Metaphysics of Laws of Nature

by

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ABSTRACT

I argue that, because fundamental scientific theories are attempts to tell us something about reality, we are required to take into account the metaphysical features of those theories. I claim that a guarded realism is the proper stance to take toward fundamental scientific theories: philosophically, one must guard against accepting every posit at face value. Laws of nature are one of the posits of fundamental scientific theories and, since they are part of the nomic, I argue that they cannot be eliminated from the ontology of the world. I consider whether the nomic can be reduced to the Humean base and argue that doing so leaves us with no metaphysical explanation for the regularities we observe. I agree with Galen Strawson that the world requires a metaphysically real glue to hold it together and argue that this glue is accounted for by reifying the nomic and not reducing any of the nomic concepts to the Humean base. I argue, against Helen Beebee, that a regularity theorist about laws of nature and causation makes the world out to be a world without reasons for the regularities, which is not acceptable. I consider the Best System Analysis of laws of nature in conjunction with Humean Supervenience and show that it is not able to account for objective chance in a metaphysically acceptable way. I then turn to Armstrong's contingent relation among universals account of laws of nature and consider Bird's ultimate argument against it. I argue that one way to overcome the argument is to allow that some universals have nontrivial modal character, which is an acceptable solution for the nomic realist.

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Crito, I owe a cock to Asclepius; will you remember to pay the debt?
— Socrates's last words in Plato's *Phaedo*
(translated by Banjamin Jowett, 1892)

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Dedication

To Howie Card and Don McCarthy

Physics is like sex: sure, it may give some practical results,
but that's not why we do it.
—Richard P. Feynman (1918-1988)

The most tempting of all the false first principles is: that *thought*, not *being*, is involved in all my representations. ... Man is not a mind that thinks, but a being who knows other beings as true, who loves them as good, and who enjoys them as beautiful. For all that which is, down to the humblest form of existence, exhibits the inseparable privileges of being, which are truth, goodness and beauty.
— Étienne Gilson (1884-1978)
(from 1937, p. 255)

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I — Introduction and Overview

Resignation in the face of the follies of mankind
is the price of wisdom. And true wisdom wonders
whether wisdom is not folly too.
But to blur the line between what we know and do not know,
between the folly of knowledge and the folly of ignorance,
and to curry favor with the mass of men
by abandoning "truth" to them
without requiring any effort on their part,
that is not wisdom but contempt of truth,
whether it be cynicism or romanticism.
— Walter Kaufmann (1921-1980)
(from 1958, p. 75)

1. Introduction: The Scientific World View

It might seem that the question of why something is the way it is can always be asked, but there are serious scientific efforts being expended on developing a *Theory of Everything*: the one set of equations (*i.e.*, laws of nature) from which all else is derivable. The claims of science go almost unchallenged by the lay person who has acquired a faith in the proclamations of science. Through their books, radio and television programs, and magazine articles, popular science writers have inculcated a popular view of science as providing us with the basic principles (*i.e.*, theories, rules, laws) upon which the, mostly rational, world can be understood.[1] Popular works that try to convey the content of

1. See, for example, some of the writings of Paul Davies (*e.g.*, 1992), who is himself a mathematical physicist. At this point I'm not differentiating between theories, laws, rules, *etc*.

scientific theories and how they are derived typically ignore the philosophical issues regarding how those theories support a consistent philosophical world view.

The modern lay person has relinquished her world-view-backed-by-religious-principles in favour of a world-view-backed-by-the-authority-of-science. But where does such a person, who is mostly untrained in the methods of science, get her purview of science and its principles? The backing of her world view comes, not from a deep understanding of science, but from a general faith in the scientific enterprise, and she justifies that faith by the success of science. Therefore, that faith is more the result of technological advancements that have provided for a comfortable living than from a clear understanding of scientific methods, theories, and laws of nature. The *success* of science is, not a success in its conception of reality, but rather an engineering success: the results and methods of science have been instrumental in providing technological advancements.

The scientific enterprise has become so specialised that specific areas of science are obscure to most people, even to scientists not versed in those areas. Thus, it is difficult for scientists themselves to have a clear philosophical world view that is justified by science. During the late 1900's and the early part of the last century, confidence in the methods of science wasn't limited to its utility, but also included its ability to answer some of the deepest questions that are traditionally considered part of philosophy. But, as the story is often told, things changed during the middle and latter parts of the twentieth century, due mostly to the development of electromagnetic field theory, general relativity, and especially quantum mechanics and quantum field theory.

In his highly influential book *Word and Object*, the philosopher Willard van Orman Quine tells us that "What reality is like is the business of scientists, in the broadest sense, painstakingly to surmise; and what there is, what is real, is part of that question."[2] Apparently, Quine had high hopes for science, but a closer look at his philosophy reveals his scepticism that the posits of science can be taken as literally true descriptions of reality. In 1951 he writes,

> [a]s an empiricist I continue to think of the conceptual scheme of science as a tool, ultimately, for predicting future experience in the light of past experience. Physical objects are conceptually imported into the situation as convenient intermediaries not by definition in terms of experience, but simply as irreducible posits comparable, epistemologically, to the gods of Homer. Let me interject that for my part I do, qua lay physicist, believe in physical objects and not in Homer's gods; and I consider it a scientific error to believe otherwise. But in point of epistemological footing, the physical objects and the gods differ only in degree and not in kind. Both sorts of entities enter our conceptions only as cultural posits. The myth of physical objects is epistemologically superior to most in that it has proved more efficacious than other myths as a device for working a manageable structure into the flux of experience. (1951, p. 41)

An underlying tenet of my thesis is that science, in and of itself, does not substantiate a *single* philosophical world view: neither a single metaphysics nor a single epistemology. If we are looking to science *qua* science, past, present, or future, to answer the most fundamental philosophical questions about our world and our existence in it, then we are bound to be disappointed. The trite *obiter dictum* that a metaphysical account of reality has been supplanted by a scientific account is more a reflection that the boundary between scientific and metaphysical explanation has moved towards the former than that metaphysical explanations are no longer required. Although the existence of some sort of boundary is generally accepted, the demarcation criteria for the boundary are clearly not,

2. Willard van Orman Quine (1960, p. 22). As an indication of the influence this book has had, my own copy is from the 23rd printing, which came out in 1999! And this for a technical book dealing with the analytic philosophy of language.

and I will argue that there can be no clearly defined boundary based solely on epistemological grounds.

When philosophers appropriate *too much* of reality for their metaphysical work, they are accused of doing science from their armchairs; when scientists make elaborate claims regarding reality based on their scientific theories, they are either ignored by philosophers (when the claims are philosophically naïve) or accused of reading too much into their theories. Within a scientific theory, finding the boundary between the metaphysical and the empirical claims usually involves choices on both sides of that boundary. In addition, those choices will typically depend on the particular nature of the scientific theory being considered: some scientific theories are purely descriptive, agnostic about the entities they quantify over, but with demonstratively instrumental value, whereas others are realistically construed theories with specific posits but can have little or no instrumental value.

Scientists require that the philosophical implications of their theories be clarified so that they can better make the theoretical choices that can be based only on a clear philosophical understanding of those choices. Sometimes, when scientists cling to terminology that is ambiguous with respect to the metaphysical possibilities, it is up to the philosopher to expose the terminological ambiguities and make the metaphysical options clear for the scientist. But it is not up to the philosopher to resolve terminological ambiguity when it is clear that a univocal resolution is not possible.

When it comes to the philosophical underpinnings of the laws of science and their relationship to the laws of nature (should they exist),[3] the philosophical situation is quite messy. Recently, John Earman has characterised the status of the current philosophical literature with regard to laws as a scandal in the philosophy of science. He accuses some modern writers on laws of nature of scholasticism and writes,

> **Laws: A Scandal in the Philosophy of Science**. It is hard to imagine how there could be more disagreement about the fundamentals of the concept of laws of nature—or any other concept so basic to the philosophy of science—than currently exists in philosophy. A cursory survey of the recent literature reveals the following oppositions (among others): there are no laws versus there are/must be laws; laws express relations among universals versus laws do not express such relations; laws are not/cannot be Humean supervenient versus laws are/must be Humean supervenient; laws do not/cannot contain *ceteris paribus* clauses versus laws do/must contain *ceteris paribus* clauses. (2004, p. 1228)[4]

I think that a major contributing factor to this scandal is the way that some philosophers try to map anything that a scientist refers to as a law onto a philosophically consistent conception of a law of nature. Can the laws of thermodynamics, Newton's laws of motion, Maxwell's equations governing the electromagnetic field, Einstein's gravitational field equation, the uncertainty principle, and Schrödinger's equation be mapped onto a univocal metaphysical conception of laws of nature? If the answer is no, it might not be because these laws of science are only approximations to the true laws of nature. It might be because these laws of science are about different metaphysical structures. It is important to first abstract out the metaphysics from the laws of science before one attempts to structure that metaphysics.

The problem is, to some extent, also a result of the different scientific understanding and competency of some of the philosophical writers. The specialised areas of science are

3. In this thesis, where it is crucial for understanding, I differentiate between laws of nature, as metaphysical entities, the law-propositions associated with those entities, and the law-propositions of science, as determined by science and constituting one of the aims of scientific enquiry. It is one of the goals of this thesis to clarify the differences and the relationships among these things.
4. This is a reprint of his 2002 presidential address at the Philosophy of Science Association 18th Biennial Meeting.

technically demanding and gaining a competency in those specialised methods requires a huge time commitment. This point is identified by Earman when he writes that

> [p]erhaps because of the formidable technical challenges they pose, philosophers have tended to shy away from the foundations of physics problems that I will identify. And perhaps because they are embarrassed to be seen to be doing metaphysics, philosophers of science have been reluctant to take up the philosophical issues. (*ibid*., p. 1227)

But Earman urges philosophers not to be afraid of delving into the technicalities of physics so that they can reveal the philosophical problems (*and thereby* identify the point where physics becomes metaphysics). I'm urging the reverse: philosophers competent in scientific methods can make contributions to the metaphysics that science needs.

Quite often in the philosophical literature of the past few decades, we see laws of nature characterised as having one of two forms, $\forall x(Fx \rightarrow Gx)$ or $F\text{-}ness \rightarrow G\text{-}ness$, and whole philosophical treatises have been written on the metaphysics of such laws. It is of course true that there are many metaphysical issues that can be discussed based on such a characterisation, but it is unlikely that all the metaphysical issues can be resolved conclusively without looking more closely at some of the current laws of science (from which those forms were abstracted). For example, the first form identifies a regularity in the properties a thing in our domain of discourse has: it says that, if a thing has the property F, then it also has the property G. The form is neutral on the question of nominalism or realism with respect to universals. But, as a sufficient condition for a law of nature (*i.e.*, that any truth expressible in this form is a law of nature), it might be that it allows too many accidental truths to be laws of nature. The second form, on the other hand, might be taken to indicate the existence of *properties* at the fundamental level of reality. It might be thought that this is a purely metaphysical conception of laws of nature that is of no concern to the philosophy of science, until one realizes that the struggle among scientists, in the late 1900's and early part of the twentieth century, to understand the electromagnetic field was a struggle about whether to accept the existence of a property that is unattached to any particular: the particular being sought, which would be clothed by the electromagnetic field, was named the luminiferous ether. When the idea of the luminiferous ether was abandoned, the electromagnetic field was pronounced to fundamentally exist by some and accepted only as a convenient fiction by others. When it became clear that the electromagnetic field itself transferred energy and had an associated momentum, it became more difficult to argue that it was just a convenient fiction. Finally, when quantum electrodynamics (QED) gave it a particulate nature (granted, a mysterious chancy particulate nature), the electromagnetic field became known as the photon, a respectable posit of physics.

2. Overview of Thesis

The main issue that I tackle in this thesis is whether a consistent metaphysical theory of laws of nature and the associated nomic concepts can be established. By a consistent metaphysical theory I don't mean only self-consistent, for I assume that this is a prerequisite that any metaphysical theory should satisfy, but rather consistent in two other important aspects: (i) the metaphysical theory should be consistent with the other metaphysical theories backing such concepts as causation, natural necessity, and objective chance, and (ii) it should be consistent with, and support, our epistemological theories of science and, more specifically, the laws of science.

Before tackling these issues, I will articulate, and argue for, the conception of science I'm assuming as the background and justification for the philosophical discourse. In Chapter II, I consider the relationship between science and metaphysics and argue that a consistent metaphysics that is sensitive to the posits of science is, in fact, required by science for making choices about scientific theories. I claim that this requires a substantial, but guarded, realism with respect to the things posited by scientific theories: entities, laws of nature, causal forces, and things that govern the events of the world. This realism, I claim, is what allows scientific theories to explain reality. I consider and reject the argument that laws are not part of scientific theories. Next, following Stathis Psillos, I defend a characterization of scientific realism that breaks it up into three theses: metaphysical, semantic, and epistemological. I briefly consider some purported fundamental laws of science so as to extract the essential metaphysical features that we might claim for a metaphysics of the laws of nature, and I argue that such a metaphysics shouldn't, in and of itself, disallow certain features of the world.

In Chapter III, I consider what a guarded realism delivers to the ontology of the nomic, explanation, and governance. I review the ontological categories of primitivism, reductionism, and eliminativism and apply these categories to the laws of nature. Laws of nature are part of a family of so-called nomic concepts, so I briefly consider what these other concepts are. One kind of Humean scepticism about the nomic results from the belief that there *cannot* be any natural necessity in the world, because the only necessity is logical necessity. I argue that when the nomic realist speaks of natural necessity, she of course doesn't think that it's the same thing as logical necessity, but, once an abductive inference to real non-Humean laws is made, logical arguments using those explanatory laws can be used to make predictions of future causal events. The laws that are inferred metaphysically explain the regularities they are inferred from. I then give a brief review of regularity accounts of lawhood and causation and argue that such accounts cannot be used for metaphysical explanations of regularities and no explanation can be given for future instances of causal regularities. An inference to the best *causal* explanation type of rationality requires the acceptance of some realism about the nomic. That realism includes reasons for the regularities we observe. I argue against Helen Beebee's recent argument that brute fact regularities are an acceptable stopping point for metaphysical explanation.

In Chapter IV, I consider whether objective chance can be united with David Lewis's Best System Analysis (BSA) of laws of nature in conjunction with Humean Supervenience (HS). I review and accept Lewis's Principal Principle as the obvious rule for relating subjective credence to objective chance: a principle that tells us how to set our degrees of belief about objectively chancy situations. Lewis's so-called *big bad bug* infests the Principal Principle when it is held with HS, and I argue that his proposed modification should be rejected because it supposes that the world's theory of chance is itself chancy. A chancy theory of chance is (1) metaphysically unacceptable in conjunction with BSA and HS and (2) entails that we have no way of knowing how our credence relates to chance. I argue that the only way to make sense of the Principal Principle is to assume that chance laws do not supervene on the Humean base.

In Chapter V, I use an argument against the claim that Humean scepticism is justified because we have no epistemic access to the nomic to justify our belief in un-reduced properties and the nomic. The lack-of-epistemic-access argument against the nomic is unconvincing, because even a regularity theorist must accept some form of non-reductionist realism about properties—otherwise she leaves it mysterious just how we can ever come to know about regularities in the first place—and, once she has accepted that sort of realism about properties, that is not much different from realism about nomic entities. My argument doesn't tell us what sort of primitivism we should embrace regarding the nomic, so I then go on to consider Armstrong's contingent relation among universals account of laws of nature and review Bird's recent, so-called, ultimate argument against it. I agree with Bird that a primitive modal feature should be part of the CRU account and consider how such a modal feature could be included in the CRU account of laws of nature. Specifically, I allow that the universals that are laws of nature, themselves, might have modal features, necessarily conferring the associated regularity on the world. I claim that it is a natural part of the scientific attitude that laws of nature do confer some sort of natural necessity on the world and so, from the point of view of science, this is not an objectionable thing to do. The alternative is to posit properties having powers, but I don't consider that possibility in detail.

I conclude the thesis by speculating on what an appropriate future metaphysics might look like and what the open philosophical questions of interest to science might be.

II — Science and Metaphysics

> The issue, in a nutshell, is whether the first person, singular or plural, is hiding at the bottom of everything we say or think.
> — Thomas Nagel (1937 -)
> (from 1997, p. 3)

> One is at liberty to suppose that somewhere along the way the scientist has intuitively abstracted rules of the game for himself, but there is little reason to believe it. Though many scientists talk easily and well about the particular individual hypotheses that underlie a concrete piece of current research, they are little better than laymen at characterizing the established bases of their field, its legitimate problems and methods. If they have learned such abstractions at all, they show it mainly through their ability to do successful research. That ability can, however, be understood without recourse to hypothetical rules of the game.
> — Thomas S. Kuhn (1922 - 1996)
> (from 1996, p. 47)

After briefly reviewing the problem of epistemic access to the world, I give an argument for what I believe to be the appropriate way of doing metaphysics in the philosophy of science. I claim that a consistent metaphysics that is sensitive to the posits of science is required by science for making choices between different scientific theories. This requires a substantial, but guarded, realism with respect to the things posited by scientific theories: entities, laws of nature, causal forces, and things that govern the events of the world. This realism, I claim, is what allows scientific theories to explain reality. I consider Kuhn's historicism, a well known argument against realism, and claim that, in fact, revolutionary episodes in the progress of science that involve theory change require that scientists engage in metaphysical arguments to choose between different theories.

Given such a general guarded realism about scientific theories, I then consider whether one can be a realist about the laws of science. The Moorean argument that defends realism as the common-sense starting point for the ontology of everyday objects is not directly applicable to science, because we're no longer starting with common-sense beliefs. I consider and reject Bas van Fraassen's premises that science does not involve laws and that none of the existing metaphysical accounts of laws of nature is adequate. It would be high-handed of philosophers to tell scientists that they are not engaged in describing reality and that they don't use laws to formulate their theories. I then go on to defend Stathis Psillos's definition of scientific realism, which consists of a metaphysical, a semantic, and an epistemological thesis, as a better definition than van Fraassen's simple definition.

Next, I look at several physical theories that make use of what are claimed to be uncontroversial fundamental laws for those theories and consider these laws as candidates for what might be called (approximations to) laws of nature. From these I extract the essential metaphysical features that we might claim our metaphysics of the laws of nature must incorporate. I also argue that the metaphysics of the laws of nature shouldn't, on their own, disallow certain features in the world.

1. Metaphysical Arguments in Science

A. "Mind the Gap" between metaphysics and epistemology

The term 'realism' is applied in many areas of philosophy: one can be a realist about such things as ethical facts, universals, numbers, a mind-external world, *etc.*[1] Before considering realism as it pertains to the philosophy of science, it is useful to briefly consider what it means to be a realist about the world in general. Philosophical discourse on the topic is probably as old as philosophy itself and the topic constitutes a bridge between metaphysics and epistemology: what is the nature of the objective world and what is the relationship of that world to minds? The common-sense view is that the world, including minds and mind-external objects, exists and that we, the minds in the world, perceive and come to know about things in the world. But, as soon as one reflects on the problem of epistemic access to an objective mind-external world, it becomes difficult not to accept some sort of separation between things inside and outside the mind. The different forms of *idealism* resulting from the increasingly subjective reflections of John Locke, David Hume, and Bishop George Berkeley seem nonsensical to the non-philosophical mind, yet even the staunchest realist is apt to accept some sort of separation. Grasping the significance of this separation seems to be a necessary and sufficient condition for anti-realist tendencies to take hold in one's philosophical thought and, in an extreme form, the intoxicating effect can even lead one to doubt the existence of an outside realm, *e.g.*, Berkeley's idealism.

Now *subjectivism*, the belief that truth is relative to what individuals think,[2] is not necessarily scepticism, but it can easily manifest itself as an agnosticism about what exists in the mind-external world and what we, as humans, can know about it. Immanuel Kant's *transcendental idealism* limits human knowledge to the *phenomenal* world: a

1. Here I take 'thing' in its broadest sense to include abstract, concrete, particular, and universal entities. I take it that one cannot be an anti-realist about all things: even if René Descartes's *cogito* argument doesn't show that the *I* exists, it does show that something exists, *i.e.*, that something is real. (If a vacuous world is possible, I trust it isn't our world. In fairness, I've allowed my nihilist friend to give her opinion: " ".)

2. *E.g.*, 'dog food is delicious' is true for my dog but not true for me. Some have convinced themselves that all truth is relative truth.

representation of the world, not as it is in-itself, but as it appears to humans. So, for Kant, there *is* an external real world-in-itself (the *noumenal* world), but humans cannot have knowledge of it: there is no possibility of knowledge that transcends the phenomenal world. This is summarized nicely by Nicholas Rescher:

> To be sure, *we* could not possibly *know* such noumenal objects. Given that they are, *ex hypothesi* sense-inaccessible, they lie beyond the reach of that sensibility through which alone objects can be given to us. But we can certainly *think* them—that is, we can meaningfully assume or suppose (and indeed posit or postulate) such things. We thus have a cognitive (or, at any rate, *intellectual*) route to things in themselves independent of outright knowledge of them, fortunately so since knowledge of them is altogether unrealizable. This think vs. know distinction is thus crucial. Were Kant to hold that we can *know* things in themselves, or even that we can know of them (that they exist), then this would contradict his characteristic critical doctrine that any and all knowledge of objectively real things must, for us, be mediated by the sensibility. (1981, p. 291)

But *objective* knowledge in the realm of the phenomenal world is possible, because objects are given to the mind (*i.e.*, to the minds of all humans) according to certain categories of concept formation. Using the mind's capacity to reason with such concepts, there is the possibility of gaining *synthetic a priori* knowledge.

Note that transcendental idealism is an *a priori* claim about our relationship to the mind-external world; it does not follow from empirical evidence that this is the way things are. It is a metaphysical belief that transcendental idealism is true. For Kant, the justification for transcendental idealism is that it is inconceivable that the world (both phenomenal and noumenal, in total) is any other way.

Kant's minimal use of metaphysics motivates the way in which Bas van Fraassen builds his empiricist philosophy of science.[3] Empirical data (observations) come to us from the

3. See van Fraassen's recent Terry Lectures at Yale University, published as *The Empirical Stance* (2002), for a good overview of his philosophy.

outside via our limited senses and are therefore subject to epistemic scepticism. Any interpretation of reality based on that data is metaphysical interpretation done from the inside, so to speak, and therefore cannot be justified exclusively via empirical means. Metaphysical interpretation of phenomenal knowledge can be used to conjecture about what the outside world is like, *i.e.*, about ontology. But a question that is intensely debated in many areas of philosophy is, what is a rationally acceptable form of ontological interpretation? How much metaphysical extrapolatory speculation is appropriate? That science can give us any special access to the outside world is doubted by many, and for some, like van Fraassen, “Ontology cannot be read off scientific theories, although everyone has a go at reading ontology into them” (*ibid*., p. 8).

B. Metaphysical arguments in the philosophy of science

The epistemological pressures just discussed figure in the various philosophical positions one might take on scientific theories. The problem is exacerbated by the fact that modern science is far from common-sense belief. Scientific theories posit all sorts of strange entities with almost no internal constraints on which ones should be reified and which ones are just instrumental constructions meant only to aid in the prediction of new phenomena. They posit entities, laws of nature, causal forces, and things that govern the events of the world. Scientists also use their theories to give explanations for how things are in the world, and it would be difficult to see how such an explanation could be given without assuming some amount of realism about those theories. But such a realist does not usually accept all of a scientific theory’s ontological claims at face value; rather, she tries to make metaphysical sense of their purported content. From such a stance, what one wants from metaphysical theorizing is to adequately and consistently categorize, organise, and explain the content of a scientific theory—one wants a *guarded* realism about scientific theories.

This view is completely misguided for some who think that metaphysics has no place in scientific theorizing. Criticisms of modern metaphysics abound, and van Fraassen gives some incisive and illuminating criticisms that I take seriously. He claims that

> Metaphysical theories purport to interpret what we already understand to be the case. But to interpret is *to interpret into* something, something granted as already understood. Paradoxically, metaphysicians interpret what we initially understand into something hardly anyone understands, and then insist that we cannot do without that. To any incredulous listener they'll say: Construct a better alternative! But that just signals their invincible presumption that metaphysics is the sine qua non of understanding. (2002, p. 3)

By using the term 'incredulous listener', I take van Fraassen to be making an allusion to, for example, metaphysical theories that posit a multitude of real, non-interacting worlds.[4] But, in reply, the realist will claim that the scientific theories arising during the twentieth century are not things we already understand without metaphysical interpretation. The *scientific interpretations*[5] of quantum mechanics are legion, and some are as counter-intuitive as any metaphysical theory on offer: metaphysical analysis isn't the only endeavour that has postulated multiple parallel worlds! When science tells us that Schrödinger's equation characterizes the *objective chance* in a system, we are within our rights to ask what is meant by 'objective chance'. The uncertainty principle tells us that any actual *particle* lacks a determinate momentum unless it is taken to have a position described by an objective chance distribution covering all of space. Particle-wave duality models pass as scientific

4. *E.g.*, those postulated by David Lewis. See his *On the Plurality of Worlds* (1986a) and, for the possible source of the allusion, see the section entitled "The Incredulous Stare" (p. 133).

5. I call these scientific, even though they are metaphysical, because it is mostly scientists themselves who have created these interpretations (see Hugh Everett III, 1957).

explanations for some of the queer phenomenal observations made by scientists. Relativity physics, with its curvature of the spacetime continuum, is not inherently understandable. I claim that the metaphysician's attempts to interpret the posits of scientific theories by using a consistent set of ontological categories (that might divide up reality) is worthwhile and necessary for the scientific program to continue. It is useful, for the work of scientific theory building, to see how much of a scientific theory's posits can be fit into those categories and to discover any inconsistencies that might arise. It is especially revealing to see how far we have to stray from the ontological categories supplied by common sense to interpret some scientific theories.

If this view of how metaphysics relates to science is correct, then the metaphysician cannot work independently of science, *i.e.*, without being sensitive to the needs of science. It seems reasonable to demand that, not only should the system of metaphysical categories be consistent within itself, but also that metaphysics should not impose or deny possibilities for science: for example, it would be unacceptable for metaphysicians to tell scientists that nature must be discrete, that all interactions must be local, or that laws that describe the world as chancy are out. On the other hand, I take it as perfectly reasonable for the metaphysician to expose metaphysical vagueness, inconsistencies, or other deficiencies in scientific theories.

The obvious rebuttal to this argument is to claim that science is not in the business of describing reality as it *truly* is, that, if we look at the history of science, we will find a history of scientific model-building and that one should think of scientific theories as mere models that help us think about the regularities we observe. From the epicycles in the Ptolemaic system of astronomy to the curvature of spacetime in Einstein's model of gravity, what we have are scientific theories constructed as *instrumental* models. No doubt, there is some truth to this historical analysis of scientific theory-building, but I claim that at least part of the scientific aim is to describe reality as it truly is. Historicism in the philosophy of science was a natural reaction to the counter-intuitive scientific theories of the early twentieth century, and it is a natural source of antirealism, that is, it has been used as a justification for accepting *instrumentalism* as the philosophy of science.

C. Historicism in the philosophy of science

One of the most debated subjects in the philosophy of science during the past 50 years has been the nature and structure of scientific theories.[6] Science, as a whole, has many aims and it requires many methods to achieve those aims; it would of course be wrong for philosophers to force a single philosophical conception upon science. Taking that seriously, historicism says that philosophers should look to the history of science to discover the philosophy *of* science.

Historians such as Thomas Kuhn[7] behold a large sociological factor in the history and development of science, including its scientific theories. According to Kuhn, the scientific enterprise can be seen as proceeding in two major stages, what he calls *normal science* followed by periods of what he calls *extraordinary science*. During periods of normal science, groups of scientific professionals (in a scientific community) engage in research

6. This is not surprising given that making sense of scientific theorizing is a large part of modern epistemology. A comprehensive survey of the nature of scientific theories can be found in Frederick Suppe's *The Structure of Scientific Theories* (1977), which is partially a compilation of a symposium on the same topic held in 1969 with extensive commentary and a critical introduction by Suppe. Peter Godfrey-Smith (2003) gives a readable modern introduction to the philosophy of scientific theories.

7. Kuhn espoused the main points of his theory in *The Structure of Scientific Revolutions* in 1962 (see Kuhn, 1996) and refined and clarified them later (see for example Kuhn, 1969). There has been much debate and refinement of his views, but my point here does not require a detailed review of his theories.

programs guided by accepted *paradigms*. The scientific community is a group of specialised professionals who are

> bound together by common elements in their education and apprenticeship, [and who] see themselves and are seen by others as the men responsible for the pursuit of a set of shared goals, including the training of their successors. Such communities are characterized by the relative fullness of communication within the group and by the relative unanimity of the group's judgement in professional matters. To a remarkable extent the members of a given community will have absorbed the same literature and drawn similar lessons from it.[8]

Kuhn later replaces the term 'paradigm' with 'disciplinary matrix' to allay confusion resulting from the multivocal sense he had previously given to the term 'paradigm' in *The Structure of Scientific Revolutions*.

A disciplinary matrix is the group of objects the community is committed to, including *symbolic generalizations*, *models*, and *exemplars*. The first are the formal expressions of generalizations (such as rules and laws) that the group uses without question. For Kuhn they are uninterpreted strings of symbols, such as '$\boldsymbol{f} = m\boldsymbol{a}$', which satisfy the laws of logic and whatever mathematics they are defined within.[9] Models are what determine the ontology of the paradigm and give meaning to the symbolic expressions; using correspondence rules, they provide the semantics for the theory. Finally, exemplars are paradigm problems (or puzzles) with solutions obtained via the symbolic formalism and the use of the model with correspondence rules.[10] Kuhn characterizes the research program undertaken during normal research as puzzle-solving within the paradigm. He says,

8. Kuhn, 1969 (quoted from Suppe, 1977, p. 461).
9. Here I'm using the standard notation to differentiate between vector-valued quantities (bold font) and scalar-valued quantities (non-bold font).
10. Examples of exemplars are the problems and solutions given to students in science textbooks. This characterization of normal science is echoed in Gordon Belot's claim that understanding a theory involves 1) the formalism, 2) the interpretation, and 3) the application (1998, p. 533).

"Normal science does not aim at novelties of fact or theory and, when successful, finds none" (1997, p. 52).

According to Kuhn, and contrary to what the positivists believed, it is not during normal science that a search for confirmation and falsification of a scientific theory occurs. Failures in puzzle-solving tend to discredit the person who is trying to solve the puzzle from within the paradigm and are not typically accepted as indicators that something is wrong with the theory. But the accumulation of evidence that problems in fitting facts to accepted theory are not due to errors in applying the theory leads to a crisis in the scientific community and can be the first step in paradigm change. Sometimes, when no alternative paradigm is available, *ad hoc* adjustments are made to the accepted theory. If one or more alternative candidate paradigms are available, then theory change might occur based on comparing the paradigms with experimental observations of nature and with each other. Competing paradigms are tried and rejected. The change is characterised as a process of *scientific revolution*. But the nature of the process that results in paradigm change is left vague by Kuhn and is one area where he has been severely criticized.

I agree with Kuhn's historical analysis of the scientific enterprise, but I don't see what resources a scientific community can appeal to when a scientific crisis occurs at the level of fundamental scientific theorizing. It seems that the resulting scientific revolution can be guided only by metaphysical interpretation. I will argue that the endeavour of building *fundamental* scientific theories is a scientific-philosophical endeavour to give us *true* descriptions of reality and that theory change associated with such fundamental science requires metaphysical interpretation.

D. Metaphysics as necessary during theory change

Some philosophers might claim that Kuhn's studies on the nature of the scientific enterprise show that metaphysics has little to do with science. On the contrary, I claim that it is exactly when scientists consider theory change that metaphysical considerations come into focus for scientists. I agree with Kuhn that few scientists worry about the problem of realism and epistemic access when they're engaged in *normal science*, but the issue comes to the fore when scientists are looking for theory change. During *extraordinary science*, the scientist is more philosophically sensitive to what her new theory posits and what it throws out from the old theory: she is more metaphysically sensitive in her theory construction. A detailed historical study of the philosophical "discussions" scientists themselves have had during scientific revolutions has recently been published by Friedel Weinert (2005). It should seem clear to anyone acquainted with the history of scientific discoveries, and how those discoveries have changed our view about reality, that one of science's aims is giving us an account of what reality is like.

Thus, I don't believe that a scientific theory's posits, and whether or not they make metaphysical sense, are of concern only for the philosopher. It can sometimes be difficult, especially when developing fundamental scientific theories that might include unobservables, to determine exactly which parts of the theory carry ontological import. This difficulty is acutely evident when one considers that there are empirically equivalent but different formulations for many scientific theories.[11] But the metaphysical position that

11. Alan Musgrave (1992) makes this point with respect to Newtonian mechanics. It is also an issue in the different equivalent formulations of electromagnetic field theory. For a clear exposition of the latter, see Belot (1998), who considers the various metaphysical commitments associated with taking different field vectors as *real* when formulating classical electromagnetic theory.

is taken with respect to the concepts in, and ontological commitments of, a scientific theory, whether that position is taken explicitly or implicitly, is an essential part of the *scientific* world view associated with that theory. As Alan Musgrave says,

> For realists physics is continuous with metaphysics. There is no "illegitimate intrusion" of metaphysics into physics. Realists look to physics (and to science in general) to inform their metaphysics, to tell them what reality is really like. Contrariwise, physics has to look to metaphysics to help decide (fallibly, of course) between experimentally undecidable alternatives. (1992, p. 696)

The question of just how much metaphysics one should allow into physics is a question of considerable debate. I think that it is an illegitimate intrusion of metaphysics into physics when metaphysics posits categories of things that have no use in physics, but it is not an intrusion for metaphysics to categorize and explicate the constituent components of a scientific theory. But how does one know when a constituent component of a scientific theory is an essential scientific posit and when it is a just a manner of speaking? This is clearly a problem when it comes to the purported *laws of science*.

E. Laws of science as legitimate parts of scientific theories

I believe that it is legitimate to ask for a metaphysical characterization of laws of science, because many scientists themselves believe that they are discovering an objective part of reality when they discover laws. But how does one go about accepting laws of science as legitimate entities that science posits and not merely as ill-defined concepts that should be eliminated from the ontology? Many scientists and philosophers alike, including van Fraassen, will agree that the starting point for the foundations of our epistemology must be empiricism. That seems like a truism, but that starting point might allow the positing of only the observable objects of everyday sense-experience and not much more.

Given our desire to advance from such an epistemically deprived position, there are at least two methodological approaches one can take: starting from a position of hypersceptical doubt, one can demand justification for every belief; or one can accept the common-sense beliefs of everyday existence and demand justification for any change in those beliefs. The first might be called Descartes's method and the latter Moore's method. Van Fraassen quotes Bertrand Russell's acknowledgement of the latter approach:

> Of course it is not by argument that we originally come by our belief in an independent external world. We find this belief ready in ourselves as soon as we begin to reflect... . Since this belief does not lead to any difficulties, ... there seems no good reason for rejecting it ... we cannot have *reason* to reject a belief except on the ground of some other belief. (1989, p. 171)[12]

But he claims that Russell adheres to what van Fraassen calls "the Prussian concept of rationality: what is rational to believe is exactly what one is rationally compelled to believe." By contrast, van Fraassen opts for the dual: "what it is rational to believe includes anything that one is not rationally compelled to disbelieve."[13] Here, van Fraassen seems to be portraying his conception of an appropriate epistemology as one of backing-off from a realistic first position (at least when it comes to everyday common-sense objects). This seems to be an appropriate way to go about one's epistemology; one can argue that the common-sense viewpoint should always be the starting position in any philosophical discourse.[14] According to Moore's method, the amount one backs away from the common-sense viewpoint should be dictated by the demands of rational argument: we do not require

12. The original quotation is from Bertrand Russell's *The Problems of Philosophy* (1912, p.25).
13. Both quotations are from *Laws and Symmetry* (1989, p. 171).
14. At least it has seemed the appropriate way ever since G. E. Moore's publication of "A Defense of Common Sense" in 1925 (see Pojman, 1999, p. 52-59). I'll ignore the problems of making sense of 'common-sense': *cf.*, the fact that Berkeley believed the common-sense position to be idealism.

reasons for what we inherently believe unless there are strong arguments against those beliefs.[15]

But we misunderstand van Fraassen if we take him to literally mean that what it is rational to believe includes *anything* that we are not rationally compelled to disbelieve, for he does not follow this approach when it comes to the laws of nature. A few pages later, by way of clarifying his epistemological stance as applied to laws of nature, he writes,

> To take one good example: belief in laws of nature does not 'go'. It is true that if some philosophers believe in the reality of laws, they are not *ipso facto* irrational. But it does not follow that they are in a position to persuade us or even give us a good reason to follow suit. They may be, for us, in exactly the same position as the sceptics: their conclusions, if we accept them, would drastically realign our opinions—but those conclusions are based on premises which they can neither substantiate nor make appealing to us. ...
>
> Summing up this way, I am not simply placing the onus on the opposition. For if it could have been shown that the reality of laws was indeed inherent in traditional, prevalent opinion—left intact under scrutiny—that would have sufficed. (1989, p. 180)

Van Fraassen's claim here is that laws are not part of the philosophy *of* science itself; that is, they are not part of the ontology that scientists posit. He spends the second part of *Laws and Symmetry* arguing that scientists come up with their so-called laws using principles of symmetry, for example, invariance under transformation. Van Fraassen looks at the output of science and does not find scientific laws conforming to the philosopher's laws. In the first part of *Laws and Symmetry*, he examines the existing metaphysical theories of laws that philosophers have created and finds them all unsuitable in meeting the criteria for adequacy that he himself has laid out. Therefore, based on two premises, (1) that the tradition of science does not seem to employ laws in formulating scientific theories, and

15. The extreme sceptic denounces common-sense belief in general without providing an alternative (in general): his project is purely destructive. Moore was battling the arguments of the extreme sceptic.

(2) that none of the existing metaphysical theories conforms to his criteria for laws of nature, he concludes that there are no laws.

Of course, this is not a valid argument, because van Fraassen is appealing only to historical data for his premises: even if scientists haven't used laws so far, they might; and, even if the existing metaphysical theories might be deficient, future theories might not be deficient. But I would find the argument convincing if it were true that the concept of laws is not part of the tradition of science. Contemporary philosophers sympathetic to van Fraassen's anti-metaphysical views also disagree with his claim that laws of science are not part of the scientific enterprise. In a recent paper, John Earman writes,

> The topic of symmetries and invariances in physics provides no comfort for those who hanker after *laws of nature* in the sense of critters that embody the goodies on the wish list drawn up by philosophers: laws of nature are supposed to be objective (independent of our interests and beliefs); they are supposed to express a strong form of non-logical necessity ("nomological necessity"); they are supposed to cut nature at the joints by expressing truths about natural kinds; they are supposed to have the power to explain the why of things; etc. Here I declare myself in sympathy with the milder form of the "no-laws" view: if there are such critters, I see no good reason to think that science can be counted on to corral them, or that we can tell the difference between cases where science has succeeded in corralling them and cases where it has failed to do so.
>
> But by the same token the topic of symmetries and invariances does not support the strong version of the no-laws view, which intimates that both the history of science and its current practice can be understood without giving pride of place to the search for *laws of science*. In the case of physics—which will be my focus—what physicists mean by the *laws of physics* is, roughly, a set of true principles that form a strong but simple and unified system that can be used to predict and explain. (2004, 1228-1229)

I agree with Earman: I don't think that van Fraassen has convincingly justified his claim that science does not recognize laws as part of its domain. But, as Earman correctly points out, that does not mean that the scientists' laws are the same as any purported laws of nature created by the metaphysician.

I think that the metaphysician can take scientific laws to be a constituent part of scientific theories—they are part of the tradition of science—and claim that philosophy has a role in making sense of them. But being a constituent part of a scientific theory says nothing about whether that part corresponds to anything in reality. The scientist might be using laws of nature as a way of speaking about reality and our observations of it, but they might not correspond to anything real. Also, we need a clear characterization of what scientific laws are and what they claim about reality.

2. The Philosophical Characterization of Scientific Realism

I've argued that, through scientific theories, science claims to say something about reality. Now, realism in science is probably a matter of degree, but it is worthwhile looking at some of the characterizations of scientific realism. In his now classic work, *The Scientific Image*, van Fraassen (1980) gives a precise statement of what he takes to be scientific realism and contrasts this with the anti-realist (instrumentalist) position that he names 'constructive empiricism'. According to van Fraassen, scientific realism is defined by the claim that

> Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true. (1980, p. 8)

In contrast, constructive empiricism he defines as the claim that

> Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate. (1980, p. 12)

For van Fraassen, a theory is empirically adequate if what it says about the observable part of the world comes out true. The well-used metaphor that the theory "saves the phenomenon" is revealing, though vague, about what van Fraassen has in mind. I will not consider his anti-realist position, but I do want to make some observations about van Fraassen's definition, because I think that a more precise and illuminating characterization can be given. (His goal was to argue against scientific realism and so he gave a very weak characterization that he thought most realists could agree with.)

The first observation is that van Fraassen states his definition in terms of an *aim of science*. This forestalls objections based on scepticism that scientific theorising will ultimately (actually) achieve a literally true story of what the world is like. It makes his definition compatible with the view that scientific theories will never get to the true story. By focusing on the aim of science, van Fraassen renders irrelevant all epistemological concerns regarding the possibility of arriving at the literal truth, and this makes his definition of scientific realism quite weak. I don't object to his casting of scientific realism in terms of an aim of science.

The second part of his definition deals with what it means to *accept* a scientific theory, but it is unclear whether van Fraassen is referring to the acceptance of any current scientific theory or the acceptance of the ultimate theory at which science aims. If someone believes that science will never get the story exactly right, then she might still want to call herself a realist but also claim that, for example, what she is referring to as electrons today might, in future better theories, be reduced to other more fundamental entities or even be replaced by other entities having a completely different nature. Therefore, she might never want to accept any *current* theory as literally true. More likely, she will want to claim that a current theory is adequate to model the currently observed phenomena, and though she tentatively accepts the posits of the theory as the best available so far that approximate the truth, she doesn't exclude the possibility that the theory might not survive scrutiny in the future when new observations are made. Van Fraassen's definitions would make such a person out to be a constructive empiricist and not a realist, because she doesn't assent to the literal truth of the theory. But, on the other hand, I claim that such a person is assenting to more than the empirical adequacy of the theory; she is assenting to the *approximate truth* of the theory.

This is exactly the reason why a realist might not want to commit to a *literally* true story of what the world is like; it is difficult to make sense of exactly what a literally true story is. This is problematic for stories, not only about unobservables, but about observables as well. What is the literally true story of a baseball? Whatever representation one comes up with for a baseball, it seems that a different, more "accurate" representation might become available in the future. The person born blind will have a different literally true story to tell about the baseball than the person who can see the baseball. Science does not tell us only that electrons exist—that would be to tell us almost nothing[16]— it also tells us what the properties (categorical and dispositional) of any electron are. So a realist might want to believe that the posits of the currently best scientific theory are approximate representations and not literally true. After all, what does it literally mean to claim that, in the Rutherford model of the atom, electrons are like planets revolving around the nucleus of the atom? This is like the blind person trying to describe the baseball.

Van Fraassen's characterization of scientific realism, as distinct from constructive empiricism, seems too much of an epistemological characterization. Any concern we have about our semantic machinery to accurately characterize the scientific posits we make seems to make us look like a constructive empiricist. We require more room to make the

16. Just like I'm telling you almost nothing when I tell you that *gtgdtebdtes* exist.

subtle distinctions we're apt to make in our metaphysics. I think that a better characterization of realism is that described by Stathis Psillos. He defines three theses as constitutive of scientific realism (2003a, p. 3):

> *The Metaphysical Thesis*: The world has a definite and mind-independent structure.
>
> *The Semantic Thesis*: Scientific theories are truth-conditioned descriptions of their intended domain. Hence, they are capable of being true or false. The theoretical terms featuring in theories have putative factual reference. So if scientific theories are true, the unobservable entities they posit populate the world.
>
> *The Epistemic Thesis*: Mature and predictively successful scientific theories are well confirmed and approximately true. So entities posited by them, or, at any rate entities very similar to those posited, inhabit the world.

I think that one can use these theses to articulate different gradations of scientific realism. To accept the metaphysical thesis is to accept the existence of an objective reality, but that says nothing about the aim of science or our epistemic access to that objective reality. The metaphysical thesis also says nothing of our ability to formulate scientific theories that accurately refer to that objective reality; that is left to the semantic thesis. But note that, in the semantic thesis, the assertion that unobservable posits of a theory actually populate the world is stated as a hypothetical: if scientific theories turn out to be true, then those posits populate the world. Whether we can know that any scientific theory ever turns out to be true is left out of the semantic thesis and is made part of the epistemic thesis. It says that successful scientific theories are at least approximately true. It allows that the posited entities might not be exactly as they are in the literal story our theories tell about them, but it does make a positive claim that those entities will be very similar to what our theories say. The epistemic thesis is in tension with the semantic thesis: if approximate truth is all we ever get out of our theories, then the antecedent in the hypothetical part of the semantic thesis never kicks in, and we might never claim that the entities posited by those theories actually populate our world. This allows us to claim literal precision for our scientific theories: if a scientific theory refers to unobservable entities having such-and-such properties, then we take such posits at face value, and we follow that up with the claim that, *if* the theory is *true*, then those entities exist exactly as featured in the theory. But belief that that theory is true kicks in only as part of the epistemic thesis, which includes provisions for our theories remaining only approximately true; the posited entities might forever remain only pale approximations to those of the theory.

I think that this is a major improvement over van Fraassen's characterization of scientific realism. Separating scientific realism into a metaphysical, semantic, and epistemic theses, allows us to be more specific in our ontological claims about scientific theories.

3. Scientific Theories: Entities, Laws, Governance, and Explanation

I will now review some typical scientific theories to show that they posit entities, properties of those entities, and relations defined over those entities and properties. These properties and relations are usually mapped to quantities (numbers), and many scientific theories include laws, formulated in terms of the properties, relations, and quantities that govern (or at least can be used to predict) the changes in those properties and relations under different circumstances. A major aim of science is to explain the changes and the regularities that we observe, and therefore the concept of governance or necessity makes it into its theories.

Physics is the science that claims most of the fundamental laws, but even the "softer" sciences, such as biology, sometimes formulate laws that govern the phenomena within their domain. For example, the biological sciences might posit certain taxa and laws that govern the ontogeny of members of that taxa within specified environments.[17] It has sometimes been claimed that the extent to which a scientific discipline formulates mathematical laws is the extent to which it is regarded as a mature (or "truer") science. But there is probably no precise notion of what a scientist might call a law in her particular discipline. Almost any *axiomatic* lawlike statement that licenses the making of inferences might be referred to as a law in science.[18] I will take three examples from physics, and use them to help me make the point that scientific theories posit entities, various kinds of laws, and governing features of our world and use these to explain reality.

A. Newton's gravitational dynamics

One way of formulating Newton's law of gravitation is to posit point-sized particles[19] (entities) each having a gravitational mass (property) that is assigned a numerical value (quantity). Suppose we have two particles p_1 and p_2 with their gravitational masses represented by the two numerical variables m_1 and m_2 and the vector distance from particle p_1 to particle p_2, a quantitative relation, represented by the vector variable $\boldsymbol{x} = x\hat{\boldsymbol{x}}$, where x is the magnitude of the distance and $\hat{\boldsymbol{x}}$ is the unit vector that gives the direction from particle p_1 to particle p_2. The force on particle p_1 due to particle p_2 is then represented by the vector quantity $\boldsymbol{f}$ and, according to Newton's law of gravitation, it is

17. Although the existence of fundamental laws (proper) in current biology is debatable, Davis Resnick (1997) makes the case that there is nothing in principle that stops the biological sciences from developing their own fundamental laws.

18. Marc Lange (2000, pp. 30-33) has compiled a list of more than 30 "laws of nature" from as many as 16 different domains.

19. For extended *rigid* bodies, the centre-of-mass can be used as the location of an equivalent point-sized particle having the same mass as the extended body. The rest of the theory remains the same for translational motion resulting from the gravitational force. Of course, replacing the extended bodies with point-sized particles will not allow one to calculate the *moments* resulting from the gravitational interaction.

related to the gravitational masses and the distance as

$$\boldsymbol{f} = G\frac{m_1 m_2}{x^2}\hat{\boldsymbol{x}} \tag{NG}$$

where G, the gravitational constant, accounts for the particular units of measurement used to quantify the gravitational mass and distance quantities. In words, one says that the force on a point-sized particle due to another point-sized particle is proportional to the product of the two gravitational masses of the point-sized particles, inversely proportional to the square of the distance between the two point-sized particles, and is an attractive force directed along the line connecting the two masses.

To Newton's law of gravitation, we can add his three laws of dynamics: (1) a body in constant (non-accelerating) motion that is not acted upon by a force remains in the same constant motion, (2) the force applied to a body will equal the resulting time-rate of change of the body's momentum, $\boldsymbol{f} = \dot{\boldsymbol{p}}$, where the body's momentum $\boldsymbol{p}$ is defined as the body's inertial mass m_i multiplied by its velocity $\boldsymbol{v}$, and (3) for every (forceful) action there is an equal and opposite (forceful) reaction.[20] Finally, adding the contingent truth that for any body the value of its gravitational mass is the same as the value of its inertial mass, we get the complete theory governing the dynamics of rigid bodies under the influence of gravity (gravity being the only force operating on the bodies).[21]

The first feature of philosophical interest here is that we have a case of classical action-at-a-distance. If the distance between particles changes, then immediately the force changes, no matter what the distance between the particles is. Second, there is no way in principle to observe this force other than to observe the effect it has on the particles'

20. A dot is used over the a variable to denote differentiation with respect to time.
21. A standard textbook presentation of Newtonian mechanics is Goldstein1980.

motions. Third, the force is what *causes* the particles to attract each other, and the law of gravitation *governs* the magnitude and direction of the resulting force on any particle. If you were to ask for an explanation of why the particles attract, the scientist might say that it is because there exists a force of attraction governed by Newton's law of gravitation acting on the particles. The explanation *ends* at the positing of the unobservable force whose nature is governed by a law of science.

Newton's theory of gravitational dynamics has remained an approximate theory even though it has been superseded by Einstein's theory of general relativity (GTR). It is approximate in two senses: (1) its predictions for phenomena like the motion of the planets, or the ballistic behaviour of cannon balls, are as accurate as the measurements typically used to verify the phenomena, and (2) the mathematical law itself can be derived by making some mathematical approximations corresponding to physical assumptions in GTR. Even so, GTR is a quite different theory in spirit. As opposed to positing a mysterious force between particles in space, GTR posits the curvature of the spacetime continuum according to Einstein's field equation.[22] In tensor notation, this equation is simply written as

$$R_{\mu\nu} - \frac{1}{2}g_{\mu\nu}R = \kappa T_{\mu\nu}, \tag{GTR}$$

where $T_{\mu\nu}$ is the stress-energy tensor, $g_{\mu\nu}$ the Lorentz metric of space, R the scalar curvature, $R_{\mu\nu}$ the Ricci tensor obtained from the Riemann curvature tensor by contraction, and κ is a constant to take care of measurement units. Einstein (GTR) is solved to give a curvature of spacetime that is consistent with a stress-energy tensor; motion of particles through the spacetime manifold has to be motion that is consistent with (GTR).

22. For a readable account of GTR see Albert Einstein (1953).

Now Einstein used classical electromagnetic theory as a model to develop GTR as a field theory: Coulomb's inverse square law of electrostatics is identical in form to Newton's gravitational law, and Coulomb's law—together with Ampére's magnetostatic law and Faraday's law of induction—was the basis upon which Maxwell created his electromagnetic field theory. I will use electromagnetic theory as a typical classical field theory.

B. Electromagnetic field theory

Electromagnetic (EM) field theory in free-space can be formulated in terms of three vector field quantities: $\boldsymbol{E}(\boldsymbol{x}, t)$, $\boldsymbol{B}(\boldsymbol{x}, t)$, $\boldsymbol{J}(\boldsymbol{x}, t)$, and one scalar field quantity, $\rho(\boldsymbol{x}, t)$, defined over time t and space $\boldsymbol{x}$ coördinates. These fields are known as the electric field intensity, the magnetic flux density, the electric current density, and the electric charge density, respectively. They are related by Maxwell's equations:

$$\begin{cases} \mathrm{curl}\boldsymbol{E} = -\dot{\boldsymbol{B}} \\ \mathrm{curl}\boldsymbol{B} = \dfrac{1}{c^2}\dot{\boldsymbol{E}} + \mu_0 \boldsymbol{J} \end{cases} \tag{ME}$$

where 'curl' denotes the usual curl operator of vector calculus and where μ_0, the permeability of free-space, and c, the speed of light in free-space, are constants. The current and charge densities also have to satisfy the law of conservation of charge:

$$\mathrm{div}\boldsymbol{J} + \dot{\rho} = 0. \tag{CC}$$

The force on any object having charge q is given by the Lorentz force law:

$$\boldsymbol{f} = q(\boldsymbol{E} + \boldsymbol{v} \times \boldsymbol{B}), \tag{LF}$$

where $\boldsymbol{v}$ denotes the charge's velocity. More complicated formulations are available, but this will suffice as an example of a typical field theory.[23]

23. A standard textbook presentation of classical electromagnetism is Jackson 1999.

A fundamental feature to note here is that the force on a particle is caused by the field vectors located at the same spatiotemporal location of the particle. There is no action-at-a-distance as part of classical EM theory. It is true that the cause of the EM field is the spatiotemporal distribution of electrical charges and electrical currents, but these are produced locally and the EM field propagates as a wave, at the speed of light, throughout the spacetime continuum. All interactions are spatiotemporally local interactions.

There is also an interesting philosophical feature that comes out of EM theory that is not usually reported. In Coulomb's electrostatics and Ampére's magnetostatics, from which EM field theory emerged, we also have action-at-a-distance, and one might have objected that we had no epistemic access to that mysterious force that acts between the charges. In EM theory, one might also want to complain that we have no epistemic access to the EM field between the charges that produce the field and those that receive the interacting field a short time later. But *look* again: EM field theory also postulates that light is an EM field, and we sometimes do have epistemic access to light travelling from its source to the place where it interacts with other entities, *e.g.*, using our eyes! That our eyes are only sensitive to a small part of the EM spectrum seems only a small objection to a theory that can encompass so many of technological advancements of the past 100 years or so.

C. Quantum mechanics

Quantum mechanics (QM) claims to be a truer representation of how particles in the realm of the microscopic behave. There are four features of QM that will be important for our characterization of scientific theories. The first is exemplified by the de Broglie relation, which associates with any particle having a momentum $\boldsymbol{p}$ a wavevector $\boldsymbol{k}$ satisfying the relation $\boldsymbol{p} = \hbar\boldsymbol{k}$ with the "wavelength" λ of the particle obtained from the *magnitude* of the wavevector, $k = |\boldsymbol{k}|$, via $\lambda = 2\pi/k$. (Here, $\hbar$ is just Planck's constant.) So we see that, in QM, particles have a *wave-nature* associated with them.

The second feature is that QM incorporates the so-called *Heisenberg uncertainty relations* that must be satisfied between properties associated with *non-commuting* operators. For example, the uncertainty Δx of the position of a particle along the x-direction and the uncertainty Δp_x of momentum along the x-direction must satisfy the uncertainty relation

$$\Delta x \Delta p_x \geq \hbar/2\,. \tag{UR}$$

The *uncertainty* is defined as the positive square root of the variance. The uncertainties are not due to experimental error or lack of experimental precision. The uncertainties are an objective part of reality.

The third feature is that the position of particles is now governed by a so-called wave function $\psi(\boldsymbol{x}, t)$ whose magnitude squared, $|\psi(\boldsymbol{x}, t)|^2$, gives the probability distribution that the particle is located at coördinate position $\boldsymbol{x}$ at time t. The wave function itself is obtained deterministically by solving Schrödinger's equation for the particle. The time-dependent Schrödinger's equation is given as

$$i\hbar\dot{\psi}(\boldsymbol{x}, t) = -\frac{\hbar^2}{2m}\nabla^2\psi(\boldsymbol{x}, t) + V(\boldsymbol{x}, t)\psi(\boldsymbol{x}, t)\,, \tag{SE}$$

where m is the mass of the particle, $V(\boldsymbol{x}, t)$ is the potential energy field the particle finds itself in, ∇^2 denotes the Laplacian operator of vector calculus, and $i = \sqrt{-1}$ is the imaginary number.[24] So, in QM, all we can ever know are the objectively defined chances of a property, such as a particle's position, obtaining.

The fourth feature of QM is that the wave functions of widely separated particles can, and do, become entangled so as to create a single wave function for the ensemble of particles. Then, any experimental measurement that gives up the properties of one of the entangled particles collapses the total wave function and thereby determines something about the other particles. The effect is instantaneous but can be across a large portion of space. We have something that might be called stochastic action-at-a-distance.

D. Philosophical features of laws of science

Having surveyed some of the important physical scientific theories, we can abstract some philosophical features common to scientific laws. The goal is to keep these features in mind when we attempt to formulate a metaphysical theory of laws of nature. The scientific theories I've reviewed might be good candidates for (or approximations to) laws of nature, or they might not be. Given the Kuhnian historical analysis of how science progresses, I doubt that we have any firm ground upon which to make such a prediction. It might be that no metaphysically consistent formulation of the laws of nature can be advanced, but I doubt that to be the case, because I believe that metaphysics was, and is, a large part of the formulation of the scientific theories we just overviewed.

All the theories I've reviewed included lawlike propositions relating quantified properties of, and relations between, (usually idealized) entities. The entities that occur in the scientific lawlike propositions are not just material things, like point-sized particles.

24. A standard textbook presentation of Quantum Mechanics is Merzbacher, 1998.

Sometimes the entities are material densities, sometimes they are vector fields defined over the whole spacetime continuum, sometimes they're mysterious causal forces like the force of gravity. The wave function in QM is probably the most mysterious thing that finds its way into scientific lawlike propositions; what kind of reality can a complex valued wave function have, whose magnitude squared gives the probability distribution for a particle's position in spacetime? In none of the theories I've reviewed does the law seem to cause anything to happen. It is closer to the truth to say that the law constrains things to happen only in certain ways.

The following is a list of proposed necessary conditions, given by Alexander Bird (in a draft of his forthcoming book), for a statement (*i.e.*, a sentence having truth-value) in a scientific theory being lawlike.

> Relative to a particular field and the state of knowledge concerning it, *S* is held to state a law only if:
> 1. it states a reasonably general relationship between quantities and properties;
> 2. it seems to state a relationship that is close to fundamental;
> 3. it is a new discovery, not easily deducible from known laws;
> 4. it has a wide application in predicting and explaining phenomena;
> 5. the relationship seems necessary rather than accidental. (pp. 138-139)

The first thing to note is that Bird makes all his criteria relative to a particular field and state of knowledge concerning that field. Thus, a law-statement for biology might not be a law-statement for physics, because physics might not even refer to the same quantities and properties that biology does. Laws are also dependent on the state of knowledge: a law-statement for Newtonian mechanics will not be a law-statement for QM. Given this qualification, the other listed features are probably quite acceptable to most scientists and philosophers. Feature (2) relates to (3) in that, typically, a new discovery will be treated as fundamental, and later it might be found to be derivable from other more fundamental laws. But that laws are new discoveries doesn't belong in the metaphysical conception of laws of nature, because (3) is a belief about the progress of science in discovering the laws. I take it that Bird is defining *fundamental* laws of science.

4. Desiderata for the Metaphysical Conception of Laws of Nature

Accepting Bird's characterization of fundamental laws of science, we might now try to extrapolate to the metaphysical features of a law of nature. It seems that the minimal metaphysical features we might want can be summarized by saying that L is lawlike if it is a counterfactual-supporting axiomatic generalization that is explanatory of phenomena. These axiomatic generalizations are *based* on (1) the properties of, and relations between, things themselves, and (2) relations between those properties and relations on the one hand, as well as relations between their mappings to quantities, on the other.[25] The generalizations are axiomatic in that they are the fundamental truths used to deduce other truths in the domain of the theory. In addition, the generalizations specify necessary, or probable, relationships, and to say that they are counterfactual-supporting is, at least, to say that they can be used for predicting future events from previous or occurrent events.

There is a lot the above list of metaphysical features doesn't say: probably the most controversial missing piece of metaphysics is the ontology of the laws of nature. What makes the lawlike propositions true? What kind of thing is it that can constrain the events in the world happening the way the lawlike statements say? How is it possible that laws of

25. Even a simple lawlike proposition like, the proposition that nothing travels faster than the speed of light is about a relation between two mappings to quantities. Suppose that the speed of anything x with respect to any reference frame r is represented in our language by the 2-place function $S(x, r)$ that maps things and reference frames to the non-negative real numbers (quantities) using whatever units of measurement are being used. Let l be the atomic constant in our language referring to light. In that case, our proposition can be represented in our language as $\forall x \forall r\, (S(x, r) \leq S(l, r))$.

nature are explanatory? The ontology of laws of nature is the subject of the rest of this thesis, but before ending this chapter it is important that we consider what laws of science don't disallow. It is important, because it wouldn't be reasonable to propose an ontology for laws of nature that might disallow something that laws of science don't themselves disallow.

A. Causal connection, relevance, and spatiotemporal locality

The first issue involves the positing of a *causal connection* between things. It seems that science posits causal connections, such as mysterious forces and fields, that can be used as the explanatory base for the events of the world. Second, there is a clear *relevance* between what is posited and the explication of the causal connection. For example, it is clear how the elements of a causal interaction are involved in that causal interaction: if there is a causal interaction, based on Newton's gravitational law, between two bodies, then it is the masses of only those two bodies that figure in the law. This will be an issue when I discuss the class nominalist's construal of properties.

The third issue involves local versus non-local laws. Non-local laws might allow things like "spooky" action-at-a-distance. There are reasons one might not want to accept such a law as an accurate portrayal of nature, but it seems that our metaphysical conception of laws should *not* exclude such non-local laws outright. A similar remark is applicable to the various conservation laws, *e.g.*, the law of conservation of energy, which I haven't discussed in detail. These are globally defined law-propositions that constrain things over large regions of spacetime to happen in the way the laws specify.

B. Determinism vs. non-determinism and objective chance

The issue of determinism versus non-determinism is another major feature of the world that should not be decided by our metaphysical conception of laws of nature. The ontology of laws of nature should not prevent them from specifying non-deterministic events as part of our world. It might be that, in fact, such laws are not part of our world, but it is not the job of the ontological formulation to exclude such laws from our world. The fact that quantum mechanics and quantum field theory currently characterize fundamental events as chancy events is enough motivation to ensure that the ontological conception of the laws of nature allows such laws.

III — Realism About the Nomic, Explanation, and Governance

The only laws of matter are those that our minds must fabricate
and the only laws of mind are fabricated for it by matter.

But in the heavens we discover by their light, and by their light alone, stars so distant from each other that no material thing can ever have passed from one to another, and yet this light, which is to us the sole evidence of the existence of these distant worlds, tells us also that each of them is built up of molecules of the same kind as those which we find on earth. A molecule of hydrogen, for example, whether in Sirius or in Arcturus, executes its vibrations in precisely the same time.

— James Clerk Maxwell (1831-1879)
(second quote from *Molecules*, 1873)

Given that a guarded realism is the appropriate metaphysical stance to take towards fundamental scientific theories, I consider what such a realism delivers to the ontology of the nomic, explanation, and governance. I start by giving a rough initial categorization of scientific posits into static and dynamic general facts and argue that the laws of fundamental science fall into the category of dynamic general facts. I then review primitivism, reductionism, and eliminativism as ways of treating any purported thing or type of thing and apply these categories to the laws of nature before surveying the other nomic concepts.

Next, I move on to Humean scepticism regarding the nomic and consider Hume's argument, as given by A. J. Ayer, that there *cannot* be any natural necessity in the world because the only necessity is logical necessity. I agree with Hume that causal or law-like inference from one event to another cannot proceed by the laws of logic alone but argue that no nomic realist would consider that to be an objection to the use of abductive inference to real laws that explain the regularities they are inferred from. I review the naïve regularity accounts of lawhood and causation and argue that (1) no regularity account of lawhood can give a metaphysical explanation for the observed regularities that scientists use to infer the laws and (2) no explanation can be given for future instances of causal regularities. I briefly argue that an inference to the best *causal* explanation type of rationality requires the acceptance of some realism about the nomic and claim that it is a common and acceptable source of scientific theorizing about the world.

I conclude the chapter by considering realism about governance and argue for the existence of things that govern as *reasons* for the regularities in the world. I accept one of Galen Strawson's arguments for *Producing Causation Realism* that some reason is required for the regularities of the world; otherwise the existence of these regularities would be an astronomical run of good luck, and belief in the continued existence of these regularities would be unfounded. I think that Helen Beebee's argument against this claim, that appeals to our intuition of how flukey our own existence is, misses Strawson's point that, if there is no glue from one event to another, then absolutely anything is equally likely to happen during the next time interval. I defend Strawson's argument that reasons for regularities are needed against Beebee's claim that a regularity realist can stop at the regularities as brute facts about the world and exclude reasons for those regularities from his ontology. I disagree with Beebee that brute fact regularities are an acceptable stopping point upon which to base a metaphysical explanation.

1. Ontology Related to Scientific Theories

A. Philosophical-scientific categories of things

To help motivate and identify the metaphysical problem that I'm interested in, I will now attempt to abstract out a philosophical-scientific categorization of things from the description of the fundamental scientific theories I gave in the previous chapter. I've argued there for a guarded realism with respect to the posits of fundamental scientific theories, which posits I've described as a wide variety of entities, laws of nature, and causal forces. These posits are meant to tell us something about reality: what the things are that exist in the world and what holds these things together in the way that we observe them to be held together. An immediate philosophical problem is that our intuitive notions of entities are much more refined than our intuition of laws of nature and causal forces. So it is worthwhile to look closer at the observational process that has led scientists to create fundamental theories in the way that they have.

Scientific observations of the world can be divided up into static observations, which are those we make of the contents of the world at a particular time, and dynamic observations, which are those we make of the changes occurring through time.[1] The tokens of static observations are then grouped into types of static observations, and the tokens of dynamic observations are similarly grouped into types of dynamic observations involving

1. Although we sometimes say that a change occurs *at* time *t*, what we really mean is that a certain situation obtains before time *t* and after that time the situation is different. So it's more precise to say that a change occurs through or across a particular time.

types of static observations. Typically, though, the typing of static and dynamic observations must be abstracted out of complicated observational situations; things we deem irrelevant to the observed patterns are not taken into account in the typing of the observations.[2] But realism about the world, and scientific realism in particular, does not stop at the typing of observations.

From static and dynamic observation types, scientists posit *static* and *dynamic general facts* that explain the observations and allow one to make predictions about future observations. Static general facts are the positing of properties and the existence of types of things having those properties, *e.g.*, things having such-and-such properties *at* particular times. They also relate the posited properties to each other. Dynamic general facts explain observations of static facts occurring through time, whether change is involved or not. An example of dynamic general fact is that the distance any spring stretches from its unstretched position is proportional to the number of equisized marbles that are hung from the spring. I don't mean the term to be too closely linked to our idea of change; for *lack of change* through time might also be something we want to lump in with the dynamic general facts (*e.g.*, Newton's first law of dynamics). That scientists assume the truth of propositions that seem to correspond to dynamic general facts is not controversial; that the world actually contains dynamic general facts as part of its ontology is controversial and is the main subject of my thesis.

I claim that the law-like propositions that are part of scientific theories correspond to these static and dynamic general facts. Notice that most of the scientific laws that I

2. One might argue that the same situation never recurs in the world, no matter how locally it is defined, if one also believes that everything in the world should be taken as relevant to a situation.

described in the previous chapter are expressed as evolution-type equations: differential equations where the time coördinate is at least one of the independent variables. Such laws tell us how things evolve through time—they are dynamic laws and we take them to be a dynamic fact about the world. Newton's gravitational law, on the other hand, tells us what the forces produced and experienced by interacting massive objects are at a particular time. It is a law, taken as a static general fact, that describes the nature of the relationship between causal gravitational forces and massive objects: massive objects produce causal gravitational forces that are experienced by other massive objects. In this case, we are led to think of the causal gravitational forces as real entities and the massive objects that produce them as their *sources*; that is, without objects having mass, the forces do not exist. The resulting gravitational forces on the interacting bodies are then used in Newton's laws of dynamics where they act as the source of the resulting dynamic changes.

If one objects to the positing of causal gravitational forces, one could easily bypass them altogether by substituting the expression for the gravitational force directly for the force term in Newton's laws of dynamics. In this way of thinking about Newton's theory, the static general facts are then just the positions and velocities of the objects, along with their masses (causal forces are altogether eliminated from the description of reality) at any particular time. Given these static facts at any particular time, the evolution of the facts through time can be calculated at any later time using the laws of Newtonian dynamics (no source terms appear when they are used in this way).

Note that, in this way of thinking of the world, a particular temporal sequence of states, described in terms of static general facts, is not *itself* a dynamic general fact: it requires an initial situation described in terms of static general facts (what scientists and mathematicians refer to as the initial condition) and a dynamic general fact (what the law-like evolution equation might correspond to) to determine a unique temporal sequence of states occurring through time. Also note that a dynamic general fact is not something that can be directly observed; rather, it must be inferred from static and dynamic observations, but static general facts are also inferred from observations!

I claim that, for scientists who deal with fundamental scientific theories, this is quite a natural way of talking about the world, but the terminology can vary. A scientist will typically say that the initial conditions along with the evolution equation determine how particular features of the world evolve through time. Given the initial conditions (*i.e.*, the initial static general facts) about the relevant properties of things (at the initial particular time of interest), the evolution equation (*i.e.*, the dynamic general fact) determines the future changes that those properties undergo.

Even the non-deterministic world of quantum mechanics is described in a similar way. Schrödinger's equation is an evolution equation that governs the evolution of the wave function. In order to uniquely *solve* this equation, one again needs an initial condition, but this time the initial condition will be an initial wave function. Thus, for example, given an initial wave function for a particle and a deterministic potential energy function, the evolution of the wave function for that particle is determined by solving Schrödinger's equation.

What is not so clear from the forgoing discussion is whether causal forces can always be eliminated from scientific theories in the way that they can be eliminated from Newton's laws of gravitational dynamics. Whether they can always be eliminated is an issue of philosophical importance, because, when we keep forces, scientists usually talk about the forces *causing* the change in motion and then talk of the laws either describing *or* governing the resulting motion. On the other hand, when the forces are eliminated and their effect is subsumed by the evolution equation, scientists are more apt to say that the laws govern the motion. In the former case the things that cause are the reified forces and laws of nature merely describe, whereas in the latter case the laws of nature take on a causal role themselves.

So the philosophical issue seems to be the question of what exactly dynamic general facts are. If dynamic general facts are genuinely part of our world, what kind of thing are they? We never actually observe static or dynamic general facts. Some ontological account of these things needs to be given.

B. Ontology: primitivism, reductionism, or eliminativism

There are three ontological stances one could take towards any purported thing or type of thing one talks about: one can a) accept it as fundamentally real; b) reduce it, without remainder, to other things that are fundamentally real; or c) eliminate it from one's ontology. It is possible to accept something as a fundamentally real part of our world and claim that a complete understanding of its nature is beyond human capacity, but someone is unlikely to be convinced that such a thing exists if nothing is known about its nature. To reduce something to other fundamental things is not to eliminate that thing from one's ontology: if a category of thing can be consistently reduced without remainder to other fundamental things, then there is still a sense in which the reduced thing is part of the ontology of the world. The "without remainder" clause is important, since it identifies the point at which many arguments regarding whether a thing has been successfully reduced arise. For example, I take emergentist theories of mind (*e.g.*, non-reductive physicalism) to be arguing that reducing the mind to neuronal activity leaves something out. The third stance says that there is no category of the purported thing in reality; when we refer to such things in our speech, we are either confused or speaking imprecisely. The first stance is *primitivist*; the second stance is *reductionist*; and the third stance is *eliminativist*. These three stances seem to exhaust the stances one can take toward a purported thing.

The term 'realism' might sometimes seem to be applied vaguely to a particular scientific theory, because some parts of the theory are reductionist, others primitivist, and still others eliminativist. In the molecular theory of heat for gasses, for example, one can be reductionist about the temperature of a particular volume of gas (the temperature being reduced to the average kinetic energy of the particles making up the gas) and then be a primitivist, or even an eliminativist, about particles. (An eliminativist about particles might claim that at its most fundamental level reality is structured by differentiable fields in a continuum.)

The different accounts of laws of nature can also be categorized as primitivist, reductionist, or eliminativist. Precise statements of these three non-overlapping and exhaustive stances towards laws of nature can be given; following Stephen Mumford (2005)

- *Primitivism* applied to laws of nature says that they exist as a distinct metaphysical category of thing.
- *Reductionism* applied to laws of nature says that they exist but that they can be accounted for entirely, without remainder, by other things that are not laws, that is, they can be reduced to those other things.

- *Eliminativism* applied to laws of nature says that they do not exist: they are not a distinct category of things and they cannot be reduced to any other category of things.

Mumford calls the first *nomological realism,* but this is a somewhat misleading name because a reductionist about laws of nature usually thinks that they are real; it's just that she thinks that they can be reduced to other more fundamental things. The various eliminativist positions on laws of nature are sometimes called *lawless* positions. Bas van Fraassen is an eliminativist about the laws of nature.

Categorizing a particular account of laws of nature as embodying one of these stances is not always an objective matter: whether any of the regularity theories of laws are claimed to be reductionist or eliminativist depends on one's preconceived notions of what some of the essential features of a law of nature ought to be. For example, if one comes to the metaphysics of laws of nature with the unassailable intuition that the laws must govern the things that go on in the world, then any of the regularity accounts, which I will shortly discuss, will seem eliminativist. For others who have the intuition that laws are epistemological tools for curve-fitting observational phenomena, the regularity accounts will justifiably be claimed to be reductive accounts.

C. Nomic concepts and the nomic

It is difficult to give a metaphysical account of laws of nature without also talking about the other nomic concepts. The philosophical-scientific conception of the world I described above, which roughly divides the world into static and dynamic facts, is useful to identify the metaphysical problem I'm interested in, but philosophers usually talk about a wide range of related nomic concepts.[3] There does not seem to be a fixed list of the nomic concepts, but it is useful to think of the nomic concepts as those that allow us to speak of the dynamic facts in the world. John Carroll (1994) describes the nomic concepts as a family of interrelated concepts that includes counterfactuals, causation, lawhood, explanation, chance, dispositions, natural necessity, and *their conceptual kin*. Now it would be difficult to deny that we have and use such concepts—the corresponding words are a natural part of everyday language—but what does the truth of a sentence that uses such concepts depend upon? What are the truth-makers for such sentences?

Carroll argues that there is no reduction of the nomic facts to non-nomic facts about the world, but he does not say what laws of nature are supposed to correspond to *in* the world. He claims that laws of nature are true statements that have no identifiable truth-makers; and, because he claims that the nomic concepts are an interrelated family of concepts, it can be assumed that he holds a similar philosophical position for the other nomic concepts.

So, for Carroll, a sentence like 'if *A* happens, then *B* must happen', where I'll assume that *A* and *B* are possible events, might be true, but what makes it true does not depend on the non-nomic facts about the world. It is just a primitive fact that this sentence is true; there are no nomic facts in the world.[4] Now this is a strange position to hold, because we usually think that sentences are made true by facts and that we can, at least sometimes, identify the ontology associated with a fact. But suppose we disagree with Carroll and think that true

3. In fact, it seems that philosophers have more terms for describing the contents and features of scientific theories than do scientists.

4. Carroll would probably claim that the sentence's being true is a nomic fact, but that is not what I mean by a nomic fact *in* the world.

lawlike sentences ought to have truth-makers. In that case, what are the facts that make such sentences true? Suppose that the events *A* and *B* have happened. One might want to say that they are part of the totality of facts that makes the sentence true; but, upon reflection, we notice that the sentence could still be true if *A* does not happen. *A*'s happening is not essential to the truth of the sentence, and *B*'s happening is essential only if *A* happens. If this lawlike sentence is true, then *B must* happen whenever *A* happens. If we take the modality of this sentence seriously, then we might want to posit some (possible?) *necessary connection* between the two possible events that makes the sentence true. But Carroll (1987) claims that "(5.4) All reductive accounts of laws fail" (1987, p. 275). He then concludes that

> (5.4) appears to be somewhat discouraging in that it looks like there is not much for philosophy to say about the nature of nomic facts. However, in this case, appearances are deceiving. Conclusion (5.4) indicates how explanation of the nature of laws and other nomic facts should proceed. Explanation of the nomic requires recognising that laws or some nomic concepts (e.g. physical necessity or causation) are primitive and irreducible. These primitives, then, might be used to give the truth conditions of other nomic statements. Furthermore, the conclusion that nomic statements do not have reductive truth conditions raises exciting epistemic and semantic issues. A complete account of how cognisers come to have knowledge and justified beliefs involving irreducibly nomic notions becomes mandatory. This suggests that conclusion (5.4) is not a discouraging conclusion at all. It encourages new work and reorients old work on the nature of nomic facts. (*ibid*., p. 276)

Some might want to call Carroll a realist about laws of nature; but he could just as well be taken to be an eliminativist, because he posits nothing in the world corresponding to laws of nature. The interrelated family of nomic facts, which new work is supposed to uncover, may not bode well for laws of nature.

It is possible to be an eliminativist about laws of nature but a realist about some of the other nomic concepts. Mumford (2005) argues that at least some of the nomic concepts correspond to real features of the world and he claims that the following list *S* names at least some of the real features of the world:

> *S* = {regularity, universality, objectivity, immanence, invariance, centrality, measurability, contingency, natural necessity}

But he also argues laws of nature are not what deliver any of these features. Mumford calls the position he wants to occupy *realist lawlessness*. He says that, although the things in *S* are actual features of our world, "there is not a single and distinct kind of thing, called laws, that delivers all these features by imposing them on top of an otherwise lawless reality" (2005, p.407). He thinks that there is no univocal concept corresponding to our use of 'law of nature'.[5] But Mumford believes that much of the nomic can be reduced to powers *in* things, what he calls *de re* modality. I will be looking at accounts of laws of nature that posit powers in the final chapter of this thesis.

So Mumford is an eliminativist about laws of nature but he contrasts his position with what he calls *Humean lawlessness*, according to which, typically, all of the nomic concepts are reduced to regularities (this is a misnomer because a regularity theorist believes that laws of nature really exist; it's just that she thinks that they can be reduced to regularities). I will describe some of the features of the various regularity theories of laws of nature, and of causation, in some detail because, in one form or another, they have been claimed to be the correct reductive analysis by many philosophers.[6]

5. An eliminativist about laws of nature might argue that we use the term 'law of nature' in the same way that we might use the term 'music': does 'music' refer to the composer's thoughts or her brain patterns, the musical score, the sound waves, the receiver's brain patterns or her thoughts, and does it refer to specific instances of each or the type?

6. David Armstrong (1983) has given several arguments for why regularity accounts won't work.

2. Humean Scepticism and Regularity Theories

Regularity analyses of the laws of nature strive for minimalism in what they posit. The main proponents of this philosophical account of laws of nature are those, such as the logical positivists of the early twentieth century, who are influenced by Hume's epistemic scepticism regarding our access to any natural necessities that might bind effects to their causes. Nomic features such as natural necessities and primitive laws of nature are considered mysterious[7] entities that should be banned from our ontology, whereas that observable regularities exist is taken as a brute fact that requires no explanation: talk about regularities is not considered problematic.

Hume is credited with making two important points that are the foundation of most regularity theories of causation (*i.e.*, exactly those that assume a regularity theory of laws of nature).[8] The first is the epistemological point that we have no access to mysterious *natural necessities*;[9] the second has to do with Hume's metaphysical view that the only necessity is logical necessity (*i.e.*, there is no such thing as natural necessity). The two points are related in that the second implies the first, but the first does not imply the second. If there is no such thing as natural necessity, then there can be no epistemic access to it, whereas, even if there is natural necessity in the world, we might not have access to it. Both points are meant to

7. Because they are inaccessible to normal modes of observation via our senses.

8. I will ignore the possibility of singular causation because, although it might indeed occur, it is by definition not related to laws of nature. According to Armstrong, "The Regularity theory of causation appears to be a conjunction of two propositions: (1) that causal connection is a species of law-like connection; (2) that laws are nothing but regularities in the behaviour of things" (1983, p. 11).

9. It is common when discussing Hume's views to use the term 'natural necessity' to refer to any real causal force that necessitates things in the world. A natural necessity is one of the nomic features of the world that cannot be reduced to the Humean base (*i.e.*, the non-nomic base), but it might be reducible to other nomic features of the world. I will sometimes use 'causal forces' or 'powers' or just 'nomic features' when discussing natural necessities and the details of the nomic feature don't matter.

justify the claim that the laws of nature are just regularities; regularities are supposed to be easily accessible via our senses, and logical necessity is not supposed to be problematic.

A. Hume's logical point: no natural necessity

I will discuss Hume's logical point based on A. J. Ayer's writings in "What Is a Law of Nature?"[10] Ayer's empiricist stance, founded on Hume's scepticism about what is not directly observable, leads him to a regularity theory of laws of nature. Ayer agrees with Hume that there are no natural necessities in the world. He describes Hume's insight into the error we make in appealing to a law of nature to explain recurrent pairs of cause and effect as follows:

> But the point of Hume's argument is not that the relation of necessary connection which is supposed to conjoin distinct events is not in fact observable: it is that there could not be any such relation, not as a matter of fact but as a matter of logic. What Hume is pointing out is that if two events are distinct, they are distinct: from a statement which does no more than assert the existence of one of them it is impossible to deduce anything concerning the existence of the other. This is, indeed, a plain tautology. Its importance lies in the fact that Hume's opponents denied it. They wished to maintain both that the events which were coupled by the laws of nature were logically distinct from one another, and that they were united by a logical relation. But this is a manifest contradiction. (*ibid*., p. 811)

According to Ayer, Hume's point isn't that we have no epistemic access to the glue-of-necessity that binds causes to their effects; rather, it's that there *cannot be* any glue-of-necessity, *because* the only necessity is logical necessity, and unlike putative natural necessities logical necessities are derivable using only the laws of logic. But, given two *independent* events, *c* and *e*, from a sentence, *C,* claiming the existence of the first event, there is no way to derive a sentence, *E,* claiming the existence of the other event *using only*

10. The article entitled "What Is a Law of Nature?" which is reprinted in Curd and Cover (1998, pp. 808-825), is from *The Concept of a Person* (1963, pp. 209-234).

the laws of logic. I accept Hume's point, but the obvious response is that I don't think that any realist regarding the nomic would claim that, when she derives *E* from *C,* her derivation proceeds using only the laws of logic. She will claim to be using a law-sentence that she takes to be true because of a previous abductive argument and that her derivation then proceeds using only the laws of logic. In any world where that law-sentence is true, the derivation can proceed. *E* follows from *C* by natural necessity, not logical necessity, and the law-sentence is a contingent statement that is inferred to be true in our world.

But Ayer's complaint might be with the abductive step that is required to accept the law-sentence as a true sentence. I will deal with that complaint shortly; but, first, I will briefly review regularity accounts of laws of nature and causation.

B. Epistemic regularity analyses of laws of nature

For the regularity theorist, the laws of nature are reduced to some of the regularities in the world. The first problem, of course, is that not all regularities are laws of nature and an acceptable reductive analysis needs to be given if we are to retain 'laws of nature' as a theoretical term of our language. Various regularity theories have been advanced that try to come up with the features of a regularity that are necessary and sufficient for that regularity to be a law of nature. Some of the purported features of a law-proposition that have been claimed by philosophers to be identifying features are[11]

(1) our psychological attitude of acceptance towards that law as a tool for prediction even when it is confirmed by only a relatively small number of observations;

11. See Ayer's "What is a Law of Nature" (reprinted in Curd and Cover, 1998, pp. 808-825), as well as the commentary by Curd and Cover (pp. 883-884). A nice summary account of Humean regularity accounts of laws is given by Bigelow and Pargetter (1990, pp. 231-238).

(2) the status that law has as a socially acceptable part of the current science;

(3) the practical utility of that law;

(4) how closely the law is related to observed phenomenal evidence; and

(5) the logical, or other formal, features of the law and its relationship to other laws. *E.g.*, whether it is a perfectly general proposition, consistent with other laws, and whether it coheres well with other accepted laws, *etc*.

Clearly, except for the last feature, these involve humans to a large extent and therefore any regularity theory of laws that makes use of points (1)-(4) is sometimes called an *epistemic regularity theory*. If we are looking for an objective metaphysical theory of laws of nature, then clearly any epistemic regularity account will not do.

C. Naïve objective regularity theories of lawhood

A simple analysis to keep in mind, one that has many of the philosophically important features of most objective regularity accounts, is the so-called *naïve regularity theory of lawhood*. In this theory, a concise definition of lawhood has been formulated by George Molnar (1969, p. 79) and is repeated by Armstrong (1983, p. 12). It says that *p* expresses a law of nature if and only if

(1) *p* is universally quantified;

(2) *p* is true;

(3) *p* is contingent; and

(4) *p* contains only non-local[12] empirical predicates, apart from logical connectives and quantifiers.

12. By 'non-local', Armstrong means that predicates such as 'Australia' be excluded from lawlike propositions. Thus, 'All the ravens in Australia are black' is a universally quantified contingently true sentence that does not qualify as making a lawlike claim. Similarly, this excludes 'All coins in my pocket are nickels' from being a lawlike sentence.

This characterization makes use of only logical and formal features of p; it does not make use of any psychological or sociological factors. (More sophisticated regularity accounts are available, and I will be discussing the Best System Analysis (BSA) reduction of laws to regularities later in this thesis.) Although this regularity theory is objective, the laws of nature are merely descriptions of the regularities. A law of nature, on this account, does not have features that could be claimed to govern what goes on in the world. Whether some governing features are *required* in the world is a question that I will turn to in the final sections of this chapter.

Causation is one of the nomic concepts that features prominently in scientific theories and is sometimes taken to be a governing feature of the world. I've previously argued that it might be possible to eliminate causal forces from scientific theories, but then the governing feature will usually be accounted for via the laws of nature. Can the regularity theorist give an account of causation that remains compatible with her laws-as-regularities without positing primitive nomic entities? If she can't, then what would be the purpose of having the laws supervene on regularities if she still has to introduce primitive nomic entities to take care of causation? I now turn to two such possibilities that show that she can give a compatible account of causation.

D. Causation for the regularity theorist

The regularity theorist about laws of nature will want a purely descriptive account of causation. Lewis (1973) gives two accounts of causation consistent with the regularity account of laws of nature. The first account, the *regularity account of causation*, says that the inference from C to E is justified by the relevant law propositions we take as true. His concise statement is formulated in terms of events c and e, the cause and effect events:

> let C be the proposition that c exists (or occurs) and let E be the proposition that e exists. Then c causes e, according to a typical regularity analysis, iff (1) C and E are true; and (2) for some nonempty set $\mathcal{L}$ of true law-propositions and some set $\mathcal{F}$ of true propositions of particular fact, $\mathcal{L}$ and $\mathcal{F}$ jointly imply $C \supset E$, although $\mathcal{L}$ and $\mathcal{F}$ jointly do not imply E and $\mathcal{F}$ alone does not imply $C \supset E$. (1973, p. 556)

This regularity account follows closely the *deductive-nomological* account of explanation, according to which law-propositions are used to infer the *explanandum*, and thereby $\mathcal{L}$ and $\mathcal{F}$ together form the *explanans* of the explanation. The account might be something that the non-regularity theorist realist would approve of except that here the laws of nature are construed simply as regularities. Lewis cites various problems with this account of causation—the account sometimes confuses other effects and epiphenomena with genuine causes—but the main problem I see, which he does not mention, is that, because the laws of nature involved in causation do not explain the regularities they're derived from, any use of this definition of causation would not be able to explain any particular future instance of causation.

In any case, Lewis abandons this definition and goes on to formulate the *counterfactual analysis* of causation by first formulating *causal dependence* between two events as follows:

> *e depends causally* on c iff the family $O(e)$, $\sim O(e)$ depends counterfactually on the family $O(c)$, $\sim O(c)$. As we say it: whether e occurs or not depends on whether c occurs or not. The dependence consists in the truth of two counterfactuals: $O(c) \mathbin{\Box\!\!\rightarrow} O(e)$ and $\sim O(c) \mathbin{\Box\!\!\rightarrow} \sim O(e)$. (1973, p. 563)

where $O(x)$ represents the proposition that holds at all and only those worlds where x occurs. We get causes from causal chains as follows:

> Let *c*, *d*, *e*, . . . be a finite sequence of actual particular events such that *d* depends causally on *c*, *e* on *d*, and so on throughout. Then this sequence is a *causal chain*. Finally, one event is a *cause* of another iff there exists a causal chain leading from the first to the second. (*ibid.*)

The counterfactual relation between any two propositions, he defines as follows:

> Given any two propositions A and C, we have their *counterfactual* $A \mathbin{\Box\!\!\rightarrow} C$: the proposition that if A were true, then C would also be true. The operation $\Box\!\!\rightarrow$ is defined by a rule of truth, as follows. $A \mathbin{\Box\!\!\rightarrow} C$ is true (at a world w) iff either (1) there are no possible A-worlds (in which case $A \mathbin{\Box\!\!\rightarrow} C$ is *vacuous*), or (2) some A-world where C holds is closer (to w) than is any A-world where C does not hold. In other words, a counterfactual is nonvacuously true iff it takes less of a departure from actuality to make the consequent true along with the antecedent than it does to make the antecedent true without the consequent. (*ibid.*, p. 560)[13]

The laws of nature contribute to Lewis's definition of causation in his account of the similarity relation between worlds. For any proposition A, an A-world is simply one of the worlds in the set A: propositions are just sets of worlds where that proposition holds. And the closeness of worlds is defined by the relative position between worlds in the weak ordering imposed by Lewis's *relation of comparative over-all similarity*. This relation is to be a primitive relation that compares worlds on matters of actual fact and the laws of nature of each of the worlds. The vagueness of the relation—that the similarity of two worlds will come out differently based on what we emphasise—is a feature that Lewis claims to be an advantage that enters into his account of causation. Now the laws of nature are buried a little deeper in this analysis, but they are still an essential part of the account. I will take up whether this account of causation fares any better at giving a proper explanation for instances of causation after I give a brief overview of what I mean by a proper explanation.

13. See also Lewis (1973a) for a detailed analysis of the counterfactual.

3. Metaphysical Explanation as Justification for the Nomic

A. Metaphysical explanation

The concept of explanation, and its uses, has been a problem for philosophers of science for the past several decades. In a recent paper, Alexander Bird (2005) argues that there are really two types of explanation, what Bird calls *subjective explanations* (S-explanations) and *objective explanations* (O-explanations). The latter, which I will sometimes just call metaphysical explanations, require that explanations be factive, *i.e.*, that there be real elements to explanations. This does not preclude explanations from being false, because the putative real elements might not exist. On the other hand, S-explanations are dependent on the information that the explainer thinks is important. I claim that we require some of the nomic features of the world to be real primitive features so that we have a basis for O-explanations using our scientific theories. Positing real primitive laws of nature, or one of the other nomic concepts, is one way to *end* the explanation process.

B. Inference to laws that explain

Now returning to what might be Ayer's point, that a law-sentence itself cannot be known to be true in our world, if Ayer's claim is that we are required to derive the law-sentence using only the laws of logic, then I don't see how any account of laws of nature can deliver that. But I don't accept that rationality involves using only laws that are *known* to be true. Psillos's epistemic thesis, which I described in the previous chapter as part of the realist's stance, allows that our law-sentences might be only approximately true. The usual realist justification for accepting a law-proposition follows a bit of abductive reasoning similar to the following:

(1) certain regularities are observed;

(2) a law of nature is *inferred* from the observations;

(3) the law of nature *explains* the observed regularities; and

(4) the law is used to *predict* that future observations will follow the same regularity.

Points (1) and (2) are the same for anyone who is non-eliminativist about laws of nature, including the regularity theorist, and (2) will be considered a fallible inference by all accounts. But only the nomic realist can say that her laws explain the observed regularities, as in (3), because the regularity theorist's laws are just the regularities, and a thing can't explain itself. The fact that the nomic realist's laws of nature explain the observations is sometimes said to (i) help confirm the inference in (2) and (ii) require fewer future observations to bring us more epistemic confidence in that inference than if the law was reduced to the mere regularities themselves. Of course, an inference to some particular law will be shown to be unjustified if future observations don't come out as predicted, but it is usually taken as part of correct abductive reasoning that the laws should be of a sort that allow new predictions to be made and do not contradict other laws of broader scope.

The abductive reasoning process that I've just outlined is sometimes called *inference to the best explanation* (IBE).[14] It can be argued that the use of abductive reasoning in science is responsible for the successes of science and that this is sufficient to justify the continued use of such inferences. But the philosopher must ask whether abductive reasoning also justifies assent to the existence of the inferred entity. I claim that IBE can be rational only if the inferred entities (*i.e.*, laws of nature in our case, but also unobservable entities like

14. The term *inference to the best explanation* seems to have been introduced into the modern philosophical literature by Harman (1965).

electrons) are taken to really exist and only if a *causal* mechanism is invoked as part of the explanation. Thus, I think that a better name for the type of reasoning process I'm promoting here is *inference to the best causal explanation* (IBCE).

Lewis also has a theory about explanation. His main thesis of explanation-as-information says that "*to explain an event is to provide some information about its causal history*" (1986b, p. 217, italics in original). So he too believes that explanations must have a causal component to them. Now causal explanations, where the causes are taken to be primitive and real, give us O-explanations: metaphysical explanations that justify the inference to the same laws that form part of the causal explanation. But causal explanation, where causation is explicated in Lewis's sense, does not contain any factive component above and beyond the regularities that the laws of nature are reduced to.

By following through with Lewis's counterfactual analysis of causation on a specific problem, we will clearly see that we will be left with no explanation for the future instances of a causal regularity. Suppose that A is the proposition that a particle is emitted from the particle gun before me in the next minute and C is the proposition that it will have negative charge. We want to know the truth of $A \mathrel{\Box\!\!\rightarrow} C$: if it were the case that a particle is emitted from the particle gun before me in the next minute, then it would be the case that the particle will have negative charge. Let us assume that it is true that in the next minute a particle will be emitted from the particle gun before me. Therefore, the closest A-world will be the actual world, because A is true in the actual world. If the actual world is also a C-world, then the counterfactual is true; if the actual world is not a C-world, then the counterfactual is false. The actual world is a C-world if the next particle I observe is negatively charged. Now suppose I think that it is a law of nature that all particles emitted from the type of particle gun before me have negative charge, and I think this because of the regularity that every particle I've observed so far being emitted from that type of gun has been negatively charged. Then, because of this law, I will think that the actual world is a *C*-world and, therefore, I will take the counterfactual $A \mathrel{\Box\!\!\rightarrow} C$ to be true. That is, I will have inferred the law that all particles emitted from the type of gun before me have negative charge without having an explanation for the law. So we get the same result as for the deductive-nomological account of causation: the regularity theorist of laws of nature will not be able to explain individual instances of causation.

The Ramsey-Lewis Best System Analysis (BSA) of laws of nature is a sophisticated regularity theory that Lewis insists is not an epistemological theory but rather a metaphysical one.[15] I will consider Lewis's account of laws in more detail in Chapter IV on BSA and objective chance, where I unpack his analysis of what it means to be the *best system*. With respect to being able to *explain* the laws, BSA might be seen to fare somewhat better if we accept that what gives us the *best* system is an objective part of the world. But, as I will argue in the next chapter, I don't think that any account of laws that supervenes on the Humean base can take objective chance into proper account.

4. Realism About Governance

I've argued at several places in this thesis that fundamental scientific theories incorporate a realism with respect to governance: that there are things in the world that *govern* the way things happen. Other locutions will imply the same thing: we might say that certain things *cause* other things, that there is a *natural connection* between things, or that

15. See Lewis, 1994, p. 481-482. I quote Lewis's claim that I'm referring to in Chapter IV.

there exists a *glue* that holds things and events together. Now whether laws of nature are said to be one of the things that govern probably depends on what other primitive modalities are taken to exist as part of the ontology of the world. By *primitive* modalities, I mean things like primitive natural necessities, dispositions or powers,[16] existing in things or in the world as a whole. If one is a realist about governance but thinks that laws of nature are not fundamental, then she will probably reduce laws to one of these other primitive modalities. On the other hand, the non-realist about governance probably thinks of our world as a regularity-filled Humean world of successive events with no natural necessities connecting events and takes the laws of nature, if she accepts them at all, as *descriptions* of the world. It seems that both views have been held by the great scientific and philosophical historical figures back to antiquity. An interesting survey is given by Jane Ruby (1986), who claims that our use of 'law' doesn't have its historical origins in the notion of a divine legislator, as one might think. Ruby cites uses of 'law' to refer to regularities in nature by Roger Bacon (c. 1210 - c. 1292) and others. It seems that philosophers have clung to the idea that "law of nature" carries the notion of necessity and universality, a point that van Fraassen makes at the beginning of his *Laws and Symmetry* (1989). Although scientists often refer to their laws as governing, they don't usually go into a philosophical treatment of how this can be so.

One thing should already be clear from the metaphysical framework I've just advanced for regularity theories: it would be begging the question to criticise a regularity theory of laws on the grounds that there is no plausible means by which laws, as so conceived, could

16. Galen Strawson calls these *producing forces*; see below.

play a governing role in nature.[17] This is because a regularity theorist is already assuming a non-governing role for his laws of nature; laws of nature as conceived by any regularity theory can play only a descriptive role. The only way I can see for laws of nature that are based on regularities to play a quasi-governing role is in the same way that God's omniscience might be said to limit our free will. This can be explained in terms of Lewis's BSA account, but the argument would be the same for any regularity account. If the best system of laws supervenes on the total history of the world, then there is a sense in which knowledge of a BSA law-statement *constrains* something to happen. If, for example, on the BSA account the law that nothing travels faster than the speed of light obtains, then that is because in fact nothing does travel faster than the speed of light (nowhere and nowhen). How we obtain this knowledge is not relevant to the argument. If something did, in the future, travel faster than the speed of light, then it would not be a law that nothing travels faster than the speed of light!

The obvious objection to this is that we don't really have an independent cause (governing entity, constraint, *etc.*)[18] here. What causes something to happen should be different than the cause itself: effects should be different from their causes. Any account of laws of nature in which the laws supervene on the Humean facts in the world is an account where the laws cannot cause any fact to obtain.

17. See Beebee (2000) for an analysis of how purported refutations of some regularity accounts of laws of nature fail exactly because they beg the question in this regard.

18. There is no need to argue here about which of these words is the applicable one. Of course there are subtle differences between what any of these, or any other similar word, might be taken to mean in the context of laws of nature, but the allusion to something having the ability to *cause, govern*, *necessitate,* or *constrain* that distinguishes laws described by any of these words from laws described by any of the regularity accounts will be the same.

A. Laws of nature that govern

Can a philosophical argument that the laws of nature (or related nomic feature of the world) govern be given? That laws of nature govern seems, for many philosophers, to be one of their essential features. Nancy Cartwright writes,[19]

> Whatever else we mean, I take it that this much is essential: Laws of Nature are *prescriptive*, not merely descriptive, and – even stronger – they are supposed to be *responsible* for what occurs in Nature. (forthcoming, p. 1)

The main reason to believe that there are governing entities in the world is to give a reason for the regularities we observe. I think that it is absurd to believe that regularities exist but that there is no reason why they exist, that it is just a brute fact that there are regularities. Can a convincing general philosophical argument be given against regularity theories of causation that attacks precisely the point that they are construed as mere regularities?[20] I think that Galen Strawson (1987) has given such an argument.

Strawson claims that *Regularity Realism*, the combination of realism with respect to an objective external world and a regularity theory of causation, is highly implausible, and he says that "realism with respect to the external world rationally requires the adoption, in some no doubt philosophically developed version, of the ordinary, common-sense view of what causation is, in the world" (1987, p. 253). According to Strawson, that common-sense view of causation is the view that there are objective forces in the world and to say that "one

19. I'm not actually clear on whether or not Cartwright believes that there are governing entities as part of our world, but she has been characterized as a realist about entities and an anti-realist about laws of nature.

20. Armstrong (1983, p. 11) points out that the regularity theory of causation entails the regularity theory of laws of nature, because the former says that causal connection is a type of lawlike connection *and* laws are nothing but regularities. So a general argument against the regularity theory of causation is not necessarily a general argument against the a regularity theory of laws of nature: someone could deny that causation is a type of lawlike connection (as singularist theories of causation do) and still believe in a regularity theory of laws. But the argument I will rehearse here attacks the regularity account of causation *because* it incorporates a regularity account of laws, so though a retreat to a singularist account of causation is possible it will not help the regularity theory of laws.

object-involving event A caused another object-involving event B is to say that, given the existence and the nature of the forces *informing* and *governing* the objects involved in A and B, the occurrence of A (i) produced or gave rise to or brought about and (ii) necessitated the occurrence of B" (p. 255, my italics). He calls this common-sense notion of causation *Producing Causation Realism*.

Strawson says that we introduce objective forces into our ontology precisely because the type of causation implied by words such as 'affect', 'modify', and 'produce' is compatible with singular causation: they allow causation without regularity in the world's behaviour. But our intuition says that the causation in the world is regular and therefore it is scientifically natural and philosophically sound to introduce objective forces: the objective forces are simply the *reason* for the regularities. Strawson does not offer a complete philosophical analysis of objective forces (partly, I believe, because there is not much more that can be said about them from a metaphysical point of view; it is up to science to tell us how those forces behave), but he claims that belief in the *existence* of such forces is "part of what it is to take seriously the Realist assumption that temporally continuant objects have continuously temporally persisting properties or natures" (1987, p. 262). So Strawson is appealing to our everyday intuition as an argument for the existence of objective forces. I've already argued that a similar intuition is part of the scientific enterprise of theory building, at least at the level of fundamental scientific theories.

I will now consider Strawson's argument that there *must* be ontological reasons for the regularities. He formulates this argument as an objection to the Regularity Realist. He argues that, because the Regularity Realist believes in regularities, she must also believe that there are ontological reasons for those regularities and that it would be absurd to believe that nothing grounds those regularities. Helen Beebee has recently defended Regularity Realism against Strawson's arguments in her "Does Anything Hold the Universe Together?" (2006); I will follow her description of Strawson's arguments.

Beebee first argues that, though the traditional regularity realist is not forced into such a position, she "ought to hold that there is, in fact, no reason why the universe is regular" (2006, p. 524). In the second part of her paper, she argues against three of Strawson's objections to this view, the first of which is the one I'm dealing with here: namely, that "it is absurd, given the regularity of the world, to say—to insist—that there is definitely no reason in the nature of things why regularity rather than chaos . . . occurs from moment to moment" (1989, pp. 21–22).[21]

Reacting to Strawson's claim that, if the regularities are considered a "fluke," then this would be extremely surprising, her first objection is that the regularity theorist need not give in to the demand that she postulate a something-in-virtue-of-which-the-universe-is-regular as part of an adequate ontology. To give in to such a demand is to try to deal with the natural *inductive vertigo* we get when we reflect on what it is that holds the world the way that it is.[22] Beebee believes that the regularity theorist can tolerate the demand to posit a reason for the "surprising" regularities by arguing as follows:

> I myself think that it can be tolerated. After all, we have mostly learned to tolerate other outrageous runs of luck. Consider, for example, how spectacularly lucky you are to exist at all. It's extraordinary enough that conditions on Earth have for a long time been, and continue to be, compatible with any life at all; the margins for error (climatic conditions, composition of the atmosphere and so on) are very narrow. But that's not the half of it. Consider how many events had to happen in order for you to be conceived, and how many occasions there must have been on which either one of your future parents might unwittingly have said or done something that

21. As quoted in Beebee 2006, p. 524.
22. Here Beebee is using Blackburn's terminology.

> would have put your future existence in jeopardy. Then go back a generation to both sets of grandparents, and so on, and on. When you think about it, your own existence is an extraordinary fluke. How much does this bother you? Most likely, not at all. You therefore probably have absolutely no inclination to suppose that there is some kind of underlying 'reason' why things throughout history panned out in such a way as to produce you. (2006, p. 527)

It seems that Beebee wants us to concede that the events of the world are flukes, but I think that she is missing the point. Just because there is a unique sequence of events that has led to me existing now, with the unique properties that I currently have, and that a similarly unique sequence is the precursor of *every* event, that does not make the end-event in any such sequence an "extraordinary fluke." Under the producing causation view, the realist believes that most of the events in the sequence of events that have led to any particular person's existence, the events that Beebee refers to in the above argument, have reasons for their existence *based* on previous events that can ultimately be explained by the existence of forces (deterministic or chancy) obtaining in the world. Under that point of view the randomness of the sequence is a consequence only of the randomness of the supposed initial event of the sequence in addition to the randomness of the acting forces. This might be extraordinary, or it might not, depending on your view of these things; and I agree that this type of uncertainty probably doesn't bother you. On the other hand, a regularity theorist cannot assume any dependence among the events and therefore my existence, and continued existence, does become an astronomical fluke. If I did believe that this type of randomness obtained in the world, then it would bother me and I wouldn't be surprised to see "lkwejlwejckekhcewkke" written on this page as opposed to what I do see.

In addition, if we assume that the individual types of events in any actual sequence are *independent* of others, as Strawson claims a regularity realist must, then we have absolutely no grounds for prediction. One might want to claim that, only if we assume that the events in a sequence of events are *dependent*, can we make the type of predictions that we do as to the further events flowing from that sequence.

But Beebee follows the above quoted argument, that the sequences we find in the world are flukes, with an argument against Strawson that a world *without reasons* for the regularities does not have to be like a world of randomly generated independent events forming a sequence in time. She claims that we know that the world is "an incredibly ordered place" and that this knowledge allows us to make predictions, that no other posits in the form of objective forces are needed. She claims that our knowledge of regularities is a sufficient stopping point for our explanations. But to assume regularities in a sequence of events, or even some statistical regularities, does not explain those regularities, and this is exactly the point of contention between any regularity theorist and a nomological realist that Strawson identifies clearly:

> Now it is true that all explanations must come to an end. But some stopping points are vastly preferable to others. Given Realism (object realism), the phenomenon of (observable) regularity is not in itself a tolerable stopping point. It demands a reason, a ground. Objective forces are a possible stopping point. (1987, p. 266)

Strawson emphasizes the point that a regularity realist already believes in objects as existing in the world and that, given this realism, she should take the further step of positing forces.

As in most things metaphysical, I don't think that there is a way to convince a regularity realist that more is needed if her intuitions do not match ours. I don't think that a scientific realist should have much hesitation in accepting the existence of objective forces, and I believe that more than one coherent metaphysical theory can be developed that incorporate such forces.

IV — Best System Analysis and Objective Chance

We would like to emphasize a very important difference between classical and quantum mechanics. We have been talking about the probability that an electron will arrive in a given circumstance. We have implied that in our experimental arrangement (or even in the best possible one) it would be impossible to predict exactly what would happen. We can only predict the odds! This would mean, if it were true, that physics has given up on the problem of trying to predict exactly what will happen in a definite circumstance. Yes! physics *has* given up. *We do not know how to predict what would happen in a given circumstance*, and we believe now that it is impossible—that the only thing that can be predicted is the probability of different events. It must be recognized that this is a retrenchment in our earlier ideal of understanding nature. It may be a backward step, but no one has seen a way to avoid it.
— Richard P. Feynman (1918-1988)
(1963, Vol. III, p. 1-10, italics in original)

In this chapter, I tentatively accept that laws of nature might be things that merely describe the events of our world and go on to consider Lewis's Best System Analysis[1] (BSA) of laws of nature, which is a sophisticated reduction of laws to regularities. Lewis's BSA assumes Humean Supervenience (HS), that the laws of nature given by his BSA supervene on the Humean base.[2] After reviewing BSA in conjunction with HS, I review and accept Lewis's Principal Principle as the obvious rule for relating subjective credence to objective chance, telling us what to believe about chancy situations—it tells us how to set our degrees

1. Sometimes called the Mill-Ramsey-Lewis Best System Analysis (BSA) of laws of nature.
2. Lewis's BSA is the best developed regularity theory of laws of nature currently available.

of belief, our credence, about objectively chancy situations. I then review the undermining problem and Lewis's *big bad bug*, which infests the Principal Principle, and consider Lewis's proposed modification of that principle: conditionalizing on the theory of chance. I argue that, because this modification supposes that the theory of chance is itself chancy, it (1) is metaphysically unacceptable in conjunction with BSA and HS and (2) entails that we have no way of knowing how our credence relates to chance, because we have no way of determining what the chances of a particular theory we are using being true are.

I conclude by considering and rejecting both Vranas's and Roberts's solutions and claiming that the only way to make sense of the Principal Principle is to assume that chance laws do not supervene on the Humean base.

1. Humean Supervenience

So why consider a sophisticated regularity theory of laws of nature that attempts to overcome the problems associated with the simpler regularity accounts? Probably the most persuasive reason is that one wants to hold a very sparse ontology that includes only events at spacetime points or regions. Lewis states that much of his life's work has been a campaign on behalf of a *Humean Supervenience* (HS) thesis (Lewis, 1986, "Introduction"). This is "the thesis that the whole truth about a world like ours supervenes on the spatiotemporal distribution of local qualities" (Lewis, 1994, p. 224).[3] More precisely, Lewis describes Humean Supervenience as

> the doctrine that all there is to the world is a vast mosaic of local matters of particular fact, just one little thing and then another. ... We have geometry: a system of external relations of spatio-temporal distance between points. Maybe points of

3. Page numbers quoted for Lewis 1994 refer to pages in Lewis 1999.

> spacetime itself, maybe point-sized bits of matter or aether or fields, maybe both. And at those points we have local qualities: *perfectly natural intrinsic properties* which need nothing bigger than a point at which to be instantiated. For short: we have an arrangement of qualities. And that is all. There is no difference without difference in the arrangement of qualities. All else supervenes on that. (Lewis, 1986, pp. *ix*-*x*; italics are mine)

Lewis's claim is that HS is contingent and therefore an empirical issue; it is up to physics to tell us if it obtains in our world. What Lewis wants to defend is the philosophical tenability of HS. He wants to show that it is logically possible that *all* the features of our world, including the laws of nature, not only supervene but be determined by the spatio-temporal distribution of local qualities in the world: that is, that they can be reduced to the spatio-temporal distribution of local qualities. The way the world is in terms of the instantiated local qualities over the total history (past, present, and future) of the world determines the laws that obtain in the world. Clearly, if such a reduction is true, the laws of nature are *not* more fundamental than the spatio-temporal distribution of local qualities; the laws act as descriptions of the world rather than as things that determine the way things are in the world.

2. Best System Analysis of Laws of Nature

Lewis follows John Stuart Mill and Frank Ramsey in reducing the laws of nature to the true theorems of the best deductive system that describes our world. For a deterministic world, Lewis defines his own version of the BSA 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. How good a balance that is will depend on how kind nature is. A regularity is a law iff it is a theorem of the best system. (1994, p. 478)

By "best," Lewis means a vaguely defined "as good as truth will allow" balance between strength and simplicity. He doesn't try to give an analysis of how to trade-off between simplicity and strength when comparing deductive systems. So as not to get simple systems that imply all truths, on the cheap, by including perverse artificial predicates, Lewis says that the primitive vocabulary appearing in the axioms of the candidate systems should refer only to the perfectly natural properties.

> In fact, the content of any system whatever may be formulated very simply indeed. Given system *S*, let *F* be a predicate that applies to all and only things at worlds where *S* holds. Take *F* as primitive, and axiomatise *S* (or an equivalent thereof) by the single axiom $\forall xFx$. If utter simplicity is so easily attained, the ideal theory may as well be as strong as possible. Simplicity and strength needn't be traded off. Then the ideal theory will include (its simple axiom will strictly imply) all truths, and *a fortiori* all regularities. Then, after all, every regularity will be a law. That must be wrong. (1983, p. 367)

This is why he needs primitive natural classes, as properties, or universals. The best theory will be stated in terms of primitive properties. Natural classes, simplicity, strength, and the idea of best balance, as used in Lewis's BSA, are supposed to be objective aspects of our world. That is, they don't depend on us in any way, although we might have special access to them.

A. Chance: objective probability

Our currently accepted science posits chance in the world; for example, the objective probability of microscopic events are described by the laws of Quantum Mechanics[4] (QM), *e.g.*, Schrödinger's equation. If we accept that chance is a feature of our world, or even if we

4. I take Quantum Mechanics to include Quantum Field Theory.

accept that chance is a possible feature of our world, then we should allow for it in our philosophical account of reality and, more specifically, our philosophy of science should make room for it in a consistent manner (not as a vague concept that we hope will go away). On the other hand, if we believe that chance is not a true possibility, then we will have to re-interpret what exactly QM, or any other scientific theory that postulates chance, is talking about. It seems high-handed of philosophy to take the second route, so the only option seems to be to draw out the philosophical consequences of scientific theories that posit chance. Lewis wants to incorporate chance into his BSA while keeping the HS thesis and therefore wants to have chance supervene on the arrangement of qualities at spacetime points. Lewis's goal is to accommodate *laws* that give the chances of something coming about, *i.e.*, chance laws, within his version of a BSA, which includes HS; therefore, his goal is to reduce chance laws to qualities at spacetime points. He also doesn't want to give up the claim that his BSA is an objective theory: his theory of chance within the BSA is to remain objective in that it doesn't depend on our interpretation of things in the world. But is it possible to reduce chancy laws to a Humean base?

In order to incorporate chance into his BSA, Lewis goes on to reformulate his BSA so that it takes into account the fit of what the chancy laws say will happen with the actual course of history.[5] He needs to introduce fit, because different chance laws could produce much the same history, but the chances of those histories will be different under the

5. There is a potential problem here that I will only mention and will generally ignore. The probability of an event can be relativised to different reference classes, specified in terms of perfectly natural properties, to which that event belongs. These reference classes are presumably what chancy laws take into account about the world when they make claims about what the chances of an event are. If so, it is difficult to pin down the truth of a probability claim. The truth of two inconsistent probability claims is then determined in each case by there being a real state of affairs in the world: truth of such claims becomes relative. See Nozick, *Invariances* 2001, p. 17, and footnote 3 on that page.

different chance laws. (*E.g.*, in a coin flip experiment, a run of nine heads and one tail will fit a chance law that puts the probability of heads at 0.9 better than one that puts it at 0.5, but both laws could produce the same sequence.) According to Lewis, the new BSA (1994, p. 480)

> (1) is true in what it says about history;
> (2) never says that *A* without also saying that *A* never had any chance of not coming about;[6]
> (3) strikes a good balance between strength and simplicity for non-chancy events; and
> (4) amongst the systems that satisfy (1)-(3), will take account of the fit with the actual course of the total history including chancy events.

So the fit with the actual course of total history might be sacrificed to gain strength and simplicity of the law: that is, the new BSA does not imply that the laws give us the best fit with actual history. For example, consider a 1000-coin-toss-world where we get 495 heads and 505 tails. The BSA might say that the chance of getting heads is 50% because, although the fit is not perfect, it strikes a better balance between simplicity and fit. Lewis says, "suppose the frequency is close to some simple value—say, 50-50. Then the system that assigns uniform chances of 50% exactly gains in simplicity at not too much cost in fit. The decisive front-runner might therefore be a system that rounds off the actual frequency" (Lewis, 1994, p. 481).

As I've already mentioned in Chapter IV, Lewis claims that his account of the BSA is *not* an epistemological account. He writes,

6. The point here is that it would be unacceptable for the system to say *A* and $P(A) < 1$. If the system says *A*, then it is assumed that $P(A) = 1$. In essence, we don't want the part of the system that deals with the deterministic part of the world to conflict with the part that deals with the non-deterministic part.

> Despite appearances and the odd metaphor, this is not epistemology! You're welcome to spot an analogy, but I insist that I am not talking about how evidence determines what's reasonable to believe about laws and chances. Rather, I'm talking about how nature—the Humean arrangement of qualities—determines what's true about laws and chances. Whether there are any believers living in the lawful and chancy world has nothing to do with it. (1994, pp. 481-482)

As we will see, that Lewis believes the best system to be objectively determined is crucial to my argument against his formulation of the BSA.

3. Chance, Credence, Undermining, and the Principal Principle

Credence is the degree of belief one has that something will happen measured as a probability distribution. Credence is subjective and depends on our knowledge and our rationality, but usually the laws of how one should set her credence are relativised to an ideal rational agent. In a classic paper entitled "A Subjectivist's Guide to Objective Chance," Lewis (1980) has given an analysis of chance and its relation to credence, and it is useful to rehearse the difference here. Chance is the objective single-case probability of an event (or outcome). On the other hand, a rational credence function represents the degrees of belief of an ideally rational epistemic subject, whose beliefs would remain reasonable after conditionalizing on new admissible evidence as it came in. Both chance and credence are probability distributions, which means that they assign a number in the interval [0, 1] to any proposition that specifies an event. Both must satisfy all the standard mathematical axioms assigned to any probability distribution. Chance is objective in that, if there was no one to believe in the chance of an event, or if someone's total evidence came from misleadingly unrepresentative samples, chance wouldn't be affected in any way.

The standard mathematical axioms of probability theory *do not* tell us what the chances of chancy events are. Nor do they tell us what objective chance *is*. Lewis believes that the BSA of chance is the most robust determinant of chance, but he also believes that symmetries and frequencies sometimes determine chance and therefore they can be part of what the BSA takes into account.[7] To say, for example, that symmetries determine chance is, roughly, to say that, if there exists a set of possible future events that are perfectly symmetrical with respect to the current state of the world (*i.e.*, there is no sufficient reason why any one event would occur as opposed to any other event in the set), then the chance of any of the particular events occurring is obtained by dividing the chance up equally among the events of the set.[8] On the other hand, to say that frequencies determine chance is to say that the frequency of occurrence of actual events, in a set of actual events in the total history of the world that are similar with respect to some relevant features, determines the chances of the actual events.[9] It is simple to see how chancy laws formulated via symmetries or frequencies supervene on the spatiotemporal distribution of local qualities, that is, how they Humean supervene. It is also seems clear how Lewis's BSA is meant to supervene on the Humean base:

> ***HS-under-BSA:*** no changes in the laws without a *significant* corresponding change in the Humean base.

7. See Lewis 1994.
8. Thus, Buridan's ass, who finds himself in the situation of having two hay piles placed perfectly symmetrically with respect to his location, one to the left and one to the right, will have a 50% chance of going to the hay pile on his left and a 50% chance of going to the hay pile on the right.
9. The main issues with the frequency interpretation are (1) making sense of 'similar with respect to relevant features', (2) dealing with the possibility of infinite sets of events, and (3) dealing with sets containing single events.

The qualifying term 'significant' is now required because, for example, a 1000-coin-toss-world in which 450 tosses turn out heads might have the same laws under the BSA account of laws as another such world in which 550 tosses turn out heads: the *simplest* law might say the chances of heads is 50%, trading on exact fit with the actual occurrences. It might therefore take a significant change in the Humean base to trade in simplicity for fit.

So far the story goes quite smoothly, but then comes Lewis's *undermining* worry: according to HS, the spatiotemporal distribution of local qualities makes the world what it is, but the laws that describe chance might allow a future (an *undermining future*) that will make the chances different from what they actually are. If BSA was just an epistemological account, then such undermining would just be an undermining of our knowledge; but, given that Lewis insists that BSA is a metaphysical description of reality, this undermining is quite strange.

Note that the undermining futures problem can be formulated in terms of chance irrespective of how the chances are set; all we need is that the chances supervene: *if chance supervenes on the total history of the world, then alternative future histories might determine different present chances*. Undermining futures occur because a theory of chance that supervenes on the Humean base needs to reach out, so to speak, to total history to determine what the chances of present events are but then does not disallow alternative futures that are widely different from the actual future.

For example, suppose that at time *t* we have a chance law *L* that tells us the probability of how some things will go in the future. According to HS, *L* supervenes on the Humean base *H* (which includes the total actual history). But *L* allows, in fact specifies the chances for, different alternative undermining futures that significantly change the Humean base.

Suppose one of these undermining futures is *F*, and it changes the Humean base to *H**. According to *L*, *F* will have a non-zero present chance of occurring. But, if such a future occurs, then *L* will in fact not be a law. Therefore, according to *L* there is some present chance that a possible undermining future will occur that will make *L* not a law. As Lewis says, "The present chances *undermine* themselves!" (*ibid.*, p. 482).

My first reaction to this sort of undermining is that it leaves science no epistemic access to chance laws. Unless we know what the actual total history of the world is, we cannot know what the chance laws are. (This is similar to the worry of gaining epistemic access to properties that are defined as classes, which I will bring up in the next chapter: unless I observe all the things that are black I will not have observed the property blackness.) But Lewis's laws are meant only to describe, and not govern, so it is not surprising that his laws require the total history of the world before they can properly describe. One might object that, if the total history is required before the laws can be obtained, then what's the use of chance laws anyway? These awkward concerns don't seem to bother Lewis because, up to this point in the story, he is concerned only with formulating a consistent metaphysics, and Lewis's metaphysical story of a BSA analysis of chancy laws that supervene on a Humean base seems to be consistent.

But Lewis himself notices a problem when he attempts to connect his metaphysics with an epistemology of chancy events. Lewis says that he was at first willing to live with this peculiar type of undermining until he discovered that it leads to a contradiction when coupled with the Principal Principle (Lewis 1986, pp. *xiv* - *xvii*, 111-113). According to Lewis, one thing we know about chance is that we should set our credence according to the *Principal Principle,* which can be described as follows. Let *P* be the chance distribution for a given world w at a given time t. That is, it gives the objective chance of any proposition, say A, being true, and therefore must take into account the laws of the world. Then at that world and at that time, for any reasonable initial credence function C, and for any admissible evidence E that specifies the objective chance of A, the (old simplified version of the) Principal Principle states that

$$C(A/E) = P(A)\,. \tag{OP}$$

That is, the credence we should have that A is true given the evidence E is equal to the chance that A is true. Evidence, E, is admissible only if it does not contain any information about the future concerning how chancy future events will turn out. But note that Lewis says that the proposition E "specifies the present chance of A, in accordance with the function P" (1994, p. 483). So already we have a hint that maybe evidence that specifies what the objective chances are is inadmissible evidence—this is what Lewis himself will claim.

Lewis draws a contradiction using (OP) and undermining futures as follows. Take the proposition A in (OP) to be F, the proposition claiming that the future will be an alternative future history that undermines the present chance theory, and let E be the conjunction of present and past history, H, and the true lawful theory of the world T, *that includes what the present chances of F actually are* (given that the actual future is not F). Then

(1) F has some present (non-zero) chance of coming about, $P(F) \neq 0$, and this implies, using (OP), that $C(F/E) \neq 0$; but

(2) F is inconsistent with E and therefore $C(F/E) = 0$.

which is clearly a contradiction.

An undermining future changes the laws; and, although $P(F) \neq 0$, we shouldn't believe that the chance of that future, on the condition that the laws are what they actually are, is not zero, *i.e.*, that $C(F/E) \neq 0$. It was the use of (OP) that led to $C(F/E) \neq 0$, that is,

$$C(F/E) = P(F) \neq 0,$$

and therefore this must be a fallacious use of the Principal Principle. If it is a fallacious use of (OP), then the contradiction goes away, because the first step will be blocked. According to Lewis, that such a use of the old principle *is* fallacious is similar to the following fallacious uses of (OP):[10]

(a) *C*(the coin will fall heads/it's fair & will fall heads in 99/100 next tosses) = 1/2

(b) *C*(the coin will fall heads/it's fair & it will fall heads) = 1/2.

Our intuition on both cases tells use that our credence should be much closer to 1, after all, in (a) we are conditioning our credence on the fact that 99 of the next 100 tosses will be heads, and in (b) we are conditioning our credence on the fact that the next toss will be heads! So if (OP) tells us to set our credence to 1/2, it is not much use as a principle on which to base our credence. Lewis argues that these are fallacious uses of (OP), because *E* bears inadmissible information about future history.

But then Lewis goes on to correctly say that *any* application of (OP) is fallacious; it can never be applied! Any proposition that bears information about present chances thereby bears information about future history. Therefore, information about present chances is inadmissible in (OP). "But, this information is not inadmissible, as witness the way it

10. Lewis, 1994, p. 485. Note that the inadmissible information in these examples might not result in undermining futures, but the information on which our credence is conditioned is obviously information about the future.

figures in everyday reasoning about chance and credence!" (*ibid.*, p. 486). That is, we would accept it to be a perfectly rational claim to say that[11]

(c) C(the coin will fall heads/it's fair) = 1/2.

4. Lewis's Solution: the New Principal Principle

Lewis's solution begins by first weakening the admissibility constraint on evidence used in determining credence, because otherwise almost no evidence will be admissible. He does this in three ways. First, evidence *admits of degrees*: a proposition E might reveal so little about the future that it makes negligible impact on rational credence and therefore can be considered as almost admissible. Second, degrees of admissibility *are a relative matter*: E might be inadmissible with respect to A but admissible with respect to B. And, third, *near-admissibility might be good enough*: if E is nearly admissible with respect to A, then $C(A/E) = P(A)$ might be a good approximation. But evidence E will not be admissible when dealing with futures F that undermine the chances specified in E, because in that case $C(F/E) = 0$ but $P(F) \neq 0$, and therefore E reveals a lot about the future.

For cases when we want to condition our credence on evidence that is clearly not admissible, Lewis comes up with a modification to (OP) that he claims solves the problem of inadmissible evidence. Thus, he claims to achieve his goal of having a principled connection between (objective) chance and (subjective) credence. The *new* Principal Principle is formulated so as to *yield* that the *objective* chance of undermining futures for world w conditioned on a complete theory of chance for w, provided by the BSA and the

11. Note that we don't have to know that the coin is actually fair to make this claim.

total history of w, will turn out to be zero: undermining futures have zero chance of coming about! The new principle is stated as follows: at time t and at world w

$$C(A/HT) = P(A/T) \tag{NP}$$

where H gives the history of world w up to and including time t, and T is the complete theory of chance for world w (a proposition giving all the probabilistic laws, and therefore all the true history-to-chance conditionals, that hold at w). According to Lewis's BSA, our theory T will Humean supervene on the total history of world w. Thus, our credence in an undermining future, conditioned on evidence of a complete theory of chance T that would be undermined by such a future, will be zero. Lewis has removed the contradiction as well as the peculiar result that we should put any credence in undermining futures. At first, this result seems appealing, but I'll now show that (NP) leads to philosophical ramifications that go against the HS thesis.

The new principle has a different right-hand side from (OP), and Lewis claims that in general $P(A/T) \neq P(A)$, because $P(T) \neq 1$. If T is certain, then $P(A/T) = P(A)$ for any proposition A, and therefore (NP) is the same as (OP). So Lewis requires that T be an uncertain theory so that, on the right hand side of (NP), the chance of an undermining future, F, conditioned on T being the true theory of the world, will turn out to be zero. For otherwise, if $P(T) = 1$, then $P(F/T) = P(F)$ by definition, and the theory T says that $P(F)$ is not zero (T allows undermining futures). But recall what T is supposed to represent. According to Lewis, T is supposed to be our total true theory: a proposition giving all the probabilistic laws, and therefore all the true history-to-chance conditionals, that hold at w, that Humean supervenes and is obtained via Lewis's BSA. Therefore, Lewis is now claiming that *T's metaphysical* chance of being the theory of the world is not equal to one!

Such a result means that claims such as the following are meaningful: In *this* infinite-coin-toss world, in which a coin is tossed an infinite number of times, there is a probability of 0.9 (*i.e.*, of chance theory T_1) that the chance of getting heads is 0.5, and a probability of 0.1 (*i.e.*, of chance theory T_2) that the chance of getting heads is 0.25. But then the question is, what determines T_1 and T_2? The Humean must say that the pattern of coin toss results (for all time) determines these two possible theories. But then why does BSA report the chances in this way instead of just reporting the chances of getting heads as simply $0.9 \times 0.5 + 0.1 \times 0.25 = 0.475$? There doesn't seem to be any reason that Lewis's BSA, which supervenes on the pattern of the mosaic events in the world, should allow more than one resulting theory; it seems that BSA should provide a single theory of the chancy events in the world. This argument is easily extended to several theories, or even a continuum many of theories: a new single theory T with $P(T) = 1$ can always be constructed.

Does this objection require BSA or can it be put in a way that appeals only to HS? It doesn't seem to matter what delivers the chances of the individual theories, all that matters is that the two theories supervene on the Humean base and that we allow undermining futures. According to HS, the Humean *must* say that not only do T_1 and T_2 Humean supervene, but the fact that they themselves each have only a chance of obtaining, also Humean supervenes. The new single theory is constructed using only the laws of probability and has nothing to do with BSA. Therefore, I think it is inconsistent for Lewis to accept such an analysis, given that he also holds the HS (and BSA) thesis.

Carl Hoefer has objected to the tenability of Humean supervening laws that are themselves taken to be chancy because he can't see what would deliver the chances for those laws. He says,

> should a Humean subscribing to BSA say that the events in history follow the course they do, and hence the (non-chance) laws of a world are what they are, as a matter of *physical necessity*? Of course not; the laws are what they are because of the pattern of events in history, and are not what they are "by law". This is just a restatement of the core idea of Humean analyses of law. For just the same reasons, the chances are not what they are "by chance", and the quantity $P_{tw}(\mathbf{T}_w)$ should be regarded by a Humean as an amusing bit of nonsense. (1997, p. 328)[12]

But the Humean would just claim that the chances of the laws also supervene, so I don't think that Hoefer's objection is the same as mine.

Hoefer offers an analysis of the undermining problem based on what he claims to be our illegitimate application of BSA to the world as a whole. He says that

> whenever we are dealing with chance-event sequences so grand in scale that they have the power to undermine the actual chances if they went some way or ways, then we are dealing with a set of events to which the concept of *Humean* objective chance does not properly apply. Neither PP nor OP should be used to govern our credences at this level; they are rationally determined, if at all, by considerations independent of the "objective chance" of such events. (*ibid*., p. 330)

For Hoefer, we should be asking for the objective chances of propositions describing only a limited part of the world. For example, we should be asking questions like, what are the chances of this particular electron travelling through this particular slit in this particular experiment? What the credence is of complete undermining futures is not a question that the Humean should be asking. The original Principal Principle will work in cases with limited domains, and there is no need to modify it. So Hoefer thinks that we should accept

12. Hoefer is using the more precise subscript notation for the specialized case of denoting objective chance at world *w* and at time *t*. Because theories are relative to worlds, he uses subscripts to denote exactly which world the theory *T* is supposed to be a theory of (bold fonts in Hoefer's notation indicate no difference in meaning from the italicized notation I'm using).

(OP) as the proper link between subjective credence and objective chance but that we should limit our use of the principle to propositions of limited scope, propositions that don't undermine the theory of chance.

The obvious concern here is how we could ever know that a proposition stating how the future might go undermines the theory we take to describe the chances of that future. The future of the actual world is unknown at the time that one would like to apply (OP), so it would be difficult to tell whether the occurrence of some possible future undermines a theory of chance I'm currently entertaining as my best theory (or even merely an arbitrary theory).

Hoefer's vaguely described solution is similar in certain respects to Lewis's claim that we can use (OP) when our evidence is admissible (whereas Hoefer focuses on future-describing propositions being *appropriate*, Lewis describes the evidence as being *admissible*). Lewis isn't quite as vague as Hoefer: as part of the new Principal Principle, Lewis defines an admissibility quotient that tells us to what degree the evidence is admissible and whether we are allowed to use the old principle. Lewis defines the admissibility quotient of T with respect to A as

$$Q_{T/A} =_{\mathrm{df}} \frac{P(A/T)}{P(A)}.$$

Obviously, the closer $Q_{T/A}$ is to one, the closer the result of using (OP) to that of using (NP) for a proposition A.[13] This happens iff A and T are independent, that is, iff

$$P(AT) = P(A)P(T).$$

13. Lewis considers (OP) an approximation to (NP), with how good the approximation is being based on how admissible the evidence is.

If, on the other hand, A contradicts T (*i.e.*, A is an undermining future), then $Q_{T/A} = 0$, and according to (NP) we get $C(A/HT) = P(A/T) = 0$. In this case, (OP) is as different as it could be from (NP). This is the result that Lewis wants.

But notice that $Q_{T/A} = 1$ on its own does not imply $P(T) = 1$. To show this, we can write out the chance of T explicitly in terms of the admissibility quotient:

$$P(T) = \frac{P(AT)}{P(A/T)} = \frac{P(AT)}{Q_{T/A}P(A)}$$

and we see that, for an admissibility quotient of one, all we require is that[14]

$$P(T) = \frac{P(AT)}{P(A)} = P(T/A).$$

Therefore, that $Q_{T/A} = 1$ does *not* imply that $P(T) = 1$ unless $P(T/A) = 1$ and that is not part of what $Q_{T/A} = 1$ gives. Lewis claims to have a "proof" (*ibid.,* p.487) that, if T is perfectly admissible with respect to A, then, because (OP) can be used, $P(T) = 1$. He claims that plugging in T for A in (OP) gives $C(T/HT) = 1$, and therefore $P(T) = 1$. But this is clearly a piece of circular reasoning, because the only way that we can be justified in plugging in T for A in (OP), with T as evidence, is the admissibility of T with respect to T is equal to one, that is, only if $Q_{T/T} = 1$. But $Q_{T/T} =_{\mathrm{df}} P(T/T)/P(T)$ is one only if $P(T) = 1$. So, after all that, it is clear that perfect admissibility will not ensure that the chances of our chance theory is one. But looking back, it would be surprising to find that it did! For admissibility is admissibility of evidence with respect to a chance proposition, and the same evidence might be admissible with respect to one proposition and not admissible with respect to another proposition.

14. This just shows the symmetry of independence property between two random events. If two random events, X and Y, are independent, then $P(X) = P(X/Y)$ and $P(Y) = P(Y/X)$.

I conclude that neither Hoefer's nor Lewis's solutions is appealing: Hoefer restricts the use of the old principle to *appropriate* propositions within a restricted domain, and Lewis restricts the use of the old principle to *admissible* evidence. Hoefer accepts that our chance theory should not be itself chancy but leaves it a mystery about how we could ever know which propositions are appropriate. Lewis defines admissibility, but admissibility does not guarantee that our chance theory will not be itself chancy. And I've argued that chancy chance theories cannot Humean supervene.

5. Other Ways to Get Around the Contradiction

A. Vranas's solution: HS contingent and OP applied to non-HS worlds

Lewis's formulation of (NP) is an attempt to block the first part of the *reductio,* that, because an undermining future F has some present (non-zero) chance of coming about, *i.e.* $P(F) \neq 0$, then, according to (OP), $C(F/E) \neq 0$. He would rather accept the second part of the *reductio*, that F and E are inconsistent and therefore $C(F/E) = 0$. The new Principal Principle is formulated so as to yield this result, but, as I've argued, it also leads to the unappealing philosophical position that law theories themselves are chancy.

Suppose one tries to fix the problem by arguing that the second part of the *reductio*, that $C(F/E) = 0$, is not true? Peter Vranas has taken such a stand and has based his argument on the assumption, also made by Lewis, that HS is contingent. His position with respect to the reductio can be described as follows. First suppose that an undermining future F has an (objective) chance $P_p(F)$ of occurring in some possible world (calculated according to the laws of world p, not our actual world, because if F occurs in p, then the laws at p will be different from our laws) and that $P_a(F)$ is the chance of F in our actual world (calculated according to the actual laws at world a). These will be different: $P_a(F) \neq P_p(F)$. The probability of F in the actual world must be taken in conjunction with $E_a = H_a T_a$, the conjunction of the present and past history and lawful theory T_a of the *actual* world. The contradiction required for the second part of the reductio results as follows: (i) the conjunction $E_a F = H_a T_a F$ determines $P_a(F)$, because of the $H_a T_a$ part of the conjunction, and (ii) it determines $P_p(F)$, because of the $H_a F$ part of the conjunction, so $E_a F$ is impossible. Therefore, the left hand side of (OP) is zero, because our credence in F given $H_a T_a$ is given by our credence in $H_a T_a F$ divided by our credence in $H_a T_a$; and, our credence in $H_a T_a F$ should be zero, because it is a contradictory proposition. This is just Lewis's argument, but then Vranas claims that the inconsistency arises only because we're inherently assuming that $P_a(F)$ and $P_p(F)$ are both calculated for our world, and that can be resisted. He rejects that this is an inconsistency and therefore rejects that there is any need to revise the old principle, because rejecting the inconsistency blocks the second part of the contradiction. He writes

> Lewis defines HS as a thesis of *restricted* supervenience: it's not any two possible worlds whatsoever, but rather any two *among worlds like ours*, that don't differ without differing in the arrangement of local properties. What counts as a world like ours doesn't matter here; what matters is that, given Lewis's definition of HS, a total arrangement of local properties 'determines' (if HS is true) a configuration of chances only among worlds like ours. But then the conjunction of [E_a and] F in the last paragraph need not be *impossible*, false in *every* possible world; therefore, given [E_a], one's conditional credence in F need not be zero, and no contradiction arises. (2002, p. 153)

To make his argument acceptable to us, Vranas has to make us accept that $E_a F$ can imply two things depending on which world we're applying it to. If we're applying it to the actual world, then by (i) it implies that $P_a(F) = 0$ and no contradiction arises; the implication associated with (ii) need not worry us. Thus $H_a T_a F$ is a contradiction according to Humean

Supervenience only if we are considering worlds like ours, where HS obtains. It is possible that there are non-HS worlds having total history H_aF *and* laws given by T_a. So Vranas claims that, because HS is contingent, when we are considering worlds where H_aF is true, it is okay to use our actual laws to calculate the probability of F as $P_a(F)$ and not think that we should calculate it as $P_p(F)$. It is just that we are considering worlds that are very much different than ours, where HS does not hold: H_aF is true but the laws are the same.

I don't think that Vranas's way of getting rid of the contradiction works, because when we are using the Principal Principle it is exactly worlds like ours that we are interested in. Lewis believes that our world is a world in which HS obtains and that the Principal Principle is the fundamental way we have of connecting subjective credence to objective chance in *our* world. Vranas would have us say that were it the case that an undermining future occurs, then it would be the case that our world is not an HS world. But how can the occurrence of an undermining future in our world, which is allowed by our physical laws, make our world change from an HS world to a non-HS world? I claim that it cannot and that Vranas's solution is therefore not tenable. It is true that Lewis takes HS to be a contingent thesis about our world, but it seems that it should take more than a simple undermining future to put us in a world where HS does not obtain.

B. Roberts's solution: condition on available evidence

Recently, John Roberts (2001) has argued that the contradiction can be blocked without appealing to the contingent status of HS. (Roberts wants to save (OP) without using or imposing the condition that HS is contingent, because he thinks that HS is a necessary truth.) Roberts's argument can be summarized as saying that the simplified form of (OP), which we've been using and which Lewis uses to derive the contradiction, never applies in practice. Roberts argues that only the General Principal Principle (GP) will ever apply and that no contradiction can be derived from it. I will summarize Roberts's argument in my own way.

The general form of the principle was stated by Lewis himself (Lewis, 1980, p. 87)[15] as

$$C(A/E) = \sum_x C(X_x/E)C(A/X_xE) = \sum_x C(X_x/E)x \tag{GP}$$

where X_x, for any value of x, is the proposition that the chance at time t of proposition A equals x. As before, E is any evidential proposition compatible with X that is admissible at time t; it is the evidence upon which we are to base our credence and will include what we know of the laws. (To get the right-hand side, (OP) is used on each term in the summation: $C(A/X_xE) = x$.)

We see that (GP) never specifies a single X, a single proposition stating the single objective chance of A, but rather a range of propositions, X_x. This might result because we don't know the exact history to time t of world w, H_{tw}, or we don't know the exact form of the laws. According to Roberts, real evidence *never* specifies a single chance but rather probabilifies contingent information about the future. He states that no evidence available to creatures like us could ever, even in principle, specify or entail such information. If we restrict (GP) to apply only to cases where the evidence E is evidence that it is possible for finite empirical cognizers to obtain, then the contradiction derived via the conjunction of HS, undermining futures, and (OP), is blocked. There will be no way to derive the second part of the contradiction, that $C(F/E) = 0$. Even if an undermining future F exists that

15. Page numbers quoted for Lewis 1980 refer to pages in Lewis 1986.

makes one of the $C(X_x/E) = 0$, the right-hand side of GP will not go to zero because the other terms in the summation will not go to zero. Roberts goes on to consider whether there is another way to make the contradiction go through using (GP) and shows that it is reasonable to think that another way doesn't exist.

I tend to agree with Roberts that in usual cases evidence does not give us a single chance for contingent futures but I don't think that Lewis is worried about usual cases. Why should it be impossible for us to have sufficient evidence in some limited domain of interest? If we take the coin-toss example offered by Lewis, or some similar simple set of events, I see no reason to believe that cognizers like us shouldn't be able to know enough in principle about the laws governing such an event to make the *reductio* go through. Recall that according to BSA the fit doesn't have to be exact: the proposition specifying the probability of the outcome of an event might specify only an interval. Why should it be impossible for us to know that the next 1000 tosses of a coin will come out heads with probability in the interval $[0.5 + \delta, 0.5 - \delta]$? I don't agree with Roberts that the simplified form of (OP) never arises in practice.

In any case, it is not clear that the different range of propositions, X_x, could not be combined into a single proposition $\bar{X}$ that itself is part of what BSA gives. Here I'm giving a similar objection to the objection I gave to the new Principal Principle allowing theories of chance that are themselves chancy. There is no reason to believe that BSA would provide a series of chance theories each having only a chance of obtaining. For Lewis, I claimed that allowing such theories would be un-Humean. That part of my objection does not hold for Roberts, who wants to allow these chancy chance theories because of our lack of knowledge and not as a metaphysical fact about the world.

6. Chance does not Humean Supervene

The conclusion I come to is that chance does not Humean supervene. Some form of non-Humean-Supervenience about chance is required to make sense of laws that describe chance in the world; and, once non-Humean-Supervenience is introduced into the BSA account, there is no reason to not introduce it for the deterministic laws. I turn now to such accounts of the laws of nature.

7. Appendix: Example of the Use of the New Principal Principle

To clarify the use of the new Principal Principle, I now consider and extend Lewis's own example to show how the chance of a theory might be calculated. Suppose that so far 2/3 of *J*s have been *K*s, but there are 10,002 more *J*s still to come. The complete theory of chance, T, says $P(K) = 2/3$. Frequentism says T holds iff the overall frequency of K is 2/3.[16] Since the frequency of past *K*s has already been 2/3, T holds if the future frequency is also 2/3. Let A be the proposition that three out of the next four *J*s will be *K*s. What is the admissibility quotient of T with respect to A? This can be calculated as

$$Q_{T/A} = \frac{P(A/T)}{P(A)} = \frac{P(AT)}{P(A)P(T)}.$$

Now $P(AT)$ is the probability of the proposition that the next three out of four *J*s are *K*s in conjunction with the proposition that T says that $P(K) = 2/3$, that is, that the next 6665 out of 9998 *J*s are *K*s. Because all the events are independent events, we can calculate this as

$$P(AT) = P(A)\left(\frac{2}{3}\right)^{6665}\left(\frac{1}{3}\right)^{3333}\left(\frac{9998!}{6665!3333!}\right).$$

16. Note that Lewis is going with frequentism instead of BSA in order to simplify the example. BSA allows a slightly deficient fit in order to strike the best balance with simplicity and strength.

In the denominator, we have $P(A)P(T)$, the probability that the next three out of four *J*s are *K*s multiplied by the probability that the next 6668 out of 10,002 *J*s are *K*s. This is calculated as

$$P(A)P(T) = P(A)\left(\frac{2}{3}\right)^{6668}\left(\frac{1}{3}\right)^{3334}\left(\frac{10002!}{6668!3334!}\right).$$

Putting this all together we get[17]

$$Q_{T/A} = \frac{P(A/T)}{P(A)} = \frac{P(AT)}{P(A)P(T)} = \frac{P(A)\left(\frac{2}{3}\right)^{6665}\left(\frac{1}{3}\right)^{3333}\left(\frac{9998!}{6665!3333!}\right)}{P(A)\left(\frac{2}{3}\right)^{6668}\left(\frac{1}{3}\right)^{3334}\left(\frac{10002!}{6668!3334!}\right)} = 1.00015$$

which says that T is quite admissible with respect to the proposition A. But that admissibility of a theory doesn't imply that the probability of the theory is one can be seen if we continue with Lewis's example and calculate the chance of theory T:

$$\begin{aligned} P(T) &= \left(\frac{2}{3}\right)^{6668}\left(\frac{1}{3}\right)^{3334}\left(\frac{10002!}{6668!3334!}\right) = \left(\frac{4}{27}\right)^{3334}\left(\frac{10002 \times 10001 \times \ldots \times 6669}{3334 \times 3333 \times \ldots \times 1}\right) \\ &= \frac{10002(4) \times 10001(4) \times \ldots \times 6669(4)}{3334(27) \times 3333(27) \times \ldots \times 1(27)} = 0.0085 \end{aligned}$$

This shows that T is a very uncertain theory.

Because Lewis is allowing us to put an objective chance on our theory, our theory is correct only if during the next 10,002 observations, 6,668 of the *J*s are *K*s and 3,334 are not. The number of possible futures in which this could happen is the astronomically large number $10002!/(6668!3334!)$ but, unfortunately, the probability of any of these futures

17. This is calculated by Lewis (1994) in a footnote on p. 488, where he also states that $P(A) = (2/3)^4 = 0.19753$, but I calculate it as $P(A) = 4(1/3)(2/3)^3 = 0.043896$. This is not a serious error, because $P(A)$ cancels out from the above expression.

happening is equal to the microscopically tiny number $(2/3)^{6668}(1/3)^{3334}$. The product of these two numbers is equal to the chance of T and turns out to be only 0.0085.

Are there other theories which take up the slack in the probability, theories that the BSA or frequentism delivers for the other possible futures? Note that $P(T)$ isn't the probability of the theory associated with a *single* actual future; theory T may fit many possible futures with acceptable accuracy. In fact it fits $10002!/(6668!3334!)$ futures exactly, but, to account for simplicity, BSA might allow T to fit, less precisely of course, many other futures: one might allow that the proportion of Ks to be in some interval $[2/3-\delta, 2/3+\delta]$. I doubt this will raise the chance of T much, but the philosophical implications are the same.

One queer result of HS and BSA, which this example shows explicitly, should be noticed. To calculate $P(T)$, the results of T itself were used! That is, $P(T)$ was calculated by assuming that $P(K) = 2/3$. It is not a simple matter to justify to oneself that this is the right thing to do, but what options does one have? None that I can think of under the HS and BSA theses.

V — Nomic Entities: Universals with Modal Character?

I regard Actualism as the most difficult and uncertain of my three assumptions. It is bound up with the difficult question whether the laws of nature involve logical necessities in things: whether there is *de re* logical necessity involved in laws. For dispositions and powers, if they are conceived of as the non-Actualist conceives them, involve logical or quasi-logical connections in the world between dispositions and powers, on the one hand, and their actualization on the other.
— David M. Armstrong (1926 -)
(from 1983, p. 9)

The failure of regularity analyses of laws of nature comes down to the fact that the accepted ontology consists of mere static facts and sequences of those facts, and nothing is posited that makes those sequences happen. As I discussed in Chapter III, fundamental science accepts all sorts of mechanisms for making such sequences happen, but sophisticated regularity analyses will have us believe that these mechanisms supervene on the static facts and that they are merely ways of describing the sequences that do happen in the most simple, informative, and fitting way possible. Clearly, the regularity theorist's ontology is too sparse for the realist who wants nomic posits that explain and govern the things in the world. But what kind of thing is a law of nature then? What kind of thing might constrain things to happen in the world, support counterfactuals, and ground explanation?

In this chapter I consider whether higher-order universals can sufficiently and consistently perform the metaphysical role required of laws of nature. To motivate laws of nature as universals, I first consider the Humean no-epistemic-access argument against the existence of mysterious nomic entities and for the reduction of laws of nature to the Humean base. I claim that this argument is unconvincing, because (a) anyone who reduces laws to regularities cannot treat properties nominalistically (reduce them to classes of things in the Humean base), because, if a regularity theorist about laws remains a class nominalist about properties, then she leaves it mysterious just how we can ever come to know that there are regularities in the first place, and (b) anti-nominalism about properties is not much different than realism about nomic entities; and, if one has to make the inference to real non-reductive properties, she might as well make the inference to primitive nomic entities.

I then go on to consider Armstrong's contingent relation among universals account of laws of nature. I follow that by Bird's recent so-called ultimate argument against the view that laws of nature are universals that don't have modal character but that entail the corresponding regularities. I agree with Bird that it is not consistent to hold the view that universals have no modal character and that laws of nature entail the corresponding regularity. It is also not reasonable to hold the position that universals as laws of nature do not have a modal character, because it is a natural part of the scientific attitude that laws of nature do confer some sort of natural necessity in the world. Finally, I briefly consider whether this requires the positing of powers in the world and leave it as an open question whether it does.

1. Justifying the Positing of the Nomic

A. Hume's epistemological point: no access to natural necessity

Hume argues that, although one might observe events of one type (causes) regularly followed by events of some other type (effects), one never *observes* the glue that binds the causes to their effects. Because the distinct events (causes and effects) are all that one ever observes, from an empiricist point of view one cannot infer more than that the events are regularly conjoined; any ascription of some mysterious necessary coupling between the events, say, by positing a law of nature or a mystical force (or any other primitive nomic feature in the world), is a psychological artifact of the way our minds represent the conjoined events.

My main objection to Hume's epistemological point is that I don't think that it can be formulated consistently without reducing one's epistemology to a deep scepticism, because the same epistemological point can be raised against our knowledge of properties. The objection can be formulated by first considering the question of what exactly it is supposed to mean to say that one *observes* an event or regularities of conjoined distinct events. It is crucial to Hume's point that what distinguishes the distinct events from the mysterious glue of necessity is that the former can be observed (and this is evidence that they are real entities) while the latter cannot be observed (and therefore there is no evidence that it is a real entity). Now, it's a simple matter to convince oneself that what we actually observe are *properties*[1] associated with an event and never any (bare) particular event itself. (Whether or not a bare particular event even exists does not concern us here, because Hume's point

1. Here I'm assuming only a pretheoretic understanding of what a property is and will look at what Hume's methodology will have to say about properties. A property is something that "clothes" a bare particular; certainly scientists speak about things *having* properties such as mass.

is epistemological.[2]) But, even if this is not accepted, *i.e.*, if we assume that we do have some experience of the bare particular event, it is still properties that make regularities, for, when I said above what the evidence for a regularity is supposed to be, I said that it is to "observe events of one type (causes) regularly followed by events of some other type (effects)," and I claim that no sense can be made of this sentence when the phrases "of one type" and "of some other type" are removed. If we could make sense of such a sentence, then our evidence for regularities would be that we observe events regularly followed by events, and this is clearly not what we mean when we say that we observe regularities, for in that case every arbitrary sequence of events would be evidence for a regularity. So it must be that, when we observe regularities, among the things we observe are properties and these allow us to type events (a regularity requires at least regularly conjoined sets of properties).

Once this point is accepted, and I don't see any way in which it can be denied, the nature of properties has to be decided. Three options have been considered by philosophers regarding the analysis of properties: class nominalism, universals, or tropes. Suppose that a regularity theorist is also a class nominalist about properties.[3] Then when she observes a regularity, as a regularity theorist she will be claiming to observe a really existing regularity in the world, but, as I've just argued, she will therefore be claiming to observe properties. But, as a class nominalist, she must claim that properties are not wholly present at the location where she claims to observe her regularity. Class nominalism about properties

2. Metaphysically, the two options are to posit a bare particular that instantiates the properties or just claim that what really exists is a bundle of properties. Neither option affects what I say in the text.

3. Different theories of class nominalism have been formulated that provide the natural classes used in science: those positing natural classes as primitive and those positing a primitive objective resemblance relation among things. Lewis (1983, footnote on p. 348) gives what I take to be a good argument for the claim that these two theories are really a single theory presented in two different ways. My arguments apply to either form of class nominalism.

requires, for example, that the property of being black is just the class of all the black things. Similarly, to any other class there corresponds a property. But I certainly do not observe the class of all black things when I observe one, or even a thousand, black things.[4] So one has absolutely no epistemic access to the class nominalist's properties, because properties are identified with classes of things and one usually never has epistemic access to all the things making up a property class. But it seems that I ought to have epistemic access to properties in order to justify a claim of access to any regularity.[5] So it seems that the regularity theorist about laws of nature who is also a class nominalist about properties has an epistemic access problem similar to the Humean problem of the lack of epistemic access to real natural necessities in the world. She lacks epistemic access to regularities, which are implicated in properties.

A response to this problem of access to properties as classes is to say that, when one observes a thing having certain properties, those properties, being classes of things, are acting non-locally. To observe something is to have it act causally on the observer, but for the class-nominalist the properties are acting in an *irrelevantly non-local* way. It seems intuitively clear that the only things that are metaphysically relevant, when someone observes a thing having a property, are the observer and the thing having a property. But, for the class-nominalist, when I see a black thing, *all* black things are entering into a non-local causal relation to me.

4. Not to mention the other-worldly things that are necessary to make sense of properties that are co-extensional in this world alone. My argument against class nominalism here is much the same as "The Causal Argument" that Armstrong uses in *Universals: An Opinionated Introduction* (1989, Chapter 2, Section VI, and Chapter 3. Section VI).

5. Notice that this is not the same problem as that of *identifying* the natural properties that are needed to formulate laws of nature. See Lewis's "New Work for a Theory of Universals" (1983).

Now non-locality with respect to the laws of nature is essentially the bullet that Lewis claims to bite; after considering Armstrong's view that laws of nature are second-order universals, he writes,

> For Armstrong, the lawful necessitation of *Ga* by *Fa* is a purely local matter: it involves only *a*, the universals *F* and *G* that are present in *a*, and the second-order lawmaking universal that is present in turn in (or between) these two universals. If we replace the universals by properties, however natural, that locality is lost. For properties are classes with their membership spread around the worlds, and are not wholly present in *a*. But I do not think this a conclusive objection, for our intuitions of locality often seem to lead us astray. The selective regularity theory I shall shortly advocate also sacrifices locality, as does any regularity theory of law. (1983, p. 366)[6]

Now it might be acceptable to think that laws of nature are somehow non-local phenomena, but when laws of nature are appealed to in the definition of causation, causation also becomes a non-local phenomenon. It doesn't seem possible to formulate a theory that encompasses all causation without involving laws of nature. Lewis states his belief that the two concepts are intimately connected as follows:

> It is fairly uncontroversial that causation involves laws. That is so according to both of the leading theories of causation: the deductive-nomological analysis, on which the laws are applied to the actual course of events with the cause and effect present; and the counterfactual analysis that I favour, on which the laws are applied to counterfactual situations with the cause hypothesised away. (*ibid*., p. 368)

The important thing here is that the regularity theorist who believes that causation is intimately connected with laws of nature will have to accept an irrelevantly non-local account of both.

This seems hard to accept, but possibly it is counter-intuitive only to those who already have an intuition for the reality of primitive laws of nature and causal forces. For Lewis, laws of nature are merely descriptions of the happenings of our world, so that they are extremely

6. In this paper, Lewis uses 'properties' to refer to the class nominalist's properties, *i.e.*, classes of things.

non-local might not be so counter-intuitive for him.[7] On the other hand, for someone who is already hesitant to accept the reduction of laws of nature to regularities, the result that the laws of nature and causation become extremely non-local on such a reduction might be enough for him to reject such an account.

So far, I've argued that a regularity theorist about laws who is a class nominalist about properties will probably have to accept an irrelevantly non-local account of laws of nature and causation.[8] The alternative seems to be to accept that similar things *instantiate* real properties that are either identical or resemble each other across particulars. This leads to properties being either universals or tropes.

Whether the regularity theorist about laws of nature accepts universals or tropes into her ontology, it seems that she is accepting something just as mysterious as the glue of necessity that she shuns. To accept universals is to accept repeatable entities that are wholly present wherever they are instantiated by particulars and that have some causal efficacy.[9] What I have argued is that to posit a regularity from our observations is at least to make an inference to real properties *in* particulars. Now it seems to me that one incurs no more metaphysical risk, above and beyond the metaphysical risk associated with using our observations to infer real properties in particulars, to use regularly conjoined real properties to infer that a *real connection* exists between the particulars involved in a regularity. The unpacking of this real connection might not necessarily lead to a primitivist

7. The counterfactual view of causation that Lewis accepts also does not posit any real causal forces.

8. I haven't considered the possibility that one might want to be a class nominalist about properties and a realist with respect to laws of nature or causation, because my main concern is with the reduction of laws to regularities.

9. To observe the blackness of a particular is to be affected by the universal instantiated by that particular. Though the universal is somehow spread across all spacetime, it is wholly present in each particular that instantiates it and, therefore, to be affected causally by a universal is to be affected locally.

account of laws of nature, but it is interesting to note that those who construe laws of nature as either universals or tropes do not posit a new type of thing. I now turn to the possibility that laws of nature are higher-order universals.

2. Laws of Nature as Universals

A. Laws of nature as contingent relations among universals

This account of laws of nature, the CRU account for short, is usually attributed to three philosophers who came up with it independently: Dretske (1977), Tooley (1977), and Armstrong (1983).[10] Following Armstrong, $N(F, G)$ is a singular statement of *nomological necessity* relating the universals F and G, where 'N' denotes the relation. To unpack this theory, we have to be clear on what universals are; not every one thinks of universals in the same way. I will first briefly review the fundamental features of Armstrong's conception of universals before moving on to the CRU account of laws of nature.

One unique aspect of Armstrong's account of universals is his rejection of transcendent (or Platonic) realism: universals are immanent (or Aristotelian) in that they are wholly present wherever and whenever they are instantiated, and they must be instantiated at some time throughout the history of the world. So, according to Armstrong, there are no universals that are nowhere or nowhen instantiated. In addition, his theory calls for a sparse set of universals established by science, *a posteriori*, that correspond to perfectly natural properties. The particular that instantiates universals is not a thing that can found alone. What we find in the world are *states of affairs*: clusters of universals *bound* together with the

10. In the literature, this account is sometimes called the DTA account of laws of nature. One major difference between Tooley and Armstrong is that Tooley does, and Armstrong does not, allow uninstantiated universals. I will be following Armstrong's account for the most part.

particularity, the *thisness* of a thing that makes it unique. So consider a particular electron e: its having the properties of negative charge and the determinate mass of 9.11×10^{-31} kg is a state of affairs. When we consider the electron with the cluster of universals it instantiates, we are considering the *thick* particular, whereas the *thin* particular is what is left after all the universals have been abstracted out: *abstracted* to emphasize that the union is not mereological.

B. The identification and inference problems

Two problems for this account have been named the *identification problem* and the *inference problem* by van Fraassen. The identification problem is the problem of identifying the relation of nomological necessity, N. The inference problem is the problem of what justifies the inference from $N(F, G)$ to the universal statement pertaining to particulars $\forall x(Fx \rightarrow Gx)$. According to van Fraassen, the two problems play against each other: if one simply reduces $N(F, G)$ to $\forall x(Fx \rightarrow Gx)$, then the inference problem goes away but identifying laws of nature amongst all the accidental generalizations becomes difficult and unfounded. On the other hand, if the universal generalization is not contained in the concept of the nomological necessity relation, then it is difficult to see how the inference from the law of nature to the universal generalization can be made.

Armstrong deals with the two problems by identifying the relation of nomological necessity as a *causal* relation between types and not tokens. Starting from our intuition about singular causation between tokens, Armstrong argues that it is natural to generalize:

> At this point, despite postulating universals, we have nothing to offer as a law except regularities of pattern involving singular causation. But now the question arises whether the regular succession—this sort of cause bringing about this sort of effect—cannot itself be explained. We have the bunch of singular causations, the

> same sort of cause bringing about the same sort of effect. May we not seek to explain this? May we not hypothesize that this uniformity holds *because* something's being F *brings it about* that that same something becomes G? This latter is not a 'general fact', one expressed by a universally quantified proposition. Rather it is supposed to be an 'atomic fact', albeit a higher-order fact, a relation between the universals F and G. It is at this point that, I claim, the Identification problem has been solved. The required relation is the causal relation, the very same relation that is actually experienced in the experience of singular causal relations, now hypothesized to relate types not tokens. There is of course no question of *proof* that this hypothesis is true. It is rather a postulation that recommends itself because of its explanatory power. (1993, p. 422)

If we accept this identification, then Armstrong claims that

> the Inference problem is solved. For if a certain type of state of affairs has certain causal effects, how can it not be that the tokens of this type cause tokens of that type of effect? The inference is analytic or conceptual. (*ibid.*)

The inference that Armstrong is talking about is the inference from $N(F, G)$ to $\forall x(Fx \rightarrow Gx)$, so if he literally means that it is analytic or conceptual, then he must mean that the inference is entailment: $N(F, G) \vDash \forall x(Fx \rightarrow Gx)$, that is, in any world where $N(F, G)$ is true, $\forall x(Fx \rightarrow Gx)$ is true. This is what Armstrong means by saying that N is a relation of nomological *necessity*, but he wants N to hold contingently. What he means by that is that it is possible that N not hold in all worlds; in some worlds F and G are so related, and in other worlds they are not.

Armstrong's reply has not convinced those philosophers that do not share his belief in universals, but those are not who I want to defend his views against. I want to consider Armstrong's views in light of Alexander Bird's so-called ultimate argument against the CRU account of laws of nature (2005). Bird's motivation is to advance his own dispositional essentialist view of laws of nature, so he objects to Armstrong's holding that all properties are categorical (non-dispositional) and claims that Armstrong can't hold that view in conjunction with the CRU account of laws of nature.

C. Bird's ultimate argument against CRU account of laws

Bird starts his argument by setting out two views about properties and laws of nature that Armstrong holds. The first view, he names

> PROPERTIES: Natural properties are categorical in the following sense: they have no essential or other nontrivial modal character. (2005, p. 147)

By 'nontrivial' Bird wants to rule out, for example, that the true trivial proposition that everything is brown or is not brown says anything about the nontrivial modal character of the property brown. The other thing to keep in mind is that Armstrong takes properties to be universals, and so PROPERTIES is something that he wants to apply to all universals. This is where the problem shows up, because Armstrong also claims that laws of nature are higher-order universals and so, he also wants to hold

> LAWS: Laws of nature are contingent relations among properties [and laws of nature *entail* the corresponding regularity.] (*ibid.*)

Therefore, the universals that are laws of nature have a nontrivial modal character. For example, if it is a law of nature that $N(F, G)$, then it *must* be the case that $\forall x(Fx \rightarrow Gx)$. So it seems that we have a conflict between PROPERTIES and LAWS. Our choices can be stated as a trilemma: either properties can have a nontrivial modal character, or laws of nature don't entail the corresponding regularity, or laws of nature are not universals.

Supposing that we want to keep laws of nature as universals and retain the nontrivial modal character of properties, Bird says that we can discern three choices for the relation between $N(F, G)$ and the extensional inclusion relation $R(F, G)$ that holds whenever $\forall x(Fx \rightarrow Gx)$. These are listed as[11]

11. The notation '$\langle \cdot \rangle$' picks out the proposition associate with whatever is in the angle brackets: for example, '$\langle N(F, G) \rangle$' refers to the proposition that F and G are related by N.

(I) $\langle N(F, G)\rangle$ entails $\langle R(F, G)\rangle$,

(II) $\langle N(F, G)\rangle$ (merely) implies $\langle R(F, G)\rangle$, or

(III) $\langle N(F, G)\rangle$ (contingently) necessitates $\langle R(F, G)\rangle$.

Claiming the relation to be entailment, *i.e.* (I), means that we have to give up PROPERTIES: universals can then have nontrivial modal character. So, if we don't want to do that, we have to give up entailment and accept either (II) or (III).

Accepting (II) makes the relation between N and R mere implication, *i.e.*, (II) is the claim that the extension of R includes the extension of N. But Bird rightly claims that (II) is not acceptable for reasons similar to why it is not acceptable to reduce laws of nature to regularities, because in that case laws cannot be used to explain their instances. Armstrong accepts that argument as part of his total argument against reducing laws to regularities, and it's clear that Bird thinks this is an important argument against laws being reduced to regularities. Accepting (II) means that there are some worlds where $N(F, G)$ obtains but the corresponding regularity does not. Thus (II) is not really an option.

Turning to (III), Bird claims that we must give an account of the relation of contingent necessitation, and if that account is given in terms of the same nomological necessitation that the relation N is supposed to bestow between F and G, then we are headed for an infinite regress. For suppose we call the contingent necessitation relation, required by (III), 'N''; then, according to Bird,

> N' is the third-order analogue of N because it was introduced to do precisely the job that N was required to do. Fa cannot explain why Ga if it is merely a regularity that Fs are Gs; there needs to be a relation of necessitation between F and G (which nonetheless isn't the same as entailment). Similarly, that $N(F, G)$ cannot explain why $R(F, G)$ if it is merely a regularity that whenever N then also R; there needs to be a relation of necessitation between N and R (which also isn't the same as

> entailment). For just this reason it is clear that we will need a fourth-order universal N'', and similarly a fifth-order universal and so on. There is a regress of ever higher order universals of necessitation. Is the regress vicious? Yes. (*ibid.*, p. 151)

This argument is essentially the same argument that van Fraassen has given in *Laws and Symmetry* (1989, pp. 103-109). I think that this is a real problem for the CRU account of laws of nature and that it results from wanting explanations for everything—explanations "all the way down". But, as I claimed in Chapter III, explanations must stop somewhere and it seems that, after positing a (first level of) law of nature, we shouldn't need to posit anything else. If our conception of a law of nature still requires other posits to make it work, then maybe our conception is incorrect.

D. Accepting laws of nature as universals that entail regularities

It seems that one should be able to (a) have laws of nature be higher-order universals, (b) claim that the existence of laws of nature explains the regularities one observes because the universals that are laws of nature *entail* the corresponding regularities, and (c) also claim that there is *no* explanation for why the laws of nature themselves obtain in any possible world. This seems the most natural stance that a scientist would take.

Armstrong rightfully wants laws of nature to be contingent: at least some of the laws of nature for the actual world might not be the laws of nature for other possible worlds. But, in any world, we should think that $N(F, G)$ brings about $R(F, G)$ and that this is a necessary singular fact, or state of affairs, that explains $R(F, G)$. It is part of the concept of what $N(F, G)$ is that it brings about the associated regularity in any world where it obtains. If a scientist says that it is a law of nature that $N(F, G)$, then that simply means that she believes that $N(F, G)$ is a primitive part of our world and that, therefore, she believes, not only that, given the set O of observed things, $\forall x(x \in O \to (Fx \to Gx))$, but also that $\forall x(Fx \to Gx)$. She would also say that, if $N(F, G)$ is a primitive part of another possible world, then that would imply the same thing about the regularities in that world. The question why $N(F, G)$ is part of any world cannot be answered, and we don't have to ask why a law of nature is part of our world, but we do have to ask why regularities are part of our world. The laws of nature that are part of our world explain the regularities. That there are laws of nature in our world can be left a primitive fact.

3. Laws of Nature as Universals with Modal Character

This still leaves us with the question of why some universals in Armstrong's account have modal character and some do not. If Armstrong has to give up his position that all universals are categorical so that he can make sense of the CRU account of laws of nature, it doesn't seem much of a loss, but a principled account is required of which universals have modal features. Armstrong himself has accepted that Bird's ultimate argument is a problem for his account of laws of nature and that he needs to have universals bestow some kind of natural necessity on the particulars they are properties or relations of. In the case of laws of nature the bestowed natural necessity would be on the lower-order universals.

One principled way to attempt to bestow necessity is via the instantiation relation. Explicating the instantiation relationship between particulars and universals has been problematic, and whether there is some necessity in the relationship might be relevant to the use of this ontological view to formulate laws of nature. Armstrong's current view on the relationship between a particular and the universals it instantiates is that the two somehow overlap in a partial identity. This is spelled out in his recent writings (2005), which it is worthwhile to quote at length; he says that

> [a] thing's properties, the universals it instantiates, go to make up the thing, and the things that a universal is instantiated by go to make up that universal. (p. 317)

The second part of this claim sounds more like the class nominalist; recall that for the class nominalist, the property black is just the class of all (transworld) black things. Armstrong continues,

> I thought that a particular, what I have in the past called a 'thin' particular, could be seen as a principle of unity, a one that runs through and collects its many properties, while the universal could also be seen as a principle of unity, a one that runs through and collects its many particulars. A state of affairs becomes an intersection of the two principles, and so the state of affairs is a partial identity. (*ibid.*)

One can still be a primitivist about universals if a universal is a *one* that runs through and collects its many properties—that's just saying what this fundamental thing does—but Armstrong goes on to say, contrasting his views with those of Donald Baxter (2001), that

> Baxter continues to hold that the link between a particular and its universal is a contingent one. It seems to me, however, that once one has identity, even if only partial identity, there will be found necessity. Consider first the particular and its properties. Could the particular have lacked any property that it in fact has? Strictly, no. Necessarily, the particular would have been at least a little different from what it actually is, and therefore would not be the same particular. David Lewis paid tribute to this when he argued that in these circumstances "the same particular in another world" could be no better than a *counterpart* of the actual particular. But now consider the missing property of the particular, with that property taken as a universal. All its instances are partially identical with it according to Baxter's theory. So if the particular is supposed to lack that property, will not the universal be a different entity? I think it must be. Having just the instances it has is essential to the universal being what it is. So the particular *must* have that property. (pp. 317-318)

The last two sentences show clearly where Armstrong is going with his theory of universals. I think that part of Armstrong's revisions to his theory are meant to solve the inference problem for his CRU account of laws of nature.

It seems that he still does not have a fully developed account of that necessity, but he gives a hint of how such a theory might go if it is based on Baxter's theory. Armstrong says that

> there seems to be no reason why the theory [Baxter's] should not apply to those relations between universals that I together with Dretske and Tooley see as the true nature of laws of nature. Suppose that you have $L(F, G)$ where F and G are universals, and L is the relation between them—a particular causal linking of the relevant states-of-affairs-types, I have argued above. Then, given the partial identity view, universal L will intersect, be partially identical, with the pair of universals. And, of course, because identity is involved, the relationship will be a necessary one, even though the universals related are, as I think, contingent beings. These laws will explain, give truthmakers for, what it is to speak of properties bestowing powers on the particulars that instantiate the properties, but without any necessity to postulate powers as entities. (2005, p. 318)

It is difficult to argue for or against a theory that is not fully developed, so what I know say is only speculation. I have my doubts that natural necessity can evolve from a theory that posits no primitive natural necessity. The universals account of properties and relations makes them purely categorical (as opposed to dispositional) and I don't see how one can somehow derive dispositional accounts of things solely from categorical properties and relations; I don't think it can be done.

That powers exist that make things happen in the world seems to be a natural scientific attitude that I've argued for in previous chapters. Whether a philosophical analysis of those beliefs can be offered using only categorical properties and relations seems unlikely, but it is still an open question. Several philosophers have recently attempted to formulate philosophical accounts that include powers or dispositions in things.[12] Whether these are better accounts is not something that I can get into in this thesis.

12. See Mumford (1998), Brian Ellis (2002), Molnar (2003), and Bird (forthcomming).

VI — Conclusions, Problems and Future Directions

> To call a posit a posit is not to patronize it. A posit can be unavoidable except at the cost of other no less artificial expedients. Everything to which we concede existence is a posit from the standpoint of a description of the theory-building process, and simultaneously real from the standpoint of the theory that is being built. Nor let us look down on the standpoint of the theory as make-believe; for we can never do better than occupy the standpoint of some theory or other, the best we can muster at the time. What reality is like is the business of scientists, in the broadest sense, painstakingly to surmise; and what there is, what is real, is part of that question.
> — Willard van Orman Quine (1908 - 2000)
> (from 1960, p. 22)

I've argued that a guarded scientific realism is the appropriate stance to take toward scientific theories. Laws of nature, as legitimate parts of those theories, deserve to be reified in some sense and take part in the glue that holds the world together. The only reified alternative I've considered in some detail is laws of nature as higher-order relations among lower-order universals. Such an account needs to provide for the natural necessity that scientists posit in the world, and I claim that it can be provided only if some universals are taken to bestow natural necessity on the things they relate or are properties of. I'm sceptical that the project can succeed; but I'm hopeful that it will, because if it does, then the metaphysics of the nomic is greatly simplified in that a unified account of laws of nature will have been given. Laws of nature will be no more mysterious than properties and relations. I think that the use of Ockham's razor is mandatory in metaphysics and that a universals account of laws of nature would give a parsimonious solution to the problem of accounting for the glue in the world. I also think that laws of nature as universals is the most likely to succeed in providing an account of global laws, such as conservation laws, which I've almost ignored in this thesis.

If the metaphysical project of accounting for laws of nature using universals cannot be done, then I think that the best recourse is to primitive powers. The relatively recent philosophical theories on powers and dispositions, as essential features of things, hold much potential for a unified account of the nomic. I think that laws of nature will still need to figure in such accounts so as to envelope all the nomic concepts that scientists use, especially conservation laws. In that case, it seems likely that global laws of nature, such as the conservation of energy, will have to be reduced to the nomic features of the individual things that exist in the world. The only alternative would be to consider the spacetime continuum as a thing having nomic powers.

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