CHAPTER V

DETERMINISM AND LAWS OF NATURE

> The problem is sometimes put in the form that we all distinguish between uniformities due to natural law and those which are merely accidentally true, 'historical accidents on the cosmic scale'; if natural laws are just uniformities, how can this distinction be made? It seems to me foolish to deny (as some Humeans do) that such a distinction is made in common speech; but it also seems perfectly sensible to try to give the rationale for this distinction within the ambit of a constant conjunction view.
>
> (R. B. Braithwaite, *Scientific Explanation*)

We have made a start on understanding what properties laws of nature must have if the world is to be deterministic, but nothing much has been said about what laws of nature are, about what distinguishes laws from non-laws. And, strictly speaking, we are in the embarrassing position of having no examples to work with, for none of the examples of so-called laws cited in previous chapters is truly a law since what is asserted has proven to be false (and, by meta-induction, a similar fate awaits every such example??). This realization need cause no undue alarm if we are willing to apply to the history of science a Principle of Respect, recommending that when we encounter a textbook example of a 'law' we assume, unless there are specific contextual indications to the contrary, that (1) the scientists of the period had good reason to believe that what the 'law' asserts is true (or approximately true), and (2) the scientists of the period were justified in believing that, if what the 'law' asserts is true, then it does indeed express a law of nature. While I agree with the spirit of this principle, I think that some caution is required in applying it. In the young sciences it may be a struggle to find any informative generalization that works tolerably well, and so the standards of lawhood may be lax. We can avoid this problem by looking only to the mature sciences for our examples. But in the mature sciences the search for laws is constrained by the record of past successes and failures; research scientists assume, consciously or not, that the candidate laws must have a certain mathematical form, must

incorporate certain variables, must conform to certain symmetry and invariance principles, must reduce in special cases to the old 'laws', must mesh with 'laws' in allied fields, etc. Here opposing snares await us. One is the vulgar relativism of seeing the notion of law so inextricably tied to a scientific community, a research tradition, or whatever that only historical *reportage* is possible. The other is the arrogant abstractionism of supposing that an analysis of laws amounts to no more and no less than finding a core concept that cuts across every branch of science and every period in the history of science. I will be careful to avoid the snare of relativism, but I will knowingly step into a mild form of the abstractionism snare as it applies to modern physics. For my focus in this chapter is on the attempts of philosophers of science to provide an abstractive analysis of laws of physics. My main concern will not be so much with the rather thin character of these attempts as with the discordance which has recently grown to the extent that it cannot be ignored. While unanimity is an unattainable and even undesirable goal in philosophy, something is amiss when we cannot agree even approximately on how to understand a notion that is fundamental to the study not only of determinism but to the methodology and content of the sciences in general.

When in doubt it is a good practice to return to the source. In this case the source is David Hume.

1. HUME'S DEFINITIONS OF 'CAUSE'

Hume defined 'cause' three times over. (Recall: The constant conjunction definition says that a cause is "an object precedent and contiguous to another, and where all the objects resembling the former are plac'd in a like relation of priority and contiguity to those objects, that resemble the latter." The felt determination definition takes a cause to be "an object precedent and contiguous to another, and so united with it in the imagination, that the idea of the one determines the mind to form the idea of the other, and the impression of the one to form a more lively idea of the other."[1] And finally, in the *Enquiry*, but not in the *Treatise*, Hume defines a cause as "an object followed by another . . . where, if the first object had not been, the second never had existed."[2])

The two principal definitions (constant conjunction, felt determination) provide the anchors for the two main strands of the modern empiricist accounts of laws of nature[3] while the third (the counterfactual definition) may be seen as the inspiration of the non-Humean necessitarian analyses. Corresponding to the felt determination definition is the account of laws that emphasizes human attitudes, beliefs, and actions. Latter day weavers of this strand include Nelson Goodman, A. J. Ayer, and Nicholas Rescher. In *Fact, Fiction and Forecast* Goodman writes: "I want only to emphasize the Humean idea that rather than a sentence being used for prediction because it is a law, it is called a law because it is used for prediction . . ." (1955, p. 26). In "What Is a Law of Nature?" Ayer explains that the difference between 'generalizations of fact' and 'generalizations of law' "lies not so much on the side of facts which make them true or false, as in the attitude of those who put them forward" (1956, p. 162). And in a similar vein, Rescher maintains that lawfulness is "mind-dependent"; it is not something which is discovered but which is supplied: "Lawfulness is not found in or extracted from the evidence, but it is superadded to it. *Lawfulness is a matter of imputation*" (1970, p. 107). By contrast, the constant conjunction definition promotes the view that laws are to be analyzed in terms of the *de re* characteristics of regularities, independently of the attitudes and actions of actual or potential knowers.

Hume himself gives passing acknowledgement to the fact that the two strands can diverge.[4] And where they diverge, I follow the constant conjunction strand and declare my starting assumptions that whatever our beliefs, we could be mistaken because there is something to be mistaken about — the distinction between uniformities due to natural laws and those which are merely cosmic accidents is to be drawn in terms of features of the uniformities and not in terms of our attitudes towards them.[5] At the same time I readily concede that this strand cannot be successfully woven into an account of laws by completely ignoring the other strand, for while ontology need not follow epistemology, our account of laws must explain how it is possible to form rational beliefs about what the laws of our world are. The hope is that this epistemological constraint can be met without becoming so entangled in the felt determination strand that we become captives of the Goodman-Ayer-Rescher web.

Against this hope I sense a rising sentiment among philosophers of science that the problem of giving a regularity analysis of laws bears an ominous resemblance to the problem of providing a criterion of 'cognitive significance' to separate empirically meaningful assertions from metaphysical nonsense. It was initially an article of faith among the positivists and logical empiricists that such a criterion must exist and that providing it in a suitable form was only a matter of finding the appropriate technical formulation. But as attempt after attempt fell into the philosophical waste bin this faith has given way to an indifferent agnosticism or, worse, an insipid lip service. If a similar ignominious fate awaits the regularity account of laws, then it would seem best to redirect our efforts elsewhere.

A growing band of philosophers is already at work in the elsewhere, constructing a non-empiricist conception of laws. But before turning to their views, let us review the sources of dissatisfaction with the standard regularity account and explore the prospects of improving it within an empiricist framework.

2. THE NAIVE REGULARITY ACCOUNT

The crudest form of the regularity account puts laws of nature and Humean regularities into one-one correspondence. In the linguistic mode favored by the logical positivists, this account might be rendered thus:

(H) Laws are what are expressed by true lawlike sentences.

What makes the naive regularity account naive is the assumption that 'lawlike' can be captured by syntactical and semantical conditions on individual sentences. E.g., S is lawlike just in case S is general in form (say, a universal condition $(x)(Fx \supset Gx)$ so dear to philosophers determined to make use of their required symbolic logic course) and the predicates are suitably kosher ('F' and 'G' are non-positional, purely qualitative, non-Goodmanized, etc.) This is, to be sure, sloppy and vague, but the impression given by the older references was that all the mysteries of laws would disappear once the appropriate technical apparatus was applied to make notions like 'generality' and 'non-positional predicate' really precise.[6]

We do not need to await the outcome of the technical maneuvers. W. A. Suchting, David Armstrong, and other down-under philosophers have done such a thorough demolition job on the naive regularity account that we can be confident that no way of fiddling with the details of (H) will produce a defensible version. I will just remind you of some of the considerations and refer you to Armstrong (1983) for further details.

There is first the difficulty of uninstantiated lawlike generalizations. To exclude all such generalizations from law status is too severe; witness Newton's First Law ("If the net impressed force acting on a massive body is zero, then the body moves inertially") whose antecedent is very unlikely to be instanced in a universe well populated by massive particles obeying Newton's Law of Universal Gravitation.[7] Contrariwise, to welcome in all uninstanced lawlike generalizations has even more unwelcome consequences, for then the vacuity of the antecedent condition would mean that $(x)(Fx \supset Gx)$, $(x)(Fx \supset G'x)$, $(x)(Fx \supset G''x)$, etc., where Gx, $G'x$, $G''x$ etc., may be pairwise incompatible, are all laws. Such contrary 'laws' are intuitively repugnant, and they pose difficulties for the widely accepted view that laws license subjunctive conditionals. If o (which as a matter of fact is non-F) were F, would it be G, or G', or G'', etc.? A uniform treatment of uninstanced generalizations is unacceptable. But what basis does the naive regularity theorist have for treating such generalizations differentially?

The problem of uninstanced generalizations pales beside the problem of instanced lawlike generalizations which, by the judgments of philosophical intuition and the history of science, do not correspond to laws. Reichenbach's old example still suffices: "All bodies of pure gold have a mass of less than 10,000 kg." This statement is general in form; its predicates are surely kosher; and it is widely instanced. But even if we were assured that it is true, we would not regard it as expressing a law. Nor would it help to be given the further assurance that the known instances are not exhaustive or that there are an infinite number of instances (so that the generalization is not equivalent to a finite conjunction of singular statements). Such assurances would do nothing to convince us that Reichenbach's generalization is a generalization of law rather than of fact.

Can the separation of generalizations of law from generalizations of fact be effected by *de re* features of regularities, or as empiricists are we forced to grasp the safety cord of Hume's felt determination definition? My strategy for answering this question will be, first, to state general constraints on an empiricist account of laws and, second, to explore the prospects and problems of constructing a more appealing regularity account within the confines of these constraints.

3. THE EMPIRICIST CONSTRAINTS

I will state the constraints in a form that may be distasteful to some empiricists. But to mix a metaphor, while I can genuflect before Hume's altar with the best of them, I am no knee-jerk empiricist. I see no reason to deny ourselves whatever analytical tools may help to shape the issues into a manageable form. Without further apology, I state the basic or 0-th empiricist constraint as

(E0) Laws are contingent, i.e., they are not true in all possible worlds.

Next, I propose two forms for further constraints:

(F1) For any possible worlds W_1, W_2, if W_1 and W_2 agree on ___, then W_1 and W_2 agree on laws.

(F2) For any possible worlds W_1, W_2, if W_1 and W_2 agree on laws, then W_1 and W_2 agree on ___.

The blanks are to be filled in by non-question-begging empirical features. 'All Humean regularities' is such a feature, but if used as the filling in both blanks it seems that the conjunction of the resulting constraints forces us back to the naive regularity account.

The filling I prefer for the blank in (F1) produces the following constraint:

(E1) For any W_1, W_2, if W_1 and W_2 agree on all occurrent facts, then W_1 and W_2 agree on laws.

I will refer to (E1) as the empiricist loyalty test on laws, for I believe it captures the central empiricist intuition that laws are parasitic on occurrent facts. Ask me what an occurrent fact is and I will pass your query on to empiricists. But in lieu of a reply, I will volunteer that the paradigm form of a singular occurrent fact is: the fact expressed by the sentence $P(o, t)$, where 'P' is again a suitably kosher predicate, 'o' denotes a physical object or spatial location, and 't' denotes a time. (This is a qualitative version of one of the classical world assumptions used earlier (see Ch. III).) There may also be general occurrent facts (I think there are), but these presumably are also parasitic on the singular occurrent facts. Conservative empiricists may want to restrict the antecedent of (E1) so as to range only over observable facts while more liberal empiricists may be happy with unobservable facts such as the fact that quark q is charming and flavorful at t. In this way we arrive at many different versions of the loyalty test, one for each persuasion of empiricist.

The well-known motivations for (E1) fall into two related categories. There are ontological argument and sloganeering ("The world is a world of occurrent facts"), the two often being hard to distinguish. Then there are epistemological arguments and threatenings, the most widely used being the threat of unknowability, based on two premises: we can in principle know directly or noninferentially only (some subset of) occurrent facts; what is underdetermined by everything we can in principle know non-inferentially is unknowable in principle. I will return to this argument in Sec. 12 below. The argument connects back to the ontological if we add the further premise that what isn't knowable in principle *isn't* in principle.[8]

Finding a filling for the blank in (F2) which produces a defensible but not toothless constraint is more difficult. Consider:

(E2) For any W_1, W_2, if W_1 and W_2 agree on laws, then W_1 and W_2 agree on regularities entailed by the laws.

This lacks bite in the case of non-probabilistic laws, but it is of some help in separating some of the views on the nature of physical probabilities. Hardcore frequency theorists would hold if W_1 and W_2 agree on lawful probabilities and if they both contain infinite repetitions of the relevant chance experiment, then they must agree on limiting relative frequencies; but the hardcore propensity theorist will counter that while agreement of relative frequencies is likely, it is not mandatory. However, a more important difference between frequency and propensity theorists concerns (E1) and the grounding of physical probabilities on occurrent facts (see Sec. 9 below and Ch. VIII). Little use will be made of (E2) in what follows.

Two things remain uncaptured by (E0)–(E2). Neither can be stated in the form of a tidy constraint, but nonetheless each is an important part of the empiricist conception of laws. The first is the intuition that appropriate qualitative and quantitative differences in particular occurrent fact and general regularity make for differences in laws (E3). The second intuition is that there is a democracy of facts and regularities in that each has a vote in electing the laws (E4). The worry about (E4), of course, is whether democracy can prevail without degenerating into the mob rule of the naive regularity view. And the problem with (E3) is that it seems impossible to specify ahead of time in a content and context free manner what counts as an appropriate difference. That (E3) and (E4) are painfully vague does not mean that they are useless; on the contrary, a good check on any proposed implementation of (E0) and (E1) is how well it makes sense of (E3) and (E4).

In the next section I will review what I take to be the most promising approach to laws which fulfills the above constraints and which maintains firm contact with Hume's constant conjunction idea. I will capitalize the *e* in 'empiricism' to indicate my brand of empiricism. There are other and perhaps better brands, but this one recommends itself as a useful foil.

4. MILL, RAMSEY, AND LEWIS

John Stuart Mill, as thoroughgoing an Empiricist as they come, was no naive regularity theorist. Humean uniformities are often called laws in common parlance; but scientific parlance is quite another thing:

> Scientifically speaking, that title [Laws of Nature] is employed in a more restricted sense to designate the uniformities when reduced to their most simple expression. (1904, p. 229)

This 'restricted sense' is explained more fully a little further on:

> According to one mode of expression, the question, What are laws of nature? may be stated thus: What are the fewest and simplest assumptions, which being granted, the whole existing order of nature would result? Another mode of starting the question would be thus: What are the fewest general propositions from which all the uniformities which exist in the universe might be deductively inferred? (1904, p. 230)

When allowance is made for the fact that Mill assumed determinism, his conception of laws seems to correspond exactly to Frank Ramsey's, or rather to David Lewis' de-epistemologized version of Ramsey. Ramsey's dictum was that laws are "consequences of those propositions which we should take as axioms if we knew everything and organized it as simply as possible in a deductive system" (1978, p. 138). Lewis suggests we expunge the reference of knowledge in favor of conditions on deductive systems, known or unknown: ". . . a contingent generalization is a law of nature if and only if it appears as a theorem (or axiom) in each of the true deductive systems that achieves a best combination of simplicity and strength" (1973a, p. 73). Deductive systems are

> deductively closed, axiomatizable sets of true sentences. Of these true deductive systems, some can be axiomatized more *simply* than others. Also some of them have more *strength*, or *information content*, than others. The virtues of simplicity and strength tend to conflict . . . What we value in a deductive system is a properly balanced combination of simplicity and strength — as much of both as truth and our way of balancing will permit. (1973a, p. 73)

Many other forms of the idea that lawhood attaches to individual regularities only via their membership in a coherent system of regularities could be cited,[9] but for the moment let us stick with the Mill-Ramsey-Lewis version and enumerate its virtues.

I take it as evident that the M-R-L account does satisfy the basic Empiricist constraints (E0) and (E1), does provide for the democracy of facts and regularities (E4) without surrendering to the mob rule of the naive regularity account, and does provide a framework for understanding what sorts of differences in particular fact and general regularity make for differences in laws (E3). It also has the virtue of explaining why laws have or tend to have various 'lawlike' characteristics, such as universality (more on this in Sec. 6 below). It allows in some vacuous generalizations without opening the floodgates to all. And it connects in a direct and natural way to the actual practice of scientific theorizing or at least to the most widely held reconstruction of the practice in the form of the hypothetico-deductive method. In fact, in much of the current literature on the structure and function of scientific theories, 'theory' and 'deductive system' can be freely interchanged.

5. DEDUCTIVE SYSTEMATIZATION: A CLOSER LOOK

It is no criticism of M-R-L to note that simplicity and allied notions such as coherence and systematization are vague and slippery, for so is the notion of laws of nature. The question is whether the vaguenesses and slippages match. That old nemesis, Reichenbach's gold lump generalization, gives pause. If this generalization is to be counted out as a law by the lights of M-R-L it is because it is not an axiom or theorem in the best (or each of the best) overall deductive systems for our world. Consider then what would happen if we were to add it as an additional

axiom. There would, by hypothesis, be a gain in strength. And, presumably, there would also be a loss in simplicity. The loss must, *pace* M-R-L, outweigh the gain. I will not say otherwise. But I do say that it is not compellingly obvious that the scales tip in this way while it is compelling that Reichenbach's generalization is not to be counted as a law.

The trouble here may not lie with the squishy notion of simplicity but with the seemingly more solid notion of strength. Lewis suggests strength be measured by information content, and that is as good a measure as any if we are interested in strength *per se*. But actual scientific practice speaks in favor not of strength *per se* but strength in intended applications; for dynamical laws this means strength as measured by the amount of occurrent fact and regularity that is systematized or explained relative to appropriate initial and/or boundary conditions. The advantage offered by deterministic generalizations here is obvious: while they can be strengthened *per se*, they are, in their intended applications, as strong as strong can be; for given the state of the system at any instant, they entail everything true of the system, past, present, and future, and any other generalization is either incompatible or adds nothing to applied strength. This helps to explain why we feel confident that in having discovered a simple set of true deterministic relationships we have discovered laws. This is not to say that determinism is either necessary or sufficient for a good trade-off between simplicity and applied strength. If a deterministic set of generalizations can be constructed only at the price of very high complexity, then the scales may tip against determinism; but typically the complexity must be great indeed before the tip becomes pronounced. And when no set of true deterministic generalizations is available, many different compromises between simplicity and strength may recommend themselves. This helps to explain why, independently of ontological considerations, determinism has been prized as a methodological guide to scientific theorizing.

What started as an objection to the M-R-L account has turned into a plus. Another plus comes from reflection on the notion of chaos. It is tempting to define chaos as the absence of any pattern or regularity, but the discussion in Ch. VIII will cast doubt on the coherence of this idea. However, chaos as the non-existence of laws is explicable on the M-R-L account. This form of chaos need not require that all regularity is absent but only that the existent regularities are sufficiently weak and messy that there are no good compromises between strength and simplicity.

In closing, I have to confess to a real worry about the M-R-L account, or rather to the linguistic version I have been reviewing. Given a choice of language — primitive predicates and logical apparatus — we may be able to identify a best overall deductive system. But different choices of language may promote different candidates for the role of best system. These candidates may be incommensurable, not admitting meaningful comparisons of simplicity and strength. Or else they may be commensurable and equally good in their different ways, forcing us to say either that there are no laws since there are no non-trivial axioms or theorems common to all the best systems, or that the laws are relative to a choice of language. These worries can be diminished by refusing to give in to the logical positivists' fear of the ontological and their flight to the linguistic. Recall that my canonical formulation of determinism assumes that the possible worlds can be characterized in terms of space-time magnitudes. Worlds are thus isomorphic to sets of basic propositions, each asserting that the value of such-and-such a magnitude takes a value of so-and-so at thus-and-such a spatio-temporal location. The laws of the actual world are then the propositions that appear in each of the deductively closed systems of general propositions that achieve a best systematization of the basic propositions true of the actual world. So while different systems may employ different concepts, there will of necessity be a strong common core.

6. LAWS AS UNIVERSAL AND ETERNAL TRUTHS

Different philosophers of science draw up different lists of properties they think laws should have, but there is wide-spread agreement that laws should express universal and eternal truths; that is, they should apply unrestrictedly to all space and all time, and they should not change with time.

The notion that laws 'change with time' is ambiguous as between having a single law that is not time translation invariant vs. having different laws in different epochs. I can offer no general criterion to decide which case is which. What I can offer for the former category is an analysis of the meaning of time translation invariance, its status as a requirement of lawhood, and its connection with other symmetry principles and with determinism. This I will do in Ch. VII. For cases

which fall into the latter category we obviously have a violation of universality since the putative laws apply only to limited stretches of time.

No purely syntactical criterion can capture the intended sense of universality. If we are clever enough we can always rephrase any assertion so that it has the form $(x)(t)[\ldots]$, which appears to assert something about *all* space and *all* time. The appearance can be belied by the intended meaning of the predicates and relations which fill the ellipsis. To cite the standard example, $(x)(t)[P(x, t) \supset S(x, t)]$ appears to be general in form and universal in scope, but if '$P(x, t)$' means that x is in Nelson Goodman's left-hand trouser pocket for t between March 30 and 31 in 1948, the relevant sense of universality is absent. It will not do, however, to exclude all predicates and relations whose intended interpretations refer to particular spatio-temporal regions; we can, for instance, formulate a relationship between the rate of expansion of the universe and its mass content which refers explicitly or implicitly to the initial 'big bang' but which applies to all space-time.

For a space-time theory in my sense we can easily express the desired sense of universality in model-theoretic terms. Recall that the intended models have the form $\mathcal{M} = \langle M, 0_1, 0_2, \ldots \rangle$ where M is the space-time manifold and the 0_i are geometric object fields on M. For any subregion $R \subset M$, the restriction $\mathcal{M}|_R$ of $\mathcal{M}$ to R is $\langle R, 0_1|_R, 0_2|_R, \ldots \rangle$. The putative law L of T lacks universality just in case it does not apply to some non-empty R; intuitively, as far as L is concerned, 'anything goes' in R. We can take this to mean that for any logically possible $\mathcal{M}$, $\mathcal{M}|_R$ satisfies L.

The M-R-L account of laws explains why universality is prized as a feature of laws; namely, it promotes both strength and simplicity. Still, is it conceivable that the occurrent regularities of the world could be so structured that there is an obviously best overall compromise between strength and simplicity involving 'laws' that are not universal? I will leave it to the reader to construct and evaluate examples for herself. A positive result will be counted against the M-R-L account by those who promote the relations-among-universals view of laws since for them laws are necessarily eternal and universal.

7. DEFEASIBILITY AND DEGREES OF LAWFULNESS

Mill has been read as defining laws as indefeasible or unconditional uniformities. This is, I think, a backwards reading. 'Unconditional' is analyzed as "invariable under all changes of circumstance," but the range of circumstances that may serve as defeasors is defined to be precisely those allowed by the "ultimate laws of nature (whatever they may be) as distinguished from the derivative laws and from the collocations" (Mill, 1904, p. 244). And these ultimate laws are defined as the axioms of the M-R-L system. Thus, it is only when we have in hand some candidate for the M-R-L axiom system that a defeasibility analysis can begin.[10]

Ramsey had a similar idea in distinguishing four categories of universal generalizations. At the top are the ultimate laws; then come, in descending order, derivative laws, then those called laws "in a loose sense," and finally the universals of fact. Derivative laws are simply the universal generalizations that are theorems of the best deductive system. Laws-in-a-loose-sense are those general propositions deducible using "facts of existence assumed to be known by everybody" (1978, p. 130). Universals of fact are the accidental or non-lawful generalizations. Ramsey was quick to note that the last two categories cannot be sharply separated. The separation rests on the amount of fact allowed in the deduction; if, for example, determinism is true, all universals of fact can be deduced from the ultimate laws together with enough facts of existence. (This is why Mill, a determinist, defined the ultimate laws to be the fewest general propositions from which *all* the uniformities which exist in the universe are deducible.) Nevertheless, I agree with Ramsey that the distinction is a useful one, and I propose to redraw it in a somewhat more elaborate form and relate it to Mill's defeasibility notion.

Ideal and complete defeasibility of a universal generalization of fact would show how its truth or falsity turns on contingencies by providing (i) a two-fold partition of the initial/boundary conditions compatible with the M-R-L axioms into those which together with the axioms guarantee the failure of the generalization (the defeasors) vs. those which together with the axioms guarantee the truth of the generalization (the enablers), and (ii) a demonstration of how generic or exceptional the enablers and defeasors are in the models of the axioms. The suggestion then is that degrees of indefeasibility or lawfulness can be assigned depending on the results of (ii). Those generalizations whose enablers are exceptional ('measure zero') and whose defeasors are generic are rightly called merely accidental, while those whose enablers are generic and whose defeasors are exceptional can approach lawhood.

Such a classification scheme demands much of our laws — (i) presupposes that the ultimate laws are appropriately complete, and (ii) assumes that a suitable measure can be defined on the initial/boundary conditions — and there is no *a priori* assurance that nature will answer these demands. But as an example where such a defeasibility analysis has been carried out, I would cite recent work on the singularities of gravitational collapse as described by Einstein's general theory of relativity. The singularities first discovered in solutions to Einstein's field equations were thought to depend on the idealized features of the special models under study (viz., perfect spherical symmetry). However, several decades of work, culminating in the theorems of Penrose and Hawking showed that the situation is just the reverse; singularities of collapse develop under generic conditions (see Ch. X).

8. CHALLENGES TO THE REGULARITY ACCOUNT OF LAWS

The details of my Empiricist constraints will be filled in in different ways by Empiricists of different stripes. And once the details are supplied, it remains to settle on the best means of implementing the key constraint (E1) by specifying how the occurrent facts determine the laws; perhaps, as argued, a slightly modified version of M-R-L is the best bet, perhaps not. All of this is subject to continuing discussion and debate. But one wonders whether the basic thrust of the Empiricist program as I outlined it is seriously discussible, or whether any attempt at discussion quickly degenerates into an exchange of Empiricist and anti-Empiricist epithets. In what follows I will try to describe challenges to the Empiricist account in such a way that something can be learned from the resulting debates, though eventual termination in irreconcilable intuitions is to be expected.

I will postpone until Ch. XI a discussion of the challenge the quantum theory poses to the occurrent ontology presupposed in the classical world view and in the Empiricist constraint (E1). However, non-occurrent dispositions, potentialities, and propensities are all encountered outside of the strange realm of quantum physics. Their implications for a regularity account of laws will be taken up next. I will then turn to the recent attack which has been launched against the regularity account by Armstrong, Dretske, Tooley and others who see laws of nature as relations among universals.

9. DISPOSITIONS: THE GARDEN VARIETY TYPE

I want first to clear away two sources of misunderstanding about dispositions, due ironically, to two of the most important contributors to the subject, Rudolf Carnap and Sir Karl Popper.

On Carnap's analysis of scientific concepts, disposition terms occupy an "intermediate position between observation terms . . . and theoretical terms" (1964, p. 63). Given Carnap's epistemologically oriented concern with testability and meaning, it is easy to appreciate the motivation for his classification, but for our purposes the ontological dimension is more important. Along this dimension, the contrast to dispositional is neither observational nor theoretical but occurrent. And the dispositional-occurrent distinction does not lie parallel to the theoretical-observational distinction but oblique to it — theoretical terms may denote either occurrent or dispositional properties (see Mellor (1971) Ch. 4).

My other complaint is aimed at Sir Karl's contention that

> all physical . . . properties are dispositional. That a surface is coloured red means that it has a disposition to reflect light of a certain wave length. That a beam of light has a certain wave length means that it is disposed to behave in a certain manner if surfaces of various colours, or prisms, or spectrographs, or slotted screens, etc., are put in its way. (1962, p. 70).

Taken literally, Sir Karl's remarks threaten to erase occurrent ontology, leaving a squirming, twisting mass of dispositions. On further reflection, however, it is clear that the sense in which all physical properties are dispositional is a harmless guilt-by-association sense. As far as we know, each physical property is joined to others by lawful regularities. (And if this were not so, how could we know it was not so?) This is especially true of properties denoted by theoretical terms because these terms are usually introduced precisely for the purpose of formulating laws. Thus, that a beam of light has a certain wave length does 'mean that' the beam is disposed to behave in a certain manner whenever a slotted screen is put in its way; that is, the relevant laws of optics license the subjunctive conditionals about such behavior. But having a given wave length is unlike having a pure dispositional property (say, solubility) in two related respects: the former is an occurrent property and it has, in Carnap's phrase, an 'open texture' in that its meaning is not exhausted by any one or even a collection of such subjunctive conditionals.

Garden variety dispositions, like solubility, hardly require Empiricism to flex its muscles. We are confident that the secrets of dispositions to dissolve are to be found jointly in (a) occurrent facts about the micro-structure of salts and crystals and (b) laws couched purely in terms of occurrent properties. Thus, to the extent that we are convinced that the relevant laws pass the Empiricist loyalty test (E1), we can likewise be confident that dispositions to dissolve do not hold non-Humean powers:

(D) For any W_1, W_2, if W_1 and W_2 agree on all occurrent facts, then W_1 and W_2 agree on dispositional facts regarding solubility (and other garden variety pure dispositions).

Nothing in the more impassioned defenses of dispositions — such as Mellor's (1974) — moves me to abandon my Victorian prejudice. Garden variety dispositions, like unmarried mothers, cannot manage (it) on their own. And the success of science in showing how it is managed on an occurrent basis makes claims to the contrary seem like so much mystery mongering.

If it is mysteries you want they are ready and waiting for us once we move from garden variety dispositions to physical probabilities construed as probabilistic dispositions or propensities. I reject out of hand the view of the finite frequentists who identify physical probabilities with ratios in finite classes or sequences.[11] This view fits the most stringent form of Empiricism imaginable, but it makes analytically false any assertion which sets probability equal to an irrational number and also any assertion that makes the probability of the outcome of an experiment p, for p strictly between 0 and 1, when as a matter of fact the experiment is performed only once. Limiting relative frequencies in infinite sequences of outcomes escape these difficulties, but, to complete the march towards probabilities as dispositions, we need only add that an actually infinite repetition of the relevant experiment is rarely, if ever, to be found in nature.

Both the hypothetical frequency theorist and the propensity theorist agree that physical probabilities are, in some sense, dispositions or propensities.[12] The difference, roughly speaking, is that the frequency theorist is guided by a determination to remain Empiricist and tends to see probabilistic dispositions as reducible in much the same way we supposed the disposition of solubility to be. The most radical of the propensity theorists — those who assign probabilities to single cases — resist reductionism and sail close to the conclusion that propensity probabilities are non-Humean powers.

These issues will be developed in Chs. VIII and XI. In the remainder of this chapter I will restrict attention to non-probabilistic laws.

10. TOOLEY'S CASE

David Armstrong, Fred Dretske, Michael Tooley, and Chris Swoyer have all proposed that laws of nature are relations among universals. Their views differ in interesting and subtle ways, but for the moment I will lump Armstrong, Dretske, and Tooley together because they accept the minimal Empiricist constraint (E0) while Swoyer does not. The triumvirate is also unanimous in rejecting any form of the regularity account. As Armstrong puts it, "I am saying that we can keep the Humean uniformities fixed, and vary the laws indefinitely" (1983, p. 71, fn. 3). This is a disavowal not of (E1) but of the stronger

(E1′) For any W_1 and W_2, if W_1 and W_2 agree on all Humean regularities, then W_1 and W_2 agree on laws.

But unless I misread Armstrong he intends to reject (E1) as well, as I think he must if he wants to overthrow every variant of M-R-L.

In trying to understand the intuition behind the rejection of (E1) it is useful to review a hypothetical case constructed by Tooley.

> Imagine a world containing ten different types of fundamental particles. Suppose further that the behavior of particles in interaction depends upon the types of the interacting particles. Considering interactions involving two particles, there are 55 possibilities with respect to the types of the two particles. Suppose that 54 of these possible interactions have been carefully studied, with the result that 54 laws have been discovered, one for each case, which are not interrelated in any way. Suppose finally that the world is sufficiently deterministic that, given the way particles of types X and Y are currently distributed, it is impossible for them ever to interact ... In such a situation it would seem very reasonable to believe that there is some *underived* law dealing with the interaction of the particles of types X and Y ... (1977, p. 669)

Tooley argues further that the best M-R-L system for his hypothetical world will not contain any axioms or theorems describing how the unsociable particle species X and Y would behave if they were to interact. Something has to give, and what gives, according to Tooley, is the M-R-L account. It is then but a short step to the conclusions that no form of the regularity account will work and that if we want truth makers in the world for the underived laws about X—Y interactions, we had best begin looking for relations among universals.

Leaving until later a discussion of the merits of the universals view of laws, I want to respond to the attack on M-R-L. I begin by asking Tooley how he can be so sure that the seemingly unsociable particle species are not acting upon one another at-a-distance and that the regularities of this interaction do not show up in the best overall deductive system. Or how can he be so sure that there is no unified particle theory which explains all ten species in terms of a more fundamental particle (the quack, say, which comes in ten honks) and which shows up as part of the best deductive system? The story can be told in increasing detail so as to rule out these and other such possibilities. But, the Empiricist would contend, the more such detail, the more implausible it becomes that there is any truth to the matter of laws about X—Y interactions.

Part of the intuitive appeal of Tooley's example comes from the meta-induction he invites us to make on the basis of the 45 laws derived from observations of interactions of pairs of particles from different species. I agree that such meta-inductions can override initial first-order inductions where we build a M-R-L system on the basis of a limited range of observed regularities. But in the limit where the basis expands to include all occurrent facts and regularities, the meta-induction must give way to the first-order induction. For example, a meta-induction on derived laws may speak strongly in favor of some conservation principle, and if this principle clashes with the results of initial attempts to incorporate a newly discovered interaction into the best deductive system, then the meta-induction may prevail, sending the deductive system back to the drawing boards. But 'back to the drawing boards' means collecting more information in the form of occurrent facts and building a new deductive system on the broader basis. If no matter how far we expand the basis, the axioms still conflict with the conservation principle, then it is the meta-principle which must go.

The sophisticated Empiricist has no blanket prohibition against uninstantiated laws; such laws may well emerge as consequences of the axioms of the best deductive system. The unalterable Empiricist constraint is that such laws must perform in the service of the actual; they must arise in the attempt to account for actual occurrent facts and regularities. To hold otherwise is to break the grip the actual holds on the possible. It is easy to imagine richer and more sociable worlds where the species *X* and *Y* do interact and that from the regularities of interaction we derive the laws. The trouble is that it is all too easy, for we can imagine many such worlds, each with different M-R-L laws of *X—Y* interaction. Armstrong's way of putting the point seems to me exactly right (see (1983), Ch. 8, Sec. 4). We do have the intuition that if conditions were such as to permit *X—Y* intercourse, there would be some sort of *X—Y* interaction laws. That intuition is, of course, perfectly consistent with M-R-L if in each of the possible worlds where intercourse is consummated, the regularities of interaction find their way into the best deductive system. But there is, in this world, no truth to the matter as to what form the laws of *X—Y* interaction would take; for by Tooley's construction there is nothing in actual occurrent fact or regularity to allow us to say which of the possible worlds with *X—Y* intercourse is 'nearest' or 'most similar' to this world.

11. NOMIC NECESSITY

The view of nomic necessity that first comes to mind upon hearing that laws express relations among universals is that of the Idealists, viz., nomic necessity is but dimly perceived logical necessity (see Ewing (1974)). A neo-Idealist conception of laws has been recently assayed by Christopher Swoyer (1982). For Swoyer, laws express non-contingent relations among properties, or more precisely, relations which are contingent only upon the existence or exemplification of the properties. Hume's ghost stirs. Surely, it quails, for any putative law involving distinct properties, we can imagine a possible world where the properties are exemplified but the relation fails. But on the present proposal, such imaginings are idle, for Hume's move from conceivability to possibility is illicit. You may think that you can dream up a possible world where the electric and magnetic fields do not obey Maxwell's laws, but if Maxwell's equations do indeed express laws, then the world you dreamed up is but a dream and a bad dream at that. The **E** and **B** fields of your dream may be like electric and magnetic fields in various ways, but they are counterfeits, for Maxwell's equations are constitutive of the very nature of electromagnetism.

While I agree that the step from conceivability to possibility must be taken with care, I have no worry that in speculating about a world where magnetic monopoles exist and where, as a result, Maxwell's equations have to be modified, I am changing the meaning or the reference of the term 'electromagnetic field'. Some physicists have seriously proposed that the actual world contains magnetic monopoles. The truth of this proposal does not make me worry that most electromagnetricians since Maxwell have been referring to nothing at all or else to counterfeit electromagnetic fields. On the other hand, if I am wrong and Swoyer is right, I want to know the error of my ways, and not just in general philosophical terms but in specific cases. But I do not see what evidence would indicate that science had uncovered a relation between **E** and **B** which is truly constitutive of the electromagnetic field vs. a contingent relation which plays a central role in the formulation of the best deductive system. I will return to the epistemological problem for contingent relations among universals in Sec. 12.

There is less agreement on whether Hume succeeded in banishing contingent forms of natural or physical necessity. To decide the matter we need to come to grips with physical necessity at least well enough to know what we are banishing. On the Empiricist conception, it is not coherent to present the metaphysics or semantics of physical necessity by postulating physically possible worlds as a single distinguished proper subset of the logically possible worlds, or to take the relation of nomic accessibility to be a primitive. For the Empiricist, there are no irreducible modal facts. A world *W* is a world of non-modal facts. Uniquely associated $W \mapsto L_W$ with each such world is a set L_W of non-modal propositions true in *W* — the laws of *W*. To mark off the elements of this set we may prefix 'it is physically necessary that', but that prefix is merely an honorific. Accessibility is a defined relation, not a metaphysical given: world *W′* is nomically accessible from *W* iff the L_W are all true in *W′*. There are then myriad subsets of physically possible worlds, each radiating outward from a logically possible world. No one of these collections is more powerful or potent than any other.[13]

We can still ask whether physical necessity so construed can display the trappings of strong necessity in the form of the S_4 and S_5 axioms. Here the S_4 axiom says that physical necessity is robust in that it is transmitted along the relation of nomic accessibility: if *W′* is nomically accessible from *W*, then $L_W \subseteq L_{W'}$. The S_5 axiom requires the converse:

if W' is nomically accessible from W, then $L_{W'} \subseteq L_W$. The full S_5 system then requires that $L_{W'} = L_W$. It should be evident that the M-R-L account of how the association $W \mapsto L_W$ is fixed is incompatible with either S_4 or S_5. Suppose, for example, that the laws $L_{W_@}$ of the actual world $W_@$ are the laws of Newtonian mechanics. A world W' containing a single massive particle moving inertially is nomically accessible from $W_@$. But, presumably, the best M-R-L system of W' will not contain Newton's laws but instead an axiom to the effect that all massive particles move inertially; so on the M-R-L version of $W' \mapsto L_{W'}$, $L_{W_@} \not\subseteq L_{W'}$, and $L_{W'} \not\subseteq L_{W_@}$.

Further, from the most basic of the Empiricist constraints, (E0) and (E1), only Confrontational Empiricism is consistent with the full S_5 system; that is, if the S_5 system reigns, the Empiricist laws of two worlds can differ only by being incompatible. The unattractiveness of the ways to affect the association $W \mapsto L_W$ so as to produce Confrontational Empiricism shows why the Empiricist would want to reject even the formalism of strong necessity. There is, for example, Fascist Empiricism: every fact corresponds to a law. Or Imperialist Empiricism: start with a possible world W; choose some set of general propositions true in W and declare them to be the laws L_W of W; take all W's nomologically accessible from W and declare the laws L_W from W to be the laws of W'; choose some W'' outside the first circle and repeat the construction; repeat again and again until all the possible worlds are covered. Any analysis of laws that rejects (E1), while perhaps not being couched in the terminology of strong necessity, will have something of this imperialistic flavor since the putative laws will ride roughshod over the occurrent facts.

Is there any reason to think that physical necessity should follow the dictates of strong S_5 necessity so that, in fine philosophical fashion, the above observations may be turned round and used as an argument against the Empiricist account? At the risk of jousting with straw men, I will note that Tooley's interpretation of the particle example fits with (but does not require) the intuition that nomic accessibility is a symmetric relation so that the laws of X—Y interaction, as evidenced by the regularities of interaction in richer and more sociable worlds, can be brought back to the actual world. I have already said my piece on this example and will say no more. As for the S_4 axiom, I sense that necessitarians — whether they construe necessity in terms of one property yielding or necessitating another or whether they hold a more

orthodox modal construal — think that in order for a law to support a counterfactual conditional, the law must not only be true in but must be a law of the world in which the counterfactual situation is imbedded. This is, I believe, a false view of counterfactuals. I will have more to say on counterfactuals in the following section.

12. LAWS AS CONTINGENT RELATIONS AMONG UNIVERSALS

I have to this point neglected the most ambitious attempt to establish the Armstrong-Tooley-Dretske thesis that laws are contingent relations among universals. I will call it the transcendental argument. It has two parts: if laws did satisfy the Empiricist constraints (E1)—(E4), then they would not be able to adequately fill the roles they are supposed to play in supporting subjunctive and counterfactual conditionals, providing explanations, and in grounding induction; and, the argument continues, it is only by adding relations among universals that laws can gain the strength they need to discharge these roles.

Dretske's version of the counterfactual complaint is representative. The complaint is that on the regularity conception of laws, it is a "complete mystery" how laws support counterfactals, for "To be told that all F's are G is not to be told anything that implies that if x were an F, it would be a G" (Dretske (1977), p. 255). True but irrelevant. The real question is whether to be told that it is a *law* that all F's are G is to be told something that implies (or, as I would prefer to say, supports) the conclusion that if this x were an F, it would be G. On behalf of Dretske I will reply: The answer must be negative on, say, the M-R-L version of the regularity account. For then to be told that it is a law that all F's are G is to be told that all F's are G and that this regularity fits neatly with other such regularities to form a strong and simple deductive system. But this just comes down to saying that some regularity, more complex and comprehensive, but no different in kind from all F's are G, holds.

Not so fast! The fact that "All F's are G" is an axiom of the best overall deductive system for this world informs the judgment of similarity we make when comparing other possible worlds to this world. With this information in hand, that "All F's are G" is true in world W_Y but not in W_N is powerful, but not irresistible, persuasion that W_Y is more similar to the actual world than is W_N. Couple this with Lewis's (1973) analysis of counterfactuals and subjunctives and we have a way of seeing how the M-R-L lawhood of "All F's are G" supports the subjunctive "If x were an F it would be a G".

Armstrong's counterfactual complaint (1983, Ch. 5, Sec. 4) is that judgments of comparative similarity are context dependent while the truth and falsity of counterfactuals are not. On the contrary, I think that the logic of counterfactuals is radically context dependent (see van Fraassen (1981)) and that in some contexts we may judge some W_N world to be nearer actuality than any W_Y world and judge it to be false that if x were an F it would be a G. But if one does not want such results, then it can be added as a constraint on comparative similarity that laws always have an overriding priority in assessing similarity. If laws are contingent — whether on occurrent facts or on non-occurrent facts about the relations of universals — then I do not see that there is any other alternative;[14] unless, that is, one is prepared to offer a wholly different analysis of counterfactuals.

I turn now to explanation. Hardbitten Empiricists are apt to disparage the notion of explanation. The Quine of "Necessary Truth" allows that "in natural necessity, or our attribution of it, I see only Hume's regularities, culminating here and there in what passes for an explanatory train or promise of it" (1976, p. 76). The Wittgenstein of the *Tractatus* was more straightforward: "The whole modern conception of the world is founded on the illusion that so-called laws of nature are explanations of natural phenomena" (1961, 6.371). Empiricists would do better, I think, to accommodate the notion of scientific explanation. It is the universalist's contention that no such accommodation is possible. Thus, Dretske writes:

> The fact that every F is a G fails to explain why *any* F is a G . . . The fact that all men are mortal does not explain why you or I are mortal; it *says* (in the sense of *implies*) that we are mortal, but it does not even suggest *why* this might be so . . . Subsuming an instance under a universal generalization has as much explanatory power as deriving Q from $P \cdot Q$. None. (1977, p. 262)

Professor Armstrong's complaint is similar:

> All F's are G's is a complex state of affairs which is in part *constituted* by the fact that all observed F's are G's. 'All F's are G's' can even be rewritten as 'All observed F's are G's and all unobserved F's are G's'. As a result, trying to explain why all observed F's are G's by postulating that all F's are G's is a case of trying to explain something by appealing to a state of affairs part of which is the thing to be explained. (1983, p. 40)

The remedy for this situation is supposed to be a linkage of necessitation between the universals F and G; in Armstrong's notation, $N(F, G)$, read "F-ness necessitates G-ness". But the explanatory force of such a linkage has got to derive not from the strength of the necessitation but from its quality; as Armstrong warns, even the strongest reasons for believing *that* something is or must be the case need not explain *why* it is the case. What then is the quality of $N(F, G)$ which confers explanatory power? Dretske's answer is that $N(F, G)$ (or in his notation, $F \rightarrow G$) explains why this F is a G because it means that "F-ness is linked to G-ness" in the sense that "the one property yields or generates the other" (1977, p. 264). Armstrong's answer is that $N(F, G)$ explains why all (observed) F's are G's because it unifies the instances of the regularity (1983, Ch. 6).

The Empiricist has a ready response to these concerns. What the universalists seek to achieve through ontological ascent, the Empiricist achieves by ascent of explanatory level. Unification of observational regularities is achieved by passing to higher level laws while evolutionary accounts of how one set of properties yields or generates another set are to be found in the dynamical laws of physics. The truth of Kepler's law, "For every planet, the radius vector from the sun to the planet sweeps out equal areas in equal times," may not explain why this phenomenon is so for the earth and for all observed planets. But Newton's laws of motion coupled with his law of gravitation do explain Kepler's generalization and, thus, why particular instances conform to it. The explanatory malaise feigned by the universalists is diagnosed not as a symptom of a defect in Empiricist explanations but as a result of the artificiality of philosophical discussions where the 'laws' discussed are of the "All ravens are black" variety.

The universalists may reply that at each ascending level the explanatory malaise arises again. We will, for example, want to know why the sun and the earth attract each other with a force inversely proportional to the square of the distance between them, and Newton's force law does nothing to help us here since it simply asserts that this is so for any pair of massive bodies. The Empiricist must concede that his brand of empiricism does not provide for ultimate explanations in the you-can't-ask-for-anything-more sense. But then neither does the universals conception. I may agree that being massive yields or generates gravitational attraction, but this does not block my request for a fuller and deeper understanding of the how and why of gravitation if, as

Armstrong and Dretske would have it, the Idealists are wrong and the relation between massiveness and gravitational attraction is contingent. It took more than two centuries before the how and why were revealed by Einstein. But his revelations came not in the form of a discovery of a bonding of universals missed by Newton but in the form of a new level of explanation in terms of space-time warps.

I turn now to a closer look at the nature and status of $N(F, G)$. $N(F, G)$ is supposed to be contingent and more so than Swoyer's thin sense that it depends on the existence or exemplification of the universals F and G. But $N(F, G)$ is not contingent on occurrent facts — (E1) is violated; rather it is contingent on another category of facts which transcend the occurrent. How then do we have epistemological access to $N(F, G)$? If W_1 and W_2 share all occurrent facts, they are, by Empiricist lights, the same world. For those who say otherwise the Empiricist will crank up the unknowability argument, rehearsed briefly in Sec. 3, in order to show that W_1 and W_2 are epistemologically indistinguishable; so if $N(F, G)$ holds in one but not the other of these worlds, we could never know which was which. This indistinguishability claim can be attacked in two ways. First, one could challenge the Empiricist premise that only occurrent facts can be known directly or non-inferentially and try to show how direct knowledge of $N(F, G)$ can be obtained. Second, one could challenge the second Empiricist premise that what is underdetermined by everything which is in principle directly knowable is unknowable in principle and try to show how inferential knowledge of $N(F, G)$ is sustained. Armstrong takes the first route and Tooley the second.

At the end of Vol. II of *Universals and Scientific Realism* Armstrong suggests that we have non-inferential knowledge of nomic necessitation via direct perceptual awareness of instances of causal connections (1978, Vol. II, pp. 162ff). This corresponds to knowledge of the intermediate link in the chain

$$N(F, G) \Rightarrow (x)N(Fx, Gx) \Rightarrow (x)(Fx \supset Gx)$$

where the arrow is an entailment relation[15] and $N(Fx, Gx)$ means that x's being F necessitates its being G. Strictly speaking, what we have is direct knowledge that one event necessitates another, which knowledge becomes upon reflection the knowledge that there exist universals F and G and particular x such that x's being F necessitates its being G. I wish Armstrong had used *What is a Law of Nature*? to elaborate further on his earlier remarks. In the absence of further elaboration I waive the well-worn Humean objection that upon reflection our knowledge of causal sequence becomes knowledge of constant conjunction and/or felt determination. But I do not waive my conviction that if there is nomic necessitation, its ultimate springs are most likely hidden from our view. The ultimate laws of nature, whatever they may be, will most likely involve universals whose instancings correspond to states of affairs which are not directly observable and which are thus knowable only inferentially.

The subject of unobservability suggests an analogy that may be helpful to the universalist in impeaching the second premise of the Empiricist unknowability argument. Strict Empiricists have sometimes sided against a realistic interpretation of scientific theories on the grounds that theories are underdetermined by everything that is in principle knowable by direct observation. Scientific realists respond that such underdetermination is not fatal because we can have general reasons for believing in the existence of unobservable theoretical entities and specific reasons for believing one observationally equivalent theory over another. The suggestion then is that the universalist try to show that he can parallel the scientific realist's response and in this way demonstrate that realism with respect to relations among universals is no worse off than scientific realism in general.

Some forms of the scientific realist responses do not appear to lend themselves to this piggyback strategem. Unless Armstrong is correct about our having direct perceptual awareness of instances of nomic necessitation, the universalists cannot avail themselves of the slippery slope response; viz., there is a blurred and shifting line between what is and is not observable, with yesterday's unobservable becoming tomorrow's observable. Nor can the universalist latch onto the goals of science response; viz., in order to achieve, say, deterministic laws or the linking together and systematization of observational regularities, it is necessary to ascend to the theoretical level. Examples due to van Fraassen (1980) and Rynasiewicz (1981, 1983) show quite conclusively that the observational content of a theory T cannot be identified with the set of observational sentences logically implied by T. A better construction might go something like this. Start with the models of the axioms of T; then take their observational reducts;[16] then restrict the domains of the reducts to objects which are directly observable. The resulting set of models corresponds to the class of observational states of affairs allowed by T. But most likely this class will not be an elementary class in even the wider sense; that is, it will not be the set of models satisfying some countable set of sentences (see Rynasiewicz, 1981), so even minimal systematization is not possible at this level. Of course, the strict Empiricist will rejoin that the systematization afforded by T is a pragmatic virtue, providing a reason to use T but no reason to believe that its theoretical assertions are true. But this is a dispute I do not wish to enter here.

Some scientific realists have held that prior probability considerations can supply the grounds for favoring one theory over another even when the theories are observationally equivalent. This strategy can by piggybacked by the universalists, but I wonder whether Bayesianism is the sort of piggy they want to back. The objectivist conception of prior probabilities remains nothing more than a collection of vague promissory notes. And the more popular subjective degree of belief conception seems to cut little philosophical ice for the case under discussion. Subjective degrees of belief can be assigned to hypotheses about relations among universals, but then they can also be assigned to hypotheses about anything you like — devils, angels, vital forces as well as electrons. Numerical representations of opinions may be helpful for certain purposes, but one expects more than mere representation of opinions from an account of the testing and confirmation of scientific theories.

More is to be found in Glymour's *Theory and Evidence* (1980). Glymour offers an objectivist account of qualitative confirmation which overcomes some of the more egregious flaws of the hypothetico-deductive view. His approach is essentially an extension of Hempel's idea that hypotheses are confirmed by deducing instances of them from evidence statements. Glymour's ingenious addition to this idea is a 'bootstrapping' operation by which instances of theoretical hypotheses are deduced from observational evidence with the assistance of auxiliary hypotheses drawn from the theory being tested.[17] I doubt that there is help to be found here for the universalists. If 'instances' of $N(F, G)$ are instances of $(x)(Fx \supset Gx)$, then Glymour's account can help to show how confirming instances are obtained from observational evidence when 'F' and 'G' denote properties that are not directly observable. But then what is being bootstrap confirmed is not $N(F, G)$ but its extensional counterpart. On the other hand, if 'instances' of $N(F, G)$ are instances of nomic necessitation, e.g., $N(F, G)$ (a's being F, a's being G),[18] then even when 'F' and 'G' denote directly observable properties, I do not see how Glymour's method can be used to confirm a theory with axioms like $N(F, G)$, at least not if, *pace* Armstrong, observational evidence comes in the Humean form Fa & Ga.

In sum, I can find no reason to share Tooley's optimistic conclusion that whatever account can be given for the grounds for accepting scientific theories in general will serve as well as an account of the grounds for accepting $N(F, G)$.

I can already hear the reply of Profs. Armstrong and Dretske; viz. it is not a matter of giving grounds for accepting $N(F, G)$; rather, relations among universals are presuppositions for induction and confirmation. But I contend that relations such as $N(F, G)$ are not presuppositions in the sense of conditions without which the wheels of confirmation would not turn.[19] If it is said that such relations are needed to make the machinery of confirmation intelligible, then we have reached an impasse. I can no more accept this standard of intelligibility than the ones set up by the Idealists and Rationalists.

In closing I want to make it plain that I do not suffer from one of those strange afflictions that make some of my colleagues hanker after desert landscapes or pant after particulars. I am fully convinced that universals occupy an important place in our ontology. And I reject Ramsey's gibe: "But may there not be something which might be called real connections of universals? I cannot deny it for I can understand nothing by such a phrase ..." (1978, p. 148). I *can* understand something by such a phrase, but my understanding of the use to which Armstrong, Dretske, and Tooley want to put it is incompatible with my understanding of empiricism.

13. CONCLUSION

What is missing in this chapter, and in most of the philosophical literature reviewed here, is the texture and feel of real-life laws. That is the sacrifice we made in attempting to abstract features common to all natural laws. The reader will have to judge for herself whether the results have justified the sacrifice.

I have attempted to obey Braithwaite's injunction to remain within the ambit of the constant conjunction view when giving the rationale for the distinction between uniformities due to natural laws and those which are merely cosmic accidents. The Attitudinal theorists despair of being able to satisfy this injunction, and in their despair they relocate the source of the distinction from the uniformities to our attitudes towards them. The Necessitarians and Universalists share this sense of despair, but rather than resort to human attitudes they appeal, in the former case, to irreducible *de re* modalities, and in the latter case, to contingent relations among universals. I argued on empiricist grounds that Necessitarian and Universals views are unacceptable. And I tried to show that while there is no easy path to the satisfaction of Braithwaite's injunction, neither is there sufficient reason to submit to the despair of the Attitudinalists.

A regularity analysis denies to laws of nature various forms of necessity that some philosophers claim for them. This denial in turn removes from determinism some of the sting Libertarians have felt; whether enough of the string is drawn to resolve the determinism-free will problem is an issue to be discussed in Ch. XII.

In previous chapters we have seen reasons for rejecting the notion that determinism is an *a priori* truth or an indispensible presupposition of scientific enquiry, but we have also seen that the force of determinism is not captured by saying that it is merely a high level empirical claim or a useful methodological guideline. The present chapter confirms the special and peculiar status of determinism; for while it is not essential to laws, it can and often does promote both strength and simplicity, the combination of which we took (following Mill) to be the essence of lawhood.

In Ch. III I suggested that determinism can be used as a probe for exploring the concepts of physical possibility and necessity. There is an obvious and innocuous sense in which this suggestion can be taken: namely, posit determinism and then see what presuppositions are needed to make it work; these presuppositions then become candidates for inscription on the list of natural laws. This suggestion involves no circularity if the standards for judging candidacy do not themselves presuppose determinism. And here we confront a difficulty: to the extent that the measures of strength, simplicity, coherence, etc. used in the M-R-L account of laws are not biased for or against determinism, they are not precise enough to cleanly decide some of the tough questions about determinism for Newtonian and relativistic physics. Is the deductive system of Newtonian mechanics with the boundary conditions at infinity needed to secure Laplacian determinism better than the system without these boundary conditions? Ditto for the boundary conditions needed to make the classical heat equation deterministic. Ditto for the entropy conditions needed to make the shock wave equation deterministic. Ditto for the prohibition against tachyons. Ditto for the conditions on the null cone structure needed to make general relativistic worlds deterministic (see Ch. X). My own answers are (in order): No; No; Yes; Don't know; No; with the overall tally going against determinism. But in every case I have to admit that my judgment is unstable and has more the feel of an esthetic judgment than a scientific judgment. And as in matters of esthetics, others give a different series of judgments, with their tallies often more in favor of determinism than mine. That the doctrine of Laplacian determinism has no firm truth value for Newtonian and classical relativistic physics is a conclusion some will find intolerable. Intolerable or not, the ambiguity is one we have to live with, at least until someone can fashion the tools to resolve it.

NOTES

[1] These are the versions of the constant conjunction and felt determination definitions Hume gives in the *Treatise*. The definitions are repeated with some significant changes in the *Enquiry*.

[2] This counterfactual definition does not appear in the first edition of the *Enquiry*.

[3] Here I am following Suchting (1974).

[4] See especially Secs. 13 and 15 of Bk. I of the *Treatise*.

[5] For a more detailed discussion of this point, see Suchting (1974) and Armstrong (1983).

[6] For an attempt to fill in some of the details, see Reichenbach (1954).

[7] Unlikely but not impossible since the net impressed force acting on a particle can be zero even when other particles are present. But the point is that we do not want the lawfulness of Newton's First Law to turn on such a happenstance.

[8] This last move would yield the stronger version of (E1); namely, if W_1 and W_2 agree on all occurrent facts, then they are the same world.

[9] See, for example, Braithwaite (1960), Berofsky (1968) and Tondl (1973); see Suchting (1974) for a critical discussion.

[10] It must be admitted that from this perspective Mill's rather labored treatment of Reid's famous day-night example is anomolous; see Mill (1904, pp. 244—247).

[11] The only finite frequentists I can cite are Russell (1948) and Sklar (1973).

[12] For a discussion of frequency and propensity theories and references to the literature, see Ch. VIII below.

[13] The truth value semantics for this type of approach have been worked out by Dunn (1973).

[14] This is one of the considerations that persuade Swoyer (1982) that laws are contingent only upon the exemplification of the universals.
[15] Hochberg (1981) has questioned the nature of this entailment relation. Armstrong's answer (1983, Ch. 6) seems satisfactory to me. What remains to be worked out is the formal semantics of the entailment relation; whether this can be done consistently with the constraints Armstrong places on universals remains to be seen.
[16] Roughly, just lop off all the terms which do not correspond to directly observable properties and relations.
[17] For Glymour the basic confirmation relation is three-place: evidence E confirms hypothesis H relative to theory T. We can say that E confirms T iff there is an axiomatization of T such that E confirms each axiom A relative to T. Glymour originally allowed the use of the hypothesis H itself as an auxiliary in deducing instances of H from E. But in later versions this feature has been dropped; see the articles by van Fraassen and Edidin in Earman (1983).
[18] Read: "a's being F necessitates a's being G in virtue of the universals F and G."
[19] Dretske (1977) says that lawfulness must be assumed for a general hypothesis H if examined instances which conform to H are to raise the probability that unexamined instances also conform to H. This I deny. If $Pr(H) \neq 0$, then it is a theorem of probability that as the number of examined instances conforming to H approaches infinity, the probability that any number of unexamined instances also conform to H approaches 1. Perhaps it may be claimed that it is unreasonable to set $Pr(H) \neq 0$ unless H is backed up by the appropriate relations among universals. This I also deny.

SUGGESTED READINGS FOR CHAPTER V

Chs. 4 ("Of Laws of Nature") and 5 ("Of the Law of Universal Causation") of Bk. III of Mill's (1904) *System of Logic* are the source of the modern regularity account of laws. Suchting's (1974) article "Regularity and Law" and the first part of Armstrong's (1983) book *What Is a Law of Nature*? detail the reasons why a growing number of philosophers have become disenchanted with the regularity analysis. The view that laws of nature are relations among universals is set out in the second half of Armstrong's book and in Dretske's (1977) "Laws of Nature" and Tooley's (1977) "The Nature of Laws." Skyrms' (1980) *Causal Necessity* offers a resiliency analysis of laws that can be traced back to Mill. Ayer's (1956) "What Is a Law of Nature?" and Goodman's (1955) *Fact, Fiction and Forecast* defend a felt determination definition of laws. Necessitarian accounts of laws are to be found in Ewing's (1974) *Idealism* and Kneale's (1949) *Probability and Induction.*

CHAPTER VI

DETERMINISM, MECHANISM, AND EFFECTIVE COMPUTABILITY

"Garbage in . . . garbage out"
"Yeah but is it computable garbage?"
(Graffiti from wall of Men's Room,
Experimental Engineering Bldg.,
University of Minnesota)

The examples of determinism studied in previous chapters should make it clear that determinism does not entail mechanism in the crude sense that determinism necessarily works by means of a mechanical contrivance composed of gears, levers, and pulleys. But it remains open that determinism involves mechanism in the more abstract sense that it works according to mechanical rules, whether or not these rules are embodied in mechanical devices. In the converse direction we can wonder whether mechanistic rules are necessarily deterministic. To make such questions amenable to discussion we need a model of mechanism and a codification of the rules by which the model works. I will take as the starting paradigm of mechanism the device which increasingly and irresistibly colors modern life — the digital computer. To understand the gist of operation of these devices it is best not to get too abstract too quickly, but to begin with the minimal embodiment described by Alan Turing in 1937.

1. TURING MACHINES

The inputs and outputs to a Turing machine are recorded on an infinite paper tape which is divided into squares. In each square one of three symbols, '0', '1', or 'B', appears. In its pristine state, before input, the tape is completely blank ('B' printed in each square). The machine 'scans' one square at a time and performs one of the following basic operations: it erases the symbol in the square it is currently scanning and prints one of the other symbols; it shifts one square to the left; it shifts one square to the right; or it puts up a flag and halts. For sake of definiteness, we can suppose that one basic operation is performed per second. The sequence of operations is governed by a finite list of