both metaphysically possible and actually possible for the laws to be false. So, suppose the proposition *in circumstances C, quantities X and Y will be in proportion Z* is physically necessary. Leckey and Bigelow claim that the proportion Z "can be guaranteed to hold in circumstances C only on the condition that some further condition $N_@$ is met, and we call this condition the naturalness condition." They propose that this naturalness condition is the proposition that "the states of x [a system that law apparently pertains to] are natural states for the actual world" (1995, 104-105). *Natural states* is a technical notion intended to indicate that there are no "non-natural" properties or forces that are interfering with the normal operation of the law. The relevant feature of such positions for our purposes is that they are only *reductive* if one can give a determinate, non-nomic interpretation of $N_@$. Leckey and Bigelow do not even purport to do this—the concept of a *natural state* is an explicitly nomic one and is as such unsuitable for use in a reductive definition of physical necessity. With this in mind, I will put aside the consideration of such positions and focus exclusively on the plausibility of LN.

4.3 CARNAP AND KRIPKE ON MODAL EPISTEMOLOGY

According to the Semantic Argument for LN, a close examination of the way in which we make judgments about physical necessity, metaphysical necessity, and the laws of nature reveals that we are already committed to LN. As a claim about actual practice, this argument is thus vulnerable to the criticism that it does not accurately reflect the judgments about physical necessity made by this population. If it could be shown, for example, that the relevant community does *not* treat the laws of nature and their consequences as metaphysically necessary, this would undercut the Semantic Argument and provide independent evidence against LN. I will be arguing that an examination of actual practice reveal that this is indeed the case. In order to do so, however, it will be necessary to provide an account of how the judgments of competent speakers are relevant to determining which of their sentences express metaphysically necessary propositions.

With this in mind, I will offer a brief presentation of what I will call the "Carnap-Kripke method" for investigating whether or not a certain *sentence* expresses a metaphysically necessary proposition. I will begin by outlining a minimal account of modal epistemology proposed by Carnap in "Meaning and Synonymy in Natural Languages" and then show how this account can be extended to offer an illuminating explanation of the cogency of Kripke's famous arguments that there exist substantive "a posteriori necessities." My presentation of this as a "method" is not meant to provide a substantive account of modal epistemology or even of Kripke's particular project. The goal here is the much more modest one of showing that one can account for the cogency of paradigmatic arguments about posteriori necessities using little more than the characterization of metaphysical necessity provided by MN. In the succeeding sections, I will contend that the Semantic Argument for LN require substantive assumptions about metaphysical necessity that go beyond MN and thus, that one can consistently uphold the cogency of Kripke's arguments while rejecting the Semantic Argument LN.

4.3.1 Carnap on Confirming Modal Claims

In *Meaning and Necessity*, Carnap proposes an explicatum N for what he calls the ordinary concept of "logical necessity." The way in which he introduces and defines N strongly suggests that "logical necessity" can be usefully thought of an explicatum for what I have called *metaphysical necessity*. Carnap first defines an *L-true* sentence to be one that "holds in every state-description" (1988a, 10). He goes on to define N as follows: "For any sentence '…', N'…' is true if and only if '…' is L-true" (1988a, 174). He characterizes state-descriptions as classes of sentences that are intended to represent "Leibniz's possible worlds or Wittgenstein's possible states of affairs" (1988a, 9). Carnap's definition of logical necessity thus strongly suggests it is meant to capture the idea of "truth in all possible worlds," which MN picks out as the defining characteristic of metaphysical necessity. At no point does Carnap suggest that logical necessities must be knowable *a priori*, that they must have any distinctive syntactic form, or that they have any of the other properties that are commonly thought of as 'logical' in a more narrow sense of the word.

In "Meaning and Synonymy in Natural Language," Carnap proposes a method that can be used to empirically investigate whether or not the sentences of a natural language express propositions that are necessary in the above sense (1988a, 238-240). This method involves querying the person who asserted the sentence to see the conditions under which she would apply the terms that comprise the sentence. Carnap considers the case of a linguist attempting to determine whether German-speaking Karl's word 'Pferd' means *horse* or whether it means *horse or unicorn*. The linguist has already the determined the extension of 'Pferd' in the actual world; it applies to all and only horses. In essence, the linguist knows that something like the proposition *no pferd is a unicorn* (the proposition expressed by 'kein pferd ist ein einhorn') is true; the linguist wants to know if it is necessary. In order to determine this, the linguist must first determine the meaning of 'Pferd.' In the case that 'Pferd' means *horse or unicorn*, the proposition is true but not necessary; if it means *horse*, then the proposition is both true and metaphysically necessary.

Carnap proposes that the following method can be used to empirically investigate the meaning of 'Pferd' (and hence for investigating the necessity of the above proposition):

> [The linguist] must take into account not only the actual cases, but also possible cases. The most direct way of doing this would be for the linguist to use ... modal expressions corresponding to "possible case" or the like ... [or] The linguist could simply describe for Karl cases, which he knows to be possible, and leave it open whether there is anything satisfying those descriptions or not. He may, for example, describe a unicorn (in German) by something corresponding to English formulation "a thing similar to horse, but having only one horn in the middle of the forehead." Or he may point toward a thing and then describe the intended modification in words ... Or, finally, he might just point to a picture representing a unicorn. Then he asks Karl whether is willing to apply the 'Pferd' to a thing of this kind. (1988b, 238).

If Karl says that he is willing to apply his word 'Pferd' in these cases, Carnap claims that

this provides *evidence* that 'Pferd' means *unicorn or horse*. If not, it provides evidence that it means *horse*. However, this evidence is not conclusive. After all, the linguist may have improperly described the scenario, or Karl's answers here may be inconsistent with the answers he has given to other such queries. The important thing to note is that the linguist's only way of discovering and correcting such errors is to ask Karl more questions of the same type, to make sure that her questions are worded clearly, and to give Karl the time he needs to answer them. Carnap explicitly requires that Karl be asked about the application of his words in cases "that are causally impossible, i.e., excluded by the laws of nature holding in our universe, and certainly those that are excluded by laws which Karl believes to hold" (1988b, 239). In modern philosophical parlance, we might say that Carnap's linguist is asking about what Karl can *intuit*, *conceive*, or *imagine*[14].

In the case that Karl consistently refuses to apply 'Pferd' to various presentations of possible unicorns, it is at least somewhat plausible that some proposition of the form *No pferds are unicorns* is both necessary and knowable a priori by Karl. After all, Karl does not appeal explicitly to any empirical evidence when deciding which answer to give the linguist's probing. However, there is nothing in Carnap's account that *requires* we assign to Karl a general capacity for determining a priori which of his sentences express metaphysical necessities and which do not. In fact, it is relatively trivial to extend Carnap's method to the problem of discovering so-called "a posteriori" necessities. This is because there are certain things that Karl could say that would provide evidence for the hypothesis that Karl's commitment to a given sentence being true in every case is itself dependent upon some external factor. In general, this will happen any time that Karl's reports suggest the proper application of some term or phrase of his *depends upon some metaphysically contingent fact about the actual world*.

An example will help to illustrate this point. Suppose that Karl, when queried by the Carnapian linguist, reports that whether or not 'Pferd' can be properly applied to

[14]It is important to note that Carnap does not assign a capacity for individual speakers to make propositions necessary merely by their intending that it be so, nor does he assign to intuition anything like the perceptual-analogue role that some contemporary thinkers do. On Carnap's view, necessity (or lack thereof) is a function of meaning, which is a semantic property of the language as a whole. Querying individual speakers is merely our best method at figuring the semantic properties of a language. Both logical necessity and the closely related notion of analyticity are clearly distinct from what might now be called "conceptual necessity."

unicorns depends on what the fellow members of his linguistic community say, or on what the zoologists say, or on the biological species of the first animal referred to as a 'Pferd' by a member of his linguistic community. In these cases, Karl is admitting that he *doesn't know* (and certainly doesn't know a priori) what types of things his term can correctly be applied to; hence, he cannot know a priori whether it applies to unicorns (or even whether it applies to horses). The linguist, however, can still contribute to the investigation of whether Karl's sentence expresses a necessary truth. She can do so by asking Karl whether 'Pferd' would apply *on the assumption that* certain conditions hold. For example, she might ask Karl whether 'Pferd' would apply to unicorns on the assumption that the first thing referred to by 'Pferd' was a horse. Karl might say no. Karl's answer, when combined with the empirical confirmation that 'Pferd' was first applied to a horse, can now serve to confirm that claim that *No pferds are unicorns* is metaphysically necessary. One interesting case (which will be examined below) is that where the linguist asks Karl about the possibility of that a certain sentence expresses a false proposition *on the assumption that the proposition expressed is actually true.*

In general, the method described above treats the case of a posteriori necessities just as it treats a priori ones. In both cases, the linguist's hypothesis that a certain sentence expresses a metaphysically necessary proposition instead of a true but contingent one can be tested by asking the relevant speakers whether or not they would assent to the sentence if various explicitly specified possibilities were to obtain. If they say that they would assent to it in every case, but only on the condition that some true C holds, this provides evidence that their sentence does, in fact, express a metaphysically necessary proposition. The acceptance of this method need not entail any particular account of meaning or reference, nor any controversial claims about the nature of metaphysical necessity. Instead, it simply presupposes that speakers are generally good judges of whether or not their terms would correctly apply[15]. After all, the linguist is free to

[15]This picture becomes a bit more complex when one is trying to consider a community's use of words, and not simply those of an individual. Speakers can, of course, be mistaken about how other members of their community use their words, so the responses of a single speaker can hardly be taken as definitive. So, for example, it might be the case that most (but not all) members of a linguistic community treat the word 'water' as a non-rigid designator. In such cases, I take it that something like 'majority rule' applies, at least for many purposes. In any case, I take it that univocal community usage is definitive; it is simply incoherent to claim that a term is rigid if every member of the relevant community treats it as non-rigid.

specify the truth or falsity of any proposition that Karl may be unsure of. While this method may seem too trivial to ground any interesting claims, I will argue that it can fully account for the cogency of Kripke's arguments that certain a posteriori truths can be metaphysically necessary.

4.3.2 Kripke on A Posteriori Necessities

Kripke argues that there is a large, important class of English sentences that (1) make substantive claims about the world whose truth cannot be ascertained by a priori methods but (2) express metaphysically necessary propositions. Kripke supports these claims directly by appeal to judgments about the correct application of terms in precisely described counterfactual scenarios. In this section, I will take a closer look at Kripke's argument with two primary goals in mind. First, insofar as LN claims that the laws are metaphysically necessary, and insofar as the Semantic Argument claims that LN is supported by appeal to ordinary practice, Kripke's argument will provide a valuable comparison case. Second, I will suggest that Kripke's methodology can, in very important respects, be interpreted as an instance of the Carnapian methodology described above. In particular, one can account for the cogency of Kripke's central arguments by noting the (implicit) role played by the judgments of competent language users; there is simply no need to appeal to the sorts of non-linguistic considerations that I will argue characterize the Semantic Argument for LN. The cogency of some of Kripke's (and Kripke's followers) more ambitious arguments concerning metaphysical necessity need not concern us here; for example, I will not consider the claim that all identities are necessary or the hypothesis that metaphysical necessity must in some way be tied to the "essences" or "natures" of various things. In any case, Kripke's overall project depends crucially on his success in establishing that a number of *particular* claims are metaphysically necessary based solely on our judgements about the reference of terms in various counterfactual scenarios. These arguments are directly relevant to the case for LN, since its proponents claim that its truth can be supported in the same way[16].

[16]Putnam, when summarizing and endorsing Kripke's main position, describes it as the claim that "a statement can be metaphysically necessary and epistemically contingent" (1975, 170). Kripke suggests at places that he thinks that *all* claims about the identity of two individuals in English express metaphysically

Kripke offers arguments that the following sentences, though they express substantive claims about the world, nevertheless express metaphysically necessary propositions[17]:

K1 The parents of Queen Elizabeth II are not Mr. and Mrs. Truman (1980, 112-113).

K2 Water is H_2O (1980, 128-129).

K3 Heat is the motion of molecules (1980, 129-130).

K4 Hesperus is Phosphorus (1980, 102-103).

K5 Gold has the atomic number 29 (1980, 116-118).

K6 Cats are animals (1980, 123-125)[18].

K7 Heat is molecular motion (1980, 98-100).

In his arguments for the necessity of K1-K7, Kripke makes explicit and repeated appeals to how we (as readers) would "describe" certain situations, or what we would "say" when presented with various counterfactual situations. This is just what Carnap's method requires. Moreover, Kripke does not explicitly appeal to any *other* form of evidence, such as considerations of simplicity or ontological parsimony when arguing that these propositions are necessary.

So, for example, consider the following reconstruction of Kripke's argument for the necessity of K1. It can be laid out as follows:

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necessary propositions. This claim is more analogous to law necessitarianism; however, the argument presented here does not establish this.

[17]Kripke doesn't usually speak of 'propositions'; however, he clearly thinks that sentences express *something* and that it is the thing that is expressed that is either metaphysically necessary or not. In any case, I have cast Kripke's arguments in terms of 'propositions.'

[18]Kripke actually says that the English *sentence* 'Cats are animals' "has turned out to be a necessary truth" (1980, 138). As I just noted, I have explicated Kripke's arguments on the assumption that Kripke's *evidence* concerns the appropriate use of various English sentences or terms and that his conclusions are that the propositions expressed by these sentences are metaphysically necessary or not necessary.

1. It is metaphysically possible that the term 'Elizabeth II' refers to a daughter of Mr. and Mrs. Truman[19].

2. Suppose that, contra the above possibility, the person denoted by the English term 'Elizabeth II' "really did come from these [her actual] parents" (1980, 112).

3. Once this supposition is made, competent speakers of English would refuse to call the daughter of Mr. and Mrs. Truman 'Elizabeth II' *in any possible case.*

4. So, if the English term 'Elizabeth II' actually refers to someone whose parents are not Mr. and Mrs. Truman, then the English sentence 'The parents of Elizabeth II are not Mr. and Mrs. Truman.' expresses a metaphysically necessary truth.

I take it that the first premise is relatively uncontroversial, since we easily could have used 'Elizabeth II' to pick out a coffee can or a goat. The second premise is simply an assumption. In his support for the third premise, Kripke asks the English-speaking reader, just as Carnap's linguist asks Karl, to consider how a certain *term* would be applied in various counterfactual scenarios *while holding the actual reference of the term fixed*. So, for example, he asks the reader to consider a case in which the actual Queen, Elizabeth, never ascended to the thrown but that a child of Mr. and Mrs. Truman, who physically resembles the actual Queen, did. Kripke claims that this would still not be a scenario "in which *this very woman* whom we call 'Elizabeth II' was the child of Mr. and Mrs. Truman" (1980, 112).

The important thing here to note is the crucial role that appeal to linguistic practice plays in the third premise. If most English speakers considered it correct to call Mr. and Mrs. Truman's daughter 'Elizabeth II', then Kripke would have provided *no reasons whatsoever* for thinking that (1) 'Elizabeth II' serves as a rigid designator in English or (2) for thinking that the sentence K1 expresses a metaphysically necessary proposition. Kripke himself recognizes the crucial role that appeals to intuition (here construed as appeal to linguistic practice) play in his argument. After offering the above argument,

[19]"There would be no contradiction, of course, in an announcement that (I hope the ages do not make this impossible), fantastic as it may sound, she was indeed the daughter of Mr. and Mrs. Truman" (1980, 112).

he notes that "one can only become convinced of this by reflection on how you would describe this situation (That, I suppose, means in many cases that you won't become convinced of this, at least not at the moment. But it is something of which I personally have been convinced.)" (1980, 113). Kripke's argument here is thus *explicitly* limited to queries of how readers (who are presumably competent users of the term 'Elizabeth II') would apply their terms when presented with counterfactual scenarios. Moreover, I think a stronger reading is plausible: if Kripke is wrong about how most competent speakers of English would use their term in the scenarios he describes, then he is also wrong in his claims about which sentences express metaphysically necessary propositions[20].

Each of Kripke's arguments is structured in an analogous manner. He first asks us to note that, depending on the reference of certain terms, the relevant sentence may be false. He goes on to argue that, on the *assumption* that certain terms refer to certain things (e.g., the things they actually refer to), we will agree that it would be impossible for this sentence to be false. In every case, he makes repeated appeals to what his readers would "say" or how they would "talk about" various worlds. For example, after granting that it is an empirical discovery that water is H_2O, he argues for the necessity of K2 on the following grounds:

> If there were a substance, even actually, which had a completely different atomic structure from that of water, but resembled water in these respects, would we say that some water wasn't H_2O? I think not. We would say instead that just as there is a fool's gold there could be a fool's water; a substance which, though having the properties by which we originally identified water, would not in fact be water. And this, I think, applies not only to the actual world but even when we talk about counterfactual situations. If there had been a substance, which was a fool's water, it would then be fool's

[20]There are rival readings of P3 available, of course. For instance, Kripke may simply assert, without any support, that 'Elizabeth II' does not refer to Mr. and Mrs. Truman's daughter in the scenario he describes (and simply proceeds on the grounds that his readers will agree). I think that this reading should be avoided, as it cannot account for the support provided to P3 by the intuition that P3 is true. On the reading given above, the intuition that P3 is true is evidentially relevant to the truth of P3 because P3 is itself a claim about how competent language users will behave.

water and not water (1980, 128).

Again, Kripke's argument here is a relatively conservative extension of the Carnapian modal epistemology described previously; Kripke's major insight is to note that speakers' intentions are more complex than Carnap had noticed and that (because of this complexity) what a given speaker's word refers to can depend, in part, on things outside that speaker's control. The claim that 'Water is H_2O' expresses a metaphysically necessary proposition is defended by appeal to what we would *say* is water on the assumption that all actual water is H_2O. These appeals to linguistic practice are relevant because, as competent speakers of English, we are good judges of whether our *term* 'water' would apply when counterfactual scenarios are described for us; when we combine this expertise with the scientific discovery that the stuff we've been calling 'water' is H_2O, we can figure out whether our sentences express metaphysically necessary propositions.

One problem case that Kripke deals with is the case where speakers report that they do think it is possible for a given sentence to be false, even though Kripke claims they are committed to this sentence's metaphysical necessity. He considers specifically the names 'Hesperus' and 'Phosphorus' (which name Venus in English). Many people (and philosophers in particular) have thought that it is possible for the sentence 'Hesperus is Phosphorus' to be false. Kripke argues that such reports are irrelevant because those specifying the queries have failed to make clear which facts were to be held fixed; in particular, it has not been specified explicitly that speakers are to hold fixed the fact that 'Hesperus' and 'Phosphorus' both actually name Venus. Kripke claims that, once it is stipulated that these facts are to be held fixed, we will no longer be willing to say that it is possible for this sentence to be false. It is perfectly possible, according to Kripke, that a person in an epistemic situation identical to our own (i.e. who sees planets in exactly the same places we do) might well name two distinct planets 'Hesperus' and 'Phosphorus'. But, Kripke argues, the fact that we can imagine ourselves in this astronomer's shoes is irrelevant to the necessity of the proposition expressed by our sentence. In order to demonstrate this, Kripke considers how we should describe this scenario of the possible astronomer once we are reminded to hold fixed that our terms actually refer to Venus. Kripke writes:

> How should *we* describe this situation? He can't have pointed to Venus twice, and in the one case called it 'Hesperus' and in the other 'Phosphorus' as we did so. If he did so, then 'Hesperus is Phosphorus' would have been true in that situation too. He pointed maybe neither time to the planet Venus—at least one time he didn't point to the planet Venus, let's say when he pointed to the body he called 'Phosphorus'. Then in that case we can certainly say the name 'Phosphorus' might not have referred to Phosphorus . . . But that is still not case in which Hesperus is Phosphorus. (1980, 102)

Again, this argument appeals directly to the readers' judgments about how they would use their *own* words 'Venus', Hesperus' and 'Phosphorus'. On the most plausible interpretation, Kripke's argument here is *premised* on his claim that speakers of English will agree, on reflection, that the astronomer does not mean the same thing by 'Phosphorus' as they currently do. If the English-speaking community does not judge as Kripke claims that they should or would, there is simply no reason to think that the sentence 'Hesperus is Phosphorus' expresses a metaphysical necessary truth.

In general, then, Kripke attempts to deal with such apparently contradictory speaker reports by showing that they are *not* reports that the given sentence might have been false, given that all the terms in it have the determinate references they actually do, but are rather reports that the speakers are unsure of what the reference of some term actually is. In these cases, he claims that the answers will change once it is stipulated that we are only to consider possible scenarios under which the relevant terms refer to the things to which they actually refer. So, for example, he claims that we will agree that Hesperus is necessarily Phosphorus "given that Hesperus is Phosphorus" (1980, 103), that gold necessarily has atomic number 79 "given that it *does* have atomic number 79" (1980, 123), and that cats are necessarily animals "given that cats are in fact animals" (1980, 126). In none of these cases does Kripke appeal to any evidence that is unsanctioned by Carnap's method, and he certainly does not appeal to any principle that would allow him to overrule the judgments of competent language users. He explicitly appeals only to what his readers would say about certain situations, and dismisses their reports only if the speakers can be convinced to revise such reports upon a more careful specification of which facts are to be held fixed.

4.3.3 Metaphysical Necessity and Semantic Facts

It was once a commonplace view to hold that sentences expressing logical or analytic truths are true solely in virtue of what a sentence "means." On the reading of Carnap and Kripke I've advocated, something like this still holds true of metaphysical necessities. When one one asserts a metaphysically necessary proposition, one is asserting something that holds in virtue of what might loosely be called "semantic facts"—facts about speaker intention, reference, and so on. Kripke's insight was to see that reference, meaning, and intention are more nuanced than had previously been thought. In particular, Kripke noticed that speakers intend to express different propositions, depending on whether or not some condition happens to hold. The speakers themselves may thus be unaware of whether the propositions they express are metaphysically necessary. There is thus some plausibly to the (somewhat vague and imprecise) claim that metaphysical necessity is a *semantic* type of necessity.

While this characterization of metaphysical necessity as a semantic variety of necessity may sound surprising, it fits in well with the way in which some proponents of LN characterize and defend their thesis. Shoemaker, for example, explicitly endorses a semantic characterization of metaphysical necessity. He writes that analytic truths (if they exist) are appropriately called metaphysically necessary insofar as their "truth is guaranteed by certain paradigmatic logical truths—say that all unmarried men are unmarried—*together with* the semantic facts, say that 'bachelor' is synonymous with 'unmarried male"' and further writes that Kripke's contribution was to show "that the class of semantic facts that can contribute to the bestowal of necessary truth is much broader than the class of synonymies and analytic equivalences" (1998, 60). Something like this thesis is also suggested (though not implied) by the wide range of law-necessitarian arguments that appeal to facts about the way we would describe various possible scenarios. In the next section, I will discuss these arguments at greater length.

Whatever the relation between meaning and metaphysical necessity, Kripke's arguments provide a useful model for utilizing a combination of non-modal empirical investigation and querying of speakers to support claims that certain sentences uttered by these speakers are metaphysically necessary. The method's reliability does not rely on

any particular conception of metaphysical necessity beyond that given by MN. It simply assumes that speakers, when given a sufficiently detailed characterization of a possible world, are generally good judges of whether a given sentence of their language would be true or false. To the extent that speakers report that whether or not some sentence of theirs is true in a particular case depends upon contingent facts about the *actual* world, we have evidence there are "a posteriori necessities." In the next section, I will argue that attempts to show that every physical necessity is metaphysically necessary fail.

4.4 THE SEMANTIC ARGUMENT FOR LAW NECESSITARIANISM

A number of philosophers have claimed that certain commitments implicit in our ordinary and scientific practice are best explained by a commitment to LN. I earlier called this style of argument the *Semantic Argument for LN*. Three distinct points of evidence have been offered in defense of this view. The first is that we purportedly refuse to countenance the possibility of properties being governed by laws other than those that actually govern them. So, for instance, many think that it is metaphysically impossible for there to be electrons that are positively charged. The second is that we regularly *define* theoretical terms (or the properties they denote) in terms of the laws that govern them. For example, some textbooks might *define* 'electrons' as 'negatively charged particles that ....' The final is that we do not recognize the possibility of any property subsisting through any actual change in the laws that govern it. That is, we do not think it is in any sense possible for an electron to (1) become positively charged and (2) remain an electron. I will consider each claim in turn, and argue that none provide sufficient reason to accept LN.

4.4.1 Laws and the Identity of Properties

Defenders of LN often note that (1) laws concern relations between properties and (2) in many cases, we refuse to countenance the possibility that the *same* property could obey different laws than those that it actually obeys. In cases where the laws in question are radically different than those that actually govern the property, this seems plausible. So, suppose that, in the actual world, 'electron' denotes particles that, as a matter

of physical necessity, have a charge of $-e$. Similarly, suppose that 'proton' picks out particles that, as a matter of physical necessity, have a charge of $+e$. Given these suppositions, it seems plausible that many people would refuse to countenance the possibility of protons having charge $-e$ and electrons having $+e$. This suggests that the sentence 'Protons have a charge of $-e$ and electrons have a charge of $+e$' expresses a necessary falsehood.

The problem for this line of argument is to show that it generalizes—i.e. to show that we are committed to the metaphysical impossibility of *any* violation of a law of nature, however small. The prospects for this do not seem as good, however. Chris Swoyer, for example, offers the following thought experiment (which he does not claim to be conclusive) in support of the thesis that electrons have their charge with metaphysical necessity:

> Imagine a possible world w_1 in which there are putative electrons with some determinate amount of charge $e'(\neq e)$ and a world w_2 which includes these particles of w_2 as well as electrons from the actual world. Now in w_2 are the first sort of particles (with charge e') electrons as well? Most people find it counterintuitive to suppose that they are and, moreover, our best theories of microphenomena seem to require that all electrons have the same charge if claims about the behaviour of atoms and the like are to be correct. (1982, 215)

Swoyer concludes that, since the putative electrons are not electrons in w_2, they cannot be electrons in w_1 (or anywhere else) either. The evidence comes from two sources: the explicit claims that particle physics makes about electrons and what "most people" find counter-intuitive. Swoyer's argument, if cogent, can easily be generalized to other cases—we simply take w_1 to be a world containing certain entities (or properties) that obey some minor variant of the actual laws and let w_2 be a world that contains both these suspicious entities and entities that obey our actual laws. We now note that, in such cases, we would not be willing to say that the suspicious entities are *the same things* as the entities obeying the actual laws. On this basis, we conclude that it is not metaphysically possible for the entities and properties of our own world to obey any

laws except those that they actually do obey. There may be worlds with other laws, of course, but these laws concern entirely different entities and properties from those that *our laws* concern.

The most significant problem with this style of argument is that it fails to show that the intuitions elicited by these sorts of thought experiments are indicative of a general commitment to LN. My suspicion is that they are not and that manipulating factors such as the nature and magnitude of the difference between the laws governing the two types of particles will change the intuitions, as will altering the context in which the thought experiment is presented. So, for example, even on the assumption that all actual electrons share precisely the same charge (and that the value of this charge is a matter of physical necessity), it does not seem metaphysically impossible for there to exist electrons whose charge differs *slightly* from these actual electrons. This suggests the size and the magnitude of the change in laws matters to which intuitions are elicited. A more significant worry involves the context in which we are being asked to make these judgments. Swoyer asks us to first consider two independently existing worlds and to then imagine that the particles of one world are exported (with all nomic properties and relations still intact) into the other world. The judgement that the these new, alien particles are not 'electrons' might well follow from the strange origins story and have little if anything to do with the metaphysical necessity of the relevant laws. Similarly, the judgement that the putative electrons are not, in fact, electrons, seems to hinge on the fact that we are first asked to consider worlds in which they must be compared to things that more closely resemble actual electrons. The failure of putative electrons to be electrons in this special class of worlds does not imply that they aren't electrons anywhere, however; it may simply be that they achieve electron-hood only in worlds where they face less vigorous "competition." In any case, I think that a better, less prejudicial thought experiment involves a direct judgment on whether there exists any possible world in which electrons have a charge that differs from that of actual electrons.

4.4.2 Laws and Definitions

The second aspect of scientific practice that defenders of LN have focused on concerns the role that laws play in *defining* certain terms or properties. So, for example, Anderson claims that LN is supported by the fact that scientifically interesting properties are "theoretically *defined* in terms of those forces, and the laws governing them" (2005, 378) and Wilson notes that "properties, at least of the sort at issue in the physicalism debates, appear to be defined by reference to (fairly) specific causal laws" (2005, 440). Neither author offers an extended argument detailing how this practice supports LN's truth over that of its denial. And indeed, it is difficult to see how such an argument could be constructed. So, consider the case of an electron. Actual electrons are presumably subject to numerous laws. Some of these are specific laws concerning their spin and charge, and may well feature in the explicit "definitions" given to 'electron' in physics textbooks. Some of these, however, are more general laws concerning the conservation of mass-energy, the maximum speed of light, and so on. These laws certainly do not *explicitly* feature in the definition of 'electron.' Even if we are committed to the impossibility, ala Swoyer, of electrons having a charge other than that which they actually have, we still might grant that it is possible for electrons to exist in a world in which these other laws are broken—e.g. worlds in which the maximum speed of light is slightly higher than it is in the actual world. Insofar as the claim that *electrons cannot be accelerated to a speed beyond c* is physically necessary, the existence of such worlds is incompatible with LN.

4.4.3 Shoemaker's Argument for LN

The best hope for defenders of LN thus seems to lie in the third aspect of practice—that is, in the (purported) fact that we do not recognize properties persisting through any *actual* change in their governing laws. Both Shoemaker (1998) and Wilson (2005) offer arguments that this feature of our practice is indicative of a commitment to LN. For the sake of clarity, I will focus primarily on Shoemaker's version of this argument, though I will return later to a particular aspect of Wilson's account. I will argue, contra Shoemaker and Wilson, that our commitment to the metaphysical possibility of *coun-*

terlegals and *miracles* shows that we can and do recognize properties existing through changes in their governing laws.

Shoemaker begins by claiming that Kripke's and Putnam's arguments about a posteriori necessities have shown us that it is "a general feature of our thought about possibility that how we think that something could have differed from how it in fact is is closely related to how we think that the way something is at one time could differ from the way that the same thing is at a different time" (1998, 69). The idea here is that the judgments about possible cases appealed to by Kripke in his arguments are (to a large extent) just the judgments that competent speakers would make were they to find themselves in an *actual* situation of the type that Kripke describes. As such, the possibilities relevant to determining metaphysical necessities concern only what speakers would say were they to encounter an actual sample of XYZ (e.g. on Mars, or far in the future), on the condition it resembled water superficially. On the basis of this principle, Shoemaker concludes that any constraint we put on recognizing *intra-world* variations of a property will imply a corresponding *inter-world* constraint on the variations we will recognize. So, if we wouldn't say that XYZ was water in the actual world (intra-world constraint), we won't say that it is water in any possible world whatsoever (inter-world constraint). For the same reasons, if we judge that is impossible for an actual tiger to cease being a mammal, we must also refuse to countenance merely possible non-mammalian tigers.

On the basis of this principle governing intra-world and inter-world variation, and on the assumption that laws concern relations between properties, Shoemaker offers a succinct argument for LN:

> But now consider the case of properties. Different things can be true of a property at different places, and at different times ... But it cannot be governed by different laws at different places or different times. Applying again the principle that constraints on intra-world variation are also constraints on inter-world variation, we get the conclusion that the same property cannot be governed by different laws in different worlds (1998, 70).

Shoemaker's claim can be restated as follows: since we do not countenance the existence of any actual property that can endure through any actual change in its governing laws,

consistency dictates that we similarly refuse to recognize the possibility that it could endure through any *counterfactual* change its governing laws[21] Thus the laws of nature, which on Shoemaker's view are entirely constituted by the constraints they place on properties, are metaphysically necessary[22].

Shoemaker's argument can be resisted in at least two ways. First, his proposal that intra-world constraints entail corresponding inter-world constraints is false if it is intended (as it seems to be) as a claim about the way in which we *actually* reason about possibilities. Assume that it is a law of nature, for instance, that physical objects cannot be accelerated beyond c. Let us grant, with Shoemaker, that the discovery of any actual object (in any circumstance) accelerated to beyond this speed would lead us to conclude that either (1) it did not have the property of being a physical object[23] or (2) the law was false. According to the Carnap-Kripke method, consistent reports of this type would suggest we are committed to the metaphysical impossibility of there existing intra-world instances of the same property that obey different laws.

It does not follow from the above, however, that we are committed the impossibility of there existing inter-world instances of a property that obey different laws. This is indicated by the fact that one may consistently refuse to recognize the metaphysical

[21]Shoemaker (for obvious reasons) cannot provide an example of an actual case in which the laws of nature have changed to support this point. His support for this point rather seems to concern the sorts of properties that are posited by physical theory and the relation they bear to laws. According to Newtonian physics, for instance, the properties Force, Charge, and Mass are always related by laws of nature. Shoemaker's premise, in essence, is that we do not posit the existence of properties that only sometimes obey laws of nature, or which occasionally change which laws of nature they observe. It would be absurd, on Shoemaker's view, to posit a property of Chmass such that its relation to Force is sometimes according to Coulomb's law (where it acts like Charge) and sometimes according to the law of gravitation (where it acts like Mass).

[22]Wilson's main argument in favor of LN appeals also appeals to fact that LN can straightforwardly explain why it is that "in both ordinary and scientific contexts, we neither experience nor posit properties as persisting through changes in their governing laws" (2005, 440). She goes on to argue that scientific practice does not recognize the existence of metaphysically possible physical impossibilities. She concludes that any view that commits itself to the existence of these possibilities is thus at odds the naturalist assumption that philosophical methodology and conclusions be "commensurable" and "consonant" with those of current science (2005, 439).

[23]We might conclude, for instance, that the object was only a "pseudo-object" analogous to Salmon's (1980, 50) pseudo-processes. Salmon offers the example of dot of light projected from a laser onto a stadium wall. One might be able to accelerate this dot of light to beyond c, but this is only because it is a pseudo-process.

possibility that the laws governing a single property can vary within a world, but nevertheless be willing to recognize the metaphysical possibility that *which* law governs the property in question can vary from world to world. And in many cases, we do just this. For example, we seem able to recognize that certain counterlegals are non-trivially true (or correct) while others are non-trivially false (or incorrect). So, for example, many of us would assent to the claim that "If gravitational force did not diminish with distance, then the earth's orbit around the sun would be far different from what it is now." Our recognition of this as non-trivially true does not, contra Shoemaker's principle, commit us to recognizing the (physical or metaphysical) possibility of the gravitational force in our world obeying more than one law.

Significantly, it does not seem that Shoemaker can simply dismiss these sorts of judgments as the products of flawed intuition. Such counterlegals are considered in a variety of ordinary and scientific contexts, the most obvious of which concerns the process of adjudicating between pairs of hypotheses that do not agree on what the laws are. For example, suppose that T_3 and T_4 are incompatible law-positing theories. In at least some cases (e.g. the case of Newtonian and Relativistic mechanics), it seems that scientists have been able to make substantive judgments regarding what the world *would have been like* on the assumption that one or the other theory were to be true. These judgments involve that consideration of subjunctive conditionals, at least one of which has a physically impossible antecedent. The important thing to note is our ability to assess subjunctive conditionals of the form "If T_3 were to be true ..." or "If T_4 were to be true ..." does not depend on our beliefs about what the laws currently are. Even if we believe that T_3 is true and T_4 is false, for instance, we can (in at least some cases) coherently entertain the proposal that it *could have turned out differently*—that is, it could have turned out that T_4 stated the laws of the *actual world* and that T_3 did not. The history of science provides ample evidence that scientists can and do reason this way.

Second, even if Shoemaker's intra-word/inter-world possibility principle is granted, LN's truth does not follow. This is because it fails to account for the (metaphysical) possibility that there could be *miracles*—i.e., cases in which the laws of a particular world are either violated or undergo change. In his explication of the Kripke-Putnam argument,

Shoemaker grants the intelligibility and relevance of nomologically impossible assumptions, such as there existing a perfect phenomenal duplicate of water, in determining which propositions are metaphysically necessary. He writes that

> the question they [Kripke and Putnam] address is that supposing that it is nomologically possible for there to be phenomenal duplicates of gold, tigers and water that differ from paradigm samples of these things in their underlying nature, these duplicates would count as instances of the kinds gold, tiger, and water. This question they answer in the negative—and expect us to agree with them . . . on the basis of our sense of how natural kind terms of this type are applied (1998, 69).

On these same grounds, we ought to be able to coherently consider possible scenarios in which there are "miraculous" violations of the laws. For example, consider what would happen if, at noon tomorrow, all the negatively charged particles of the type that orbit the nucleus of atoms were to acquire twice the charge of currently existing electrons. Even on the condition that electrons have their charge with physical necessity, it seems undeniable that at least *some* competent users of 'electron' would continue to claim that (1) electrons existed and (2) that they were now governed by different laws. In any case, there seems to be no *conceptual* barrier to our concluding that the laws governing the properties had changed. Insofar as law necessitarians are making the universal claim that all physical necessity is metaphysical necessity, they need to find some way of accounting for the intuition that such "miracles" are in some real sense "possible"[24].

The above examples seem to show that it is perfectly coherent to claim that a physically necessary proposition is nevertheless metaphysically contingent. Moreover, they show that Shoemaker's defense of LN, at the very least, requires some principle of modal epistemology that is stronger than those required for Kripke's argument. As explicated above, the Kripkean argument purports to show that some English sentences

[24]This argument does not require that all (or even the majority) of speakers recognize the possibility of miracles. Some scientists and philosophers, for example, might describe the above 'miracle' as an event revealing that the law governing the charge of electrons had a 'temporal index' such that it mandated different charges at different times (a bit like discovering that the emeralds we had thought were green were actually grue all along). My point here is simply that there is no reason to think that the speakers who do recognize the possibility of miracles are in some sense conceptually confused or incompetent.

express metaphysically necessary propositions despite making substantive claims about the world. It does so by appealing to competent speakers' explicit and implicit judgments concerning the application of their terms in certain counterfactual cases. In order to maintain the truth of LN, by contrast, its defenders need to find a principled way of *dismissing* the judgments of competent speakers. In the next section, I will consider the plausibility of two general principles intended to do just this.

4.5 IMAGINATION AND POSSIBILITY

If the arguments of the previous section are cogent, then the semantic argument for LN fails. There is no aspect of our ordinary way of talking about laws that unequivocally supports LN's truth, and the fact that both counterlegals and miracles are intuitively possible directly supports LN's falsity. LN still might be saved, however, if one of two things could be established: (1) that what people "say" or "imagine" is irrelevant to whether their sentences express metaphysical necessities or (2) that LN need not be committed to the metaphysical impossibility of every violation of a law of nature. I will conclude my discussion of LN by considering each of these strategies in turn. The first strategy is due to Shoemaker, who argues that the fact that we can "imagine" the laws being different does not imply that they actually could be; the second is due to Wilson, who briefly suggests a revision of LN that might put closer in line with our intuitions.

4.5.1 Imagination and Evidence

Shoemaker argues that our ability to "imagine" scenarios in which the laws are false cannot serve as evidence that we are committed to LN being false. He specifically argues that, insofar as we accept Kripke's claim that we are committed to water's being necessarily H_2O despite the fact that we can purportedly "imagine" it being false, we have reason to believe that anti-necessitarian arguments of the type above must be uncogent. In particular, Shoemaker argues that a reflection on the types of scenarios that we can possibly imagine supports the following principle concerning the relation between imagination and possibility:

> So there are three sorts of possibility we may reasonably come to believe in by reflecting on what we can imagine. There is the nomological possibility of phenomenal states of affairs. There is the epistemic possibility of non-phenomenal states of affairs. And there is the conceptual possibility of states of affairs of both kinds. But nowhere in all this, I submit, do we find any reason for supposing there are states of affairs that are metaphysically possible but not nomologically possible. (1998, 74)

His reasoning for each of the possibilities is as follows. First, if we can imagine something happening, we can be assured this is a type of experience we could have. Second, we can imagine perceiving things that we would take as evidence to indicate that the laws were other than what they were. Finally, insofar as we are not perfect judges of what propositions are possible and which are not, a proposition might be conceptually possible without being metaphysically possible. It is worth emphasizing this final point—on Shoemaker's account, the fact that a sentence expresses a conceptual possibility not only fails to *determine* that it expresses a metaphysical possibility but is in fact *evidentially irrelevant* to it.

If Shoemaker's characterization of the relation between what we can "imagine" and what is metaphysically necessary are correct, then the Carnap-Kripke method of investigating metaphysical possibilities is fundamentally flawed. This method consists, in essence, of querying speakers about whether they could imagine scenarios under which their sentences would be true or false and using these reports as evidence to determine whether or not their sentences express metaphysically necessary propositions. If Shoemaker is correct, querying speakers about what they can imagine is irrelevant to whether or not some sentence expresses a metaphysical possibility. If Shoemaker is correct, then, the cogency of Kripke's arguments is unaccounted for. In particular, it is unclear why Kripke's repeated appeals to what we would "say" or how we would "describe" various scenarios ought to be evidentially relevant to establishing his claims about the metaphysical necessity of certain propositions[25].

[25]There is in particular a marked contrast between the way that Shoemaker and Kripke deal with "competing" intuitions. Kripke, as I argued above, often rephrases the intuition prompt in order to get the reader to focus in on what he takes to be the major point. At various points, he explicitly grants that

There are at least two significant problems with Shoemaker's proposed principle. The first is that it is hard to see how such a principle can be reconciled with Shoemaker's own defense of LN[26]. In the argument described in the previous section, Shoemaker appeals directly to the reader's "sense" of how laws and properties interact, and to general principles regarding intra-world and inter-world possibility judgments. In particular, Shoemaker expects the reader to agree with him that a property cannot be governed by different laws at different times and places (in the actual world). If Shoemaker's principle concerning the role of imagination is correct, however, then the fact that we (as readers) cannot imagine a property subsisting through changes in its governing laws is simply *irrelevant* to whether it is metaphysically possible. The same holds for arguments such as Swoyer's, which appeal directly to imaginary scenarios to establish claims about metaphysical necessities.

The second problem is that, if Shoemaker's principles concerning the relations between metaphysical necessity and imagination are correct, then it is unclear what the explanatory advantages are of identifying physical necessity as a variety of metaphysical necessity. So, for example, consider the problem of determining whether or not some sentence expresses (1) an accidental generalization or (2) a law of nature. If LN were conjoined with a Carnap-Kripke view of modal epistemology, we would have a satisfactory explanation of how this could be determined—we would simply ask the speakers whether or not they would assent to the sentence in all possible cases. Once Shoemaker's principle is accepted, however, the defender of LN has no answer to the question "how can we tell whether something is a law of nature?" that is not also available to the primitivist about laws. The same problem arises when one considers the ontology of laws. What feature of the world, it might be asked, is responsible for making it the case that 'there are no spheres of uranium a mile in diameter' expresses a physically necessary truth while 'there are no spheres of gold a mile in diameter' does not? On Shoemaker's view, as on the primitivist view, this question remains unanswered. The

there is nothing he can say to convince those who do not share his intuitions. Shoemaker, by contrast, provides generalized reasons for dismissing (a certain class) of intuitions altogether.

[26](Korman, 2005) has offered a general argument that any law-necessitarian claim that we can ignore problematic "intuitions" is in considerable tension with the fact the main support for their own position is given by such intuitions.

physical necessity of the first cannot be, by Shoemaker's own principle, simply reduced to the fact that relevant speakers *intend* that it be true in all possible cases. Instead, it looks as if the physical necessity of the respective claims will depend on something like the underlying *essences* of uranium and gold. If this is the case, then LN no longer looks like a reductive account of physical necessity.

4.5.2 Is Law Necessitarianism "Almost" True?

A second way of defusing the problematic intuitions about counterlegals and miracles would be to show that they can be accommodated within LN. Wilson adopts this strategy. When considering Armstrong's argument that scientists regularly consider hypotheses on which the laws are different from those that actually hold, Wilson writes that defenders of LN can accept that "properties of the type that actually exist might be governed by laws that are very similar to the actual laws" and can therefore can "accept Armstrong's observation without accepting his view" (2005, 441). She gives no specific examples of the precise ways in which laws might vary, but her discussion suggests that she is committed (at the very least) to small variations in the various constants being metaphysically possible. In any case, Wilson's view seems to yield the intuitively correct result for at least some cases. So, for example, it is compatible with the intuitions that (1) it is metaphysically possible for electrons to have a charge that differs somewhat from the charge they actually have but (2) it is not metaphysically possible for electrons and protons to switch charges.

While Wilson does not defend this modified version of LN at any length, it has a certain *prima facie* plausibility that makes it worthy of serious consideration. Once one begins to spell out the potential implications of this position, however, a number of difficulties become apparent. First, it is unclear which of two distinct theses Wilson intends to advance: (1) that the laws of nature *themselves* do not entail definite, precise claims about the values of physical quantities or (2) that the laws of nature do entail such precise quantities, but that only some weaker proposition is metaphysically necessary. The first seems implausible, since many paradigmatic laws of nature make claims about precise values and scientists regularly rely upon such claims for a variety of theoretical

and practical endeavors[27]. In any case, Wilson's suggestion that the actual properties might be governed by "slightly different laws" properties suggests that she adopts some version of the second view. The view ("weak law necessitarianism") might be expressed as the following modification of LN:

LN_W : P is physically necessary $\Rightarrow$ The proposition that P *is approximately true* is metaphysically necessary.

I will assume that the notion of approximate truth is sufficiently clear for the present purposes. It is meant to capture Wilson's idea that the properties or entities of the actual world can exist only at worlds where some approximation of the actual laws hold.

There are at least two *prima facie* problems for LN_W. The first is that it, unlike LN, cannot account for the laws' distinctive relation to counterfactuals. If the laws are metaphysically necessary (and would thus be true in any world), we have an explanation for why they would continue to be true under specific counterfactuals. If on, the other hand, the laws are only approximately true in all possible worlds, we have no explanation for why the laws' *specific* truth is preserved under various counterfactual suppositions. So, for example, consider the counterfactual claim "Were it the case that I knocked the pen off the desk, then electrons would have only .99 of the charge they actually have." This claim seems false—were it the case that I knocked the pen off the desk, electrons would still have had the *same* charge they actually have.

A defender of LN_W might respond to the argument just given by noting that we do not need to appeal to the laws of nature to explain the falsity of this particular counterfactual—instead, we need only appeal to the Lewisian point that the world in which electrons have only .99 of their actual charge is less *similar* to the actual world than one in which they have the same charge. The problem is that examples can be constructed in which the small variations of the laws allowed by LN_W make a large

[27]It's important to distinguish the first view from the (true) observation that we, for epistemic reasons, may not have knowledge of these precise claims. So, for instance, we might have sufficient evidence to justify our believing it is a law of nature that electrons have the charge they do, but this evidence may not justify belief in a more precise proposition about the magnitude of this charge. What is implausible about the first view is that it claims that these latter claims (about the precise value of the charge) can fail to be physically necessary even when the more general claims (that electrons have a specific charge) are physically necessary.

difference. So, for example, consider a world in which a very powerful nuclear device can be set off by pushing a button. The button, in turn, is attached to a mechanism that sends a set number of electrons down a wire to an igniter. Finally, the igniter will trigger the bomb only if the total charge of the electrons falls within some narrow bounds. Suppose that this device is well-built and reliable at setting off explosions, but that even a small variation in the charge of the relevant electrons would lead it to it failing to go off. Now consider, the plausibly true counterfactual "If I were to push the button, a nuclear explosion would result." On the assumption that electrons have their charge with metaphysical necessity, LN successfully captures the truth of this counterfactual. LN_W, by contrast, may not. This is because LN_W is committed to the metaphysical possibility of worlds in which the electrons have charges that would *not* set off the bomb. Some such worlds would undoubtedly be more similar, at least in terms of non-nomic facts, to the actual world than one in which the bomb explodes. It thus looks likes LN_W is committed to the bomb *not* going off on the supposition that the button was pushed.

The second problem for LN_W involves its possible incompatibility with MN. The plausibility of LN_W is tied to its requirement that it entails only that small, quantitative (and not large or qualitative) changes in the laws are metaphysically possible. So, LN_W is committed to the possibility of a world in which electrons have a *slightly* greater charge than the actual charge. The problem arises when one considers what one should say about the physical and metaphysical necessities of *that* world. Presumably, the electrons there have *their* charge with physical necessity, and it is metaphysically necessary that they have approximately that charge. Relative to this world, it should thus be metaphysically possible that electrons have a still greater charge. If we repeat this process, we will eventually arrive at a world in which the electrons have a *vastly* different charge than the actual world. Intuitively, we want to say that such a world does not represent a metaphysically possible case. Yet, the defender of LN_W seems committed to it.

If the above argument is correct, then defenders of LN_W seem to be vulnerable to a version of Chandler's (1976) "transitivity argument." The argument could be structured as follows: suppose we are in w_1 with laws L_1. Law necessitarians wish to say that world w_2, which has slightly different laws L_2, can still have the same properties as our

w_1. The problem arises when one considers worlds such as w_3 with laws L_3 which are slightly different than L_2 (and even more different that w_1). If w_3 represents a metaphysically possible way that the properties of w_2 could be, and w_2 represents a way that the properties of w_1 could be, it seems that w_3 represents a way that the properties of w_1 could be. The process can be iterated *ad infinitum*. At some point, however, we will get to a world w_n with laws L_n such that the laws of this world are radically different than those of w_1. Defenders of LN_W presumably wish to deny that such a world could contain the same properties as the actual world. However, it is unclear how this consequence could be avoided. The necessitarian could deny MN, and could instead claim that which propositions are "metaphysically necessary" is relative to which world one is in (and thus, deny that metaphysical necessity is transitive). Conversely, they could reject LN_W's claim that even small variations in the laws are metaphysically possible. I take both of these options as unpalatable. In any case, I have proceeded upon the assumption that metaphysical necessity can be characterized by MN, and theses that deny this are beyond the scope of this chapter.

4.6 CONCLUSION

If the arguments presented in this chapter are correct, then at least some laws of nature are not metaphysically (or logically) necessary, and physical necessity is not simply a special type of metaphysical necessity. In particular, I have argued that the principles relied on by law necessitarians to dismiss anti-necessitarian arguments are in considerable tension with the necessitarian goal of offering an explanatory, reductive account of physical necessity. Instead of clarifying the concept of physical necessity, such principles risk muddying the concept of metaphysical necessity. For the purposes of this chapter, I have remained neutral as to whether the principles concerning metaphysical necessity appealed to by Shoemaker and Wilson are correct, though my strong intuition is that they are not. In any case, if such principles are adopted, necessitarians will need to provide an account of how it is that we can have epistemic access to such necessities[28].

[28] A second reason for rejecting LN, and one that I will not pursue here, involves its inability to explain the laws' distinct relation to counterfactuals. Both Lange (2004) and Schaffer (2005) have argued that LN cannot explain the fact that the laws remain true under every physically possible antecedent. So, for

There is a broader point here as well. If both Humean reductionism and law necessitarianism are false, and if there are such things as laws of nature at all (i.e., eliminativism about about laws is false), then it seems that facts about laws are *primitive* in a fairly strong sense—the facts about laws do not reduce to (and do not supervene upon) the facts about the entities they govern, even if this reductive base is expanded to include things such as essences or dispositions. Moreover, it is possible for there to exist pairs of worlds with identical inhabitants but which have divergent laws. The irreducibility of laws, in turn, raises a number of problems for other popular reductionist projects. So, for example, many accounts of causation, counterfactual truth, and physical probability appeal to the laws of nature. While such accounts might serve as valuable explorations of the relationship between laws and these other nomic phenomena, one cannot claim that such accounts succeed in reducing the nomic features of the world to the non-nomic features.

example, consider the counterfactual antecedent "Were there to exist an electron at location X . . . ." LN can presumably explain (by appeal to the semantics of 'electron') why the electron would have a charge of $-e$; it cannot explain why the world considered by this counterfactual would not contain schprotons (and not protons) with a charge of $+1/2e$.

CHAPTER 5

CONCLUSION

In the previous four chapters, I have offered a sustained argument against the thesis that strong laws—that is, the laws of nature that fundamental physics aims to discover—can be reduced to other sorts of facts. In chapter 1, I presented and defended the Carnapian methodology of explication, according to which claims in philosophical metaphysics are to be evaluated by comparison to concepts of ordinary language and scientific practice. This methodology requires that we maintain a clear distinction between the concepts of law of nature that are currently in use and the concepts that are the outcomes of various philosophical proposals. In chapter 2, I applied this methodology to the case of laws, and argued that there are in fact two types of laws—strong laws and weak laws—that philosophers have so far failed to distinguish. In chapters 3 and 4, I argued that the facts about laws cannot be reduced to either (1) the non-modal, occurrent facts concerning the entities that the laws "govern" or (2) the facts about logical or metaphysical necessities.

In this brief closing chapter, I'll take another look at some of the main conclusions of the dissertation, but this time with an eye toward seeing how they relate to some broader philosophical themes and concerns. In particular, I'd like to suggest that the arguments presented here, if cogent, have consequences for the correct way to think about methodology in philosophical metaphysics. To conclude, I'll present some suggestions for further research that, for reasons of time and space, were left out of this dissertation.

5.1 MAJOR THEMES AND CONCLUSIONS

While the conclusions of this dissertation have been explicitly constrained to laws of nature, I think they are also of more general philosophical interest. In this section,

I'll discuss some of the broader implications of the arguments that have been offered so far.

5.1.1 Explication and Metaphysics

One prominent, though often implicit, concern in this dissertation has been metaphysical methodology. So, for example, many of the arguments in this dissertation concern such things as (1) the relation between metaphysics and conceptual analysis, (2) the appropriateness of various standards for evaluating metaphysical claims, and (3) the cogency of various principles that have been used to defend metaphysical claims. My arguments, if correct, suggest that certain received views about these issues may be mistaken.

So, for example, in chapter 1, I defended a version of Carnapian explication, according to which claims in metaphysics ought to be interpreted as proposals for the adoption of new concepts. This method emphasizes the importance of articulating and maintaining clear distinctions between the vague concept (or concepts) of law inherent in ordinary language and scientific practice and the precise analyses of law of nature that philosophers have proposed as potential replacements for these concepts. I then compared metaphysics-as-explication with two rival views: (1) the view that metaphysical claims are descriptions of preexisting concepts and ought to be evaluated accordingly, and (2) the view that metaphysical claims are to be interpreted as novel claims about reality and that their evaluation has nothing to do with concepts at all. I argued that metaphysics-as-explication has a clear advantage over both of these views: it allows us to formulate clear, relevant evaluative criteria that recognize both the importance of our preexisting concepts and the need to improve upon them.

In chapter 3, I argued that the laws of nature do not reduce to "history" (i.e., the totality of non-modal, occurrent facts). In the process of making this argument, I considered a variety of methodological principles that have been offered in support of reductionism. These principles were

1. that intuitions/judgments are evidentially irrelevant if they are shown to have a "suspicious history,"

2. that more parsimonious metaphysical claims are more likely to be true, and

3. that all truths must be "grounded" in a particular manner.

I argued that all three of these principles are misleading at best and false at worst. If my arguments were accurate, then this will have implications for a number of debates in metaphysics and the philosophy of science. So, for instance, many of the arguments in favor of process theories of causation or actual frequency theories of probability lean on appeals to principles like these; if I am right, these arguments will need to be reassessed.

5.1.2 Modal Reasoning

Throughout the dissertation, and in particular in chapters 3 and 4, I have presented a number of arguments with modal conclusions. While these arguments were developed with the goal of establishing a specific conclusion about laws, I think that they also have broader implications for the way we think about conceivability and possibility.

In Chapter 3, I offered the "Prima Facie Argument" against reductionism about laws. One subconclusion of this argument was that it is (metaphysically) possible for worlds to have identical histories but different laws. In order to establish this modal conclusion, I appealed to a number of judgments (or "intuitions") regarding the possibility of various worlds. In particular, the argument appeals to the intuition that moving a mirror in a physically possible way cannot change the laws of nature. On Carroll's (1994) original argument, not much is said about *why* we ought to take this intuition to be evidentially relevant to the truth of the corresponding modal claim. In chapter 3, I have provided a preliminary answer: the intuition is evidentially relevant because it represents the judgment of competent users of the law-concept and, absent evidence of inconsistency, we have good reasons to trust these sorts of judgments.

In chapter 4, I spelled out the relation between the intuitions of competent speakers and modal epistemology at greater length. In particular, I argued that the "Carnap-Kripke method" offers an example of how one might confirm modal claims without recourse to any mysterious properties and relations; moreover, I suggested that this method is consonant with Carnap's and Kripke's historically influential accounts of modal reasoning. In this chapter, I wasn't concerned to argue for this account of modal

reasoning against rival views; instead, I aimed merely to show that there are significant disanalogies between the modal arguments offered by law necessitarians and by modal arguments that are widely recognized as cogent. I think that more might be said here, however, if this argument is successful. After all, if the Carnap-Kripke method can explain paradigmatic cases of modal reasoning, it has much to recommend it over competing accounts. In particular, it does not carry with it any of the counterintuitive consequences (e.g., to hidden essences or to the existence of possible worlds) that many have thought are required to make sense of modal claims.

5.1.3 Science, Philosophy of Science, and Metaphysics

This dissertation is located squarely in the intersection between metaphysics and the philosophy of science. Some of the issues discussed, such as the role of laws in the special sciences or the cogency of underdetermination arguments for reductionism about laws, are usually thought of as debates in the philosophy of science. Others, such as the truth or falsity of Humean Supervenience or law necessitarianism, seem to fall more naturally under the heading of "metaphysics." Part of the goal of this dissertation, however, has been to show how debates in these two subdisciplines relate to one another, and how both relate to the discussions of laws of nature that occur in scientific practice and everyday life.

In chapter 1, I suggested that at least some of the divergence we see between competing philosophical accounts of laws of nature can be attributed to the fact that there is a divergence of *goals*. Certain philosophical analyses aim to provide accurate (or nearly accurate) descriptions of actual practice, and diverge from such practice only in the face of explicit, internal inconsistency. Other philosophical accounts seek to locate laws within a far more comprehensive picture of reality and are thus less beholden to capture the idiosyncrasies of actual practice. On the explication-based methodology I defend, this sort of variety is to be expected and commended; after all, it might well be the case that different sorts of philosophical accounts work better for various purposes. Given this variety of goals, however, it becomes especially important to keep sight of *what we are trying to do* in proposing a certain philosophical account.

In chapter 4, I returned to this theme again, this time in the context of modal facts. It has sometimes been thought that the sort of *a posteriori* necessities posited in Kripke's *Naming and Necessity* are incompatible with a naturalistic methodology according to which there are no hidden "essences" built into nature. In this chapter, I suggested that this conflict can be partially resolved by noticing that Kripke's methodology is, in essence, a logical extension of Carnap's (seemingly ontologically innocuous) methodology in *Meaning and Necessity*. In both cases, I argued, the goal is the same—to determine whether a given sentence of a natural language expresses a metaphysically (or logically) necessary truth. Kripke simply recognized that speakers may use certain words *conditionally*—that is, they intend that their words pick out different things, depending on what happens to be the case. My main claim here was that we can recognize the cogency of Kripke's arguments—and the existence of *a posteriori* necessities—without committing ourselves to anything that Carnap's methodology does not already commit us to. Insofar as contemporary analytic metaphysics is dominated by discussion of a posteriori necessities, I take this to be a substantive result, though certainly one that will need to be unpacked further.

5.2 DIRECTIONS FOR FURTHER RESEARCH

In this project, my main focus has been on defending nomic primitivism. In future work, I hope to explore in more detail the form an adequate thesis of nomic primitivism should take. In particular, I plan to look more closely at the relationship between laws and counterfactuals, which is one primary means by which various versions of nomic primitivism can be distinguished.

I have argued that one of the central features of strong laws is their support for counterfactuals. Moreover, in chapter 2, I suggested that (at least according to our ordinary concept), this support has a specific form. In particular, I claimed that the following counterfactual was true, for any strong law L and any physically contingent proposition P: *If P were to be the case, then L would still have been a law.* I did not offer any extended argument for this principle, however, and did not compare it to some of the prominent alternatives that have been proposed in recent years. Here, I briefly outline

two of the alternative principles that have been proposed, together with some of my reasons for thinking that they might be inadequate.

5.2.1 Lange on Non-backtracking Counterfactuals

Marc Lange (2000; 2005), a prominent defender of non-reductive realism about laws, has argued that there are physically possible counterfactual antecedents under which the laws fail to be "preserved" (that is, if these antecedents were to be true, the laws would be false). More specifically, he argues that there are counterexamples to what he calls the *non-nomic stability* of laws, which would also entail that the thesis mentioned above is false.

On Lange's account a set of true propositions Γ is non-nomically stable just in the case that there is no counterfactual supposition S such that (1) S is consistent with all of the members of Γ being true, (2) S makes no claims about laws or physical necessities, (3) the counterfactual *If S were the case, then at least one member of Γ would be false* is correct/true. So, if the set of laws fails to be non-nomically stable, this means that there is some physically possible counterfactual antecedent such that, were it the case, a law would fail to hold.

While Lange argues that counterfactual support and non-nomic stability are central to the idea of lawhood, he argues that the set of laws is only approximately stable. He offers two reasons for this: first, that the laws of the special sciences can't belong to a non-nomically stable set (since they are false); second, that the laws fail to be preserved when counterfactuals are evaluated in "non-backtracking contexts." If my argument in chapter 2 was correct, the first reason isn't persuasive, since the laws of the special sciences are weak laws, while those of fundamental physics are strong laws. I argued that it was only the latter sort of laws that bore the distinctive relationship to counterfactuals.

Lange's second reason, which relates to so-called "non-backtracking counterfactuals," is a bit more difficult to dismiss. A backtracking counterfactual is a counterfactual of the form $p > q$ where q is actually false and is temporally prior to p. A non-backtracking counterfactual is, by contrast, one in which q is actually false but is temporally subsequent to p.

Lange offers the example *were Darcy to ask Elizabeth for a favor, she would not have granted it to him* and specifies that it is to be considered in a context in which Darcy and Elizabeth have recently had an argument. Lange then offers the following succinct argument that not every law could be preserved under this counterfactual antecedent:

> Now the problem is that if the actual laws are deterministic and their truth is preserved in the closest possible world, and if the period in that possible world before Darcy makes his request is just as it is in the actual world ... then the rest of the possible world's history must also be the same as the actual world. But this precludes Darcy's asking Elizabeth for a favor ... So the actual laws cannot be preserved in this world. (2000, 73)

To summarize: if the past were the same and the laws were the same, Darcy could not have asked Elizabeth for a favor. So, the supposition requires that *either* the past is different or the laws are different. Lange suggests, however, that the context *requires* that we preserve the past (i.e., it requires that we interpret it as a non-backtracking counterfactual). He concludes from this the laws are not preserved under the counterfactual supposition, even though they are not logically incompatible with it. This demonstrates that the laws are not non-nomically stable.

My suspicion is that Lange has been too hasty here, for at least two reasons. First, even if we assume that the laws are deterministic, it is not clear we have to accept Lange's conclusion. Lange claims that because we are interested in showing how Elizabeth (as she is at the time of our supposition) would respond to Darcy's request, the context *requires* that we preserve the actual past. We might be able to accomplish this same goal, however, by supposing that the initial conditions of the universe were slightly different in a way that led to Darcy momentarily forgetting the conflict and asking Elizabeth for a favor. Our interests, after all, require merely that the counterfactual Darcy and counterfactual Elizabeth resemble the actual Darcy and Elizabeth with respect to request-granting. A second possible response might appeal to the (implicit) physical impossibility of the antecedent. Specifically, if it could be shown that the preservation of the actual past is implicitly contained in the antecedent, then the antecedent is physically impossible (since it isn't logically compatible with the laws still holding) and is

thus no threat to either non-nomic stability or the principle concerning counterfactuals I've defended. In any case, this issue needs to be looked at more closely.

5.2.2 Woodward on Laws, Invariance, and Scope

James Woodward's manipulability account of explanation (2003; 2004) has seen widespread acceptance in recent years. Like Hempel's classic DN model, Woodward's manipulability account entails that laws often feature prominently in explanations. This is in accordance with the view of laws defended in this dissertation, as is Woodward's defense of non-reductive realism about laws, causes, and counterfactual truth. However, unlike the account I have defended, Woodward argues that there is no *distinctive* relation between counterfactuals and laws of nature (strong laws), or between such laws and explanation. Instead, Woodward argues that causal generalizations occupy a sort of continuum of *scope*, *range of invariance*, and *usefulness*. He suggests that the principles I've identified as strong laws are simply generalizations with wide scopes and ranges of invariance, the knowledge of which is central to projects that humans find important.

Woodward offers a nonreductive account of causal explanation that ties successful explanation to the truth of certain counterfactuals. Roughly, the idea is that a variable X taking the value x causes a variable Y to take the value y just in case that there is some way of changing or manipulating the value of X such that the value of Y would change as well. If this is the case, then we can *explain* Y taking y by citing a generalization G that describes a different value Y would take, under at least some of these manipulations of X. Woodward explicates the relevant notion of "change" or "manipulation" via the nomic concept of an *intervention*. Specifically, I assuming value i counts as an *intervention* on X with respect to Y if and only if (1) I causally determines the value of X in such a way that the value of X does not depend on any other causes of X, (2) I does not causally affect Y, unless it does so by changing X and (3) I's assuming value i is the cause of the actual value taken by X(Woodward, 2003, pp.93-94).

With this general picture of causation and explanation in mind, Woodward's analysis of law of nature can be presented. He says that

> paradigmatic laws are simply generalizations with wide scope that are invari-

> ant under a large and important set of changes that can be given a theoretically perspicuous characterization. We are willing to regard other invariant generalizations as laws to the extent that they resemble these paradigms in these respects. (2003, 286)

The *scope* of a generalization varies according to the number of actual systems whose behavior can be described and explained using the generalization in question (2003, 270). So, for instance, the weak laws of Newtonian mechanics have a relatively wide scope, since they can be used to describe a great number of actual systems (e.g., masses not at light-like speeds or in strong gravitational fields). Where scope is defined with respect to the actual world, *invariance* is characterized counterfactually; a generalization's invariance depends (roughly) on the number of interventions it would continue to hold under. It is the invariance of laws, according to Woodward, that accounts for their explanatory power.

If Woodward's account is correct, then my claims about the distinctive relation between strong laws and counterfactuals is false; moreover, his account entails that I am incorrect in drawing a sharp line between strong laws and weak laws. However, my suspicion is that Woodward's attempt to define laws in terms of counterfactual invariance fails to account for the judgment that there must be *some* distinction between the necessity of laws and the necessity of accidental generalizations. This worry stems from the fact that Woodward defines invariance with respect to *all logically possible interventions* (2003, pp.129-131). If this is the case, then there is little reason to suppose that we will be able to rank causal generalizations in order of "invariance" in a manner that would allow us to determine which generalizations should count as laws. After all, there are presumably an infinite number of logically possible interventions under which *any* generalization would be invariant, irrespective of whether this generalization is a law. If this were true, then it looks like all generalizations would have equivalent invariance and thus, that Woodward's central causal concept would be irrelevant to distinguishing laws from non-laws. Instead, laws would be distinguished by merely their wide scope and usefulness, which seems contrary to the spirit of Woodward's proposal. This is just the sketch of an argument, of course, and much more work is needed to determine what, if anything, might be said in defense of an invariance-based characterization of

strong laws.

5.3 FINAL THOUGHTS

In this dissertation, I have argued that the facts about the laws of nature are primitive. If this is correct, then many of the current philosophical approaches to laws, which are premised on various forms of reductionism, are deeply flawed. The acceptance of nomic primitivism does not mean that the philosophical study of laws should be abandoned, however. A correct understanding of laws is crucial to making sense of many aspects of scientific practice, including the confirmation of theories, the structure of successful explanations, and the role played by claims about counterfactuals, causes, and probabilities. Philosophers are well-positioned to contribute to the further understanding of laws, so long as they pay close attention to actual scientific practice. The study of laws thus remains a vital and exciting research program.

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