INTRODUCTION

Jan Faye
Paul Needham
Uwe Scheffler
and Max Urchs

1. Preamble

Descartes envisaged an infinite universe in which order could no longer be described, as Aristotle had done, with reference to a unique point at the centre, but depended entirely upon the idea of following universal laws. Newton accepted the general idea, but better appreciated the enormity of the task of actually formulating such laws having found wanting many of Descartes' arguments purporting to subsume phenomena under his mechanical principles. Newton's own attempts led him to a confront the a priori imposition of the principle of action by contact with an array of arguments subsuming a variety of phenomena under his law of gravitation. Because it was unrivalled by any equally articulated theory, the body of scientific opinion soon came to accept this principle of action at a distance, which must surely count as one of the most successful theories of modern science as measured by the time it has reigned unchallenged by any articulate alternative. This is an important point to bear in mind when considering Hume's regularity theory of causation.

Philosophers have also wrestled with the problem of how to articulate the general notion of a law of nature. Their efforts are likewise motivated by obscurities they perceive in certain conceptions, which they try to remedy with more or less detailed frameworks of their own. One leading idea lying at the heart of many subsequent treatments derives from Hume's analysis of causation. Even those who dispute the claims of this tradition must address the *regularity theory*, an exposition of which therefore provides a suitable starting point for this overview of theories of lawlikeness in modern philosophy.

2. The Regularity Theory of Causation

Intuitively, the concept of causation involves a kind of necessity. We say such things as, given the cause, the effect had to occur, and if it were not for the cause, the effect would not have occurred – expressions which suggest that the cause necessitates its effect. Hume opens his discussion by "observing that the terms *efficacy, agency, power, force, energy, necessary connexion*, and *productive quality*" – several of which were used by Newton – "are all nearly synonymous" (Treatise 1739, I.III.xiv)[1]. No one of them can therefore be used to explain any of the others, and he goes on to consider what independent sense can be made of this notion of necessary connection. The first point he makes is that it cannot be logical necessity – it cannot, as he puts it, be a matter of a "relation of ideas". For the idea of the effect is distinct from the idea of the cause, and reason alone is not sufficient to obtain the idea of the effect from the idea of the cause – "Reason alone can never give rise to any original idea" (Treatise 1739, I.III.xiv). In more modern jargon, Hume's first point was that given a description of the cause, a description of the effect does not follow logically. Accordingly, whatever the connection between cause and effect, it is not an analytic truth that the particular event which happens to be the effect follows the particular event which happens to be the cause. The existence of a causal relation is a substantial fact about the world, and statements of causal connections are synthetic truths.

Having denied that causal connections involve analytic necessity, David Hume's second point is to deny that the causal relation is a necessary relation at all. He argues that if we consider what we see when one billiard ball collides with and causes another to move, all we observe is the movement of the one ball and then the movement of the other. There is no connection to be seen, only the cause and the effect. "All ideas are deriv'd from, and represent impressions. We never have any impression, that contains any power or efficacy. We never therefore have any idea of power" (Treatise 1739, I.III.xiv). He concludes that since there is no such connection to be seen, there is no necessary connection at all.

There is an air of paradox in this. We began by talking about the kind of causal connection we express by saying that, given the cause, the effect had to occur, and conclude that no such connection is observable. But if this putative connection is not observable, how could we recognise and

[1]This manner of referring to Book I, Chapter III, Section xiv of Hume's *Treatise of Human Nature* follows a standard format, independent of the particular edition used here (for which, see the bibliography).

discuss it in the first place? There must be something distinguishable about causally related events since some, but not all, pairs of events are causally related. And if it is not the connection that distinguishes them, what does? At the very least we must be able to explain how the original question of the nature of the causal necessity arose. Hume certainly didn't want to deny the distinction between causally related and non-related events, and the theory he elaborated addressed these questions. Specifically, he distinguished two problems: (i) How is our conviction that the effect *must*, so it seems, follow the cause to be explained? and (ii) What does distinguish causally connected events from others?

Hume answers the first question with the claim that

> Necessity is something that exists in the mind, not in objects (Treatise 1739, I.III.xiv).

It is a psychological peculiarity of our minds that we develop a tendency to associate a certain kind of event with events like the cause, and the expectation prompted by the cause leaves us with the impression of a necessary connection which we mistakenly read into nature. This explanation relies on the propensity of one belief to give rise to another, and clearly presupposes the notion of causation at issue. If causation of this sort were available to us for inspection, it would undermine Hume's initial critique. But as Stroud says, "Hume does not think that we actually perceive the necessity of the connection between any two events, even events that occur in our minds. If we did, then we could get the idea of necessity directly from one of our internal experiences" (Stroud, 1977, p. 84). Our impression of necessity is just a feeling that arises in the mind when one mental event causes another; it is not an impression of a causal connection between two events. The analysis which Hume provides in his answer to the second of his questions would be redundant if this were not the case.

A concise summary of his answer to the second question is given by the following definition:

> We may define a cause to be "An object precedent and contiguous to another, and where all objects resembling the former are placed in like relations of precedency and contiguity to those objects that resemble the latter" (Treatise 1739, I.III.xiv).

The nub of Hume's analysis is what he calls the "constant conjunction" of events which fall into kinds of mutually resembling objects. Objects of each kind are paired off with one another in virtue of their *conjunction* – a term which is to be understood as the astronomer, rather than the logician, uses it. The modern logician wants to reserve the term "conjunction" for a sentential connective, and would use some other expression, say "occurring together", for the relation between objects involved in causation. Now, without some such relation of occurring together, the mere resemblance of events would leave us with the unhelpful statement that events of one kind occur and events of another kind occur. But Hume well understood that his notion of constant conjunction involves some "relation among objects" pairing off events:

> The idea, then, of causation must be deriv'd from some *relation* among objects; and that relation we must now endeavour to discover. I find in the first place, that whatever objects are consider'd as causes and effects, are *contiguous*; and that nothing can operate in a time or place, which is ever so little remov'd from those of its existence. Tho' distant objects may sometimes seem productive of each other, they are commonly found upon examination to be link'd by a chain of causes, which are contiguous among themselves, and to the distant objects; and when in any particular instance we cannot discover this connexion, we still presume it to exist. We may therefore consider the relation of CONTIGUITY as essential to that of causation; at least may suppose it as such, according to the general opinion, till we can find a more [fn. referring to the later section I.IV.v] proper occasion to clear up this matter, by examining what objects are or are not susceptible of juxtaposition and conjunction. (Treatise 1739, I.IV.ii)

A distinction is drawn here between mediate and immediate causation, and the contiguity requirement applies to the latter. Distant causation is allowed on the condition that there is a chain of events, in which each is an immediate cause of the next, linking distant cause and effect. A more general notion of mediate causation can thus be defined once a concept of immediate causation involving the contiguity constraint is available, and so the latter remains the basic notion which Hume is concerned to delimit in his original definition.

The contiguity requirement can be motivated with reference to examples like the famous one of Russell's in which a whistle blows every weekday in the afternoon in a certain London factory and workers pour out of a Manchester factory a few minutes later. It would be absurd to suggest that a whistle they couldn't even hear caused the Manchester workers to down tools for the day, and there is no suggestion of an intermediate chain of events mediating a link between London and Manchester. Causes are accordingly restricted to events near their effects. Roughly, the general idea is that there are regularities between events of various kinds which we wouldn't want to say are causally connected, and the contiguity requirement is called upon to eliminate such coincidences. Note that the same argument would apply against a whistle's blowing yesterday in Manchester as the cause of the workers leaving the Manchester factory today, so the contiguity includes both spatial and temporal nearness.

Whatever might be said in support of the reasonableness of appealing to contiguity, however, it shouldn't be forgotten that the constant conjunction account *requires* some analysis of the conjunction, or occurring together, relation. Given a relation of occurring together defined between particular events, a relation of constant conjunction can be defined between kinds of events X and Y by saying that each event of kind X stands in a relation of occurring together with an event of kind Y. How is this relation of occurring together to be defined? If the analysis is not to be circular, or uninformative by reason of relying on a concept Hume regards as synonymous with causation, it cannot be explained in terms of causal connection, but must be explicitly defined in non-synonymous terms. Hume thought a definition satisfying these requirements was available in terms of the spatial and temporal contiguity of events, which can be motivated along the lines indicated above. Should this motivation be found wanting, some other account of the "occurring together" relation must be provided unless the whole framework of the analysis is to be radically rethought.

Hume did express doubts about the contiguity condition. In section I.IV.v of the *Treatise* he considers that "Thought . . . and extension are qualities wholly incompatible", and worries about "the soul['s] . . . *local conjunction* with matter", which leads him to wonder whether "it may not be improper to consider in general what objects are, or are not susceptible of a local conjunction" (pp. 234–235). The absurdities of "endeavouring to bestow a place on what is utterly incapable of it" (p. 238) may have convinced Hume that "our perceptions are not susceptible of a local union". But he is not then at liberty to conclude that "as the constant conjunction of objects constitutes the very essence of cause and effect, matter and motion may often be regarded as the causes of thought, as far as we have any notion of that relation" (p. 250). For the *relation* of constant conjunction depends upon a relation of occurring together, and the only interpretation of this Hume has offered is one in terms of contiguity.

A further element is required, for as it stands, this analysis gives no account of the asymmetry of the causal relation, i.e. it makes no distinction between cause and effect, since the contiguity relations are symmetric. Asymmetry is introduced with the requirement that the cause precedes the effect in time. Putting these pieces together, then, the regularity theory defines the causal relation as follows:

x causes y if and only if x is of some kind X and y of some kind Y which are

(a) constantly conjoined and y is the Y-event occurring together with x (an X-event), and

(b) the X events uniformly precede the Y events with which they occur together.

This definition provides Hume with an answer to his second question of what distinguishes causally related events without the need to appeal to any notion of causal power, efficacy or necessity. No features are mistakenly attributed to nature which are in reality only features of our psychology.

3. Some Difficulties with Hume's Analysis

Objections have been raised against the regularity theory, as Hume's theory is often called, ranging from technical details concerned with including and excluding exactly the right cases to general matters of principle. Sometimes technical problems raise matters of principle. Not all regularities, as we have seen, provide cases of causally connected events, and the question arises whether the spatio-temporal contiguity constraint suffices to preclude such counterexamples. Thomas Read pointed out that night invariably follows day.[2] This is perhaps best seen not so much as a counterexample to the sufficiency of Hume's condition as a reminder that the definition specifies causation as a relation between events with clearly defined spatial and temporal boundaries – objects "susceptible of juxtaposition". "Night" and "day" don't refer to any such thing, but merely indicate general conditions, and whatever kind of regularity they give rise to, it isn't a constant conjunction as defined and so not a case in which the *definiens* is satisfied but not the *definiendum.* Responding to Read's example in this way emphasises an ontological presupposition of Hume's own version of his regularity theory, namely the existence of entities of a certain kind with definite spatio-temporal boundaries.

A second challenge to the sufficiency of the condition is provided by examples in which several distinct events are regularly produced by a single common cause. Symptoms of diseases are often of this kind, for

[2] "But no reasoning is more fallacious than this – that, because two things are always conjoined, therefore one must be the cause of the other. Day and night have been joined in a constant succession since the beginning of the world; but who is so foolish as to conclude from this that day is the cause of night, or night the cause of the following day?" (Read, 1785, Ch. IV, p. 253).

example the appearance of swellings under the cheeks followed by severe headache and rise in temperature which are all symptoms of mumps. Again, a fall in pressure causes a swing in the barometer needle and a subsequent storm. Reichenbach (1928) discussed the problem of distinguishing causally connected processes from what he called "unreal sequences" in which events in a spatio-temporally continuous series are not causally linked such that earlier ones cause the immediately succeeding ones; for example, when the beam from a torch (or rather a laser) is swung so that the image falling on a distant object moves across the surface at a speed greater than that of light. The trajectory of the image is continuous, but not causally linked. The common effects are constantly conjoined with one preceding the other, and so satisfy the two conditions in Hume's definition; but the earlier one is not the cause of the later.

Reichenbach's solution is to repeat the process with a slight variation in the cause – more specifically, in the kind of event featuring as cause. The variation should not be so much as to constitute an essentially different kind of event, but just sufficient to "mark" events as variations of the putative cause kind. If it really is the cause kind that is thus marked, a concomitant variation will be noticed in the effect kind, and the mark may be said to be transmitted from cause to effect. No such transmission is observed if the event kind marked corresponds to one of two effects of a common cause.

Another, albeit less illuminating, approach is to adopt a similar manoeuvre to that involved in extending the basic concept of immediate causation to cover mediate causation. There the strategy was to extend the original definition by adding a disjunct, thus

> x causes y if and only if *either* x is the immediate cause of y *or* there are events $z_1, \ldots, z_n$ such that x is the immediate cause of z_1 and z_1 is the immediate cause of z_2 and ... and z_n is the immediate cause of y.

In the present case, however, the point of the added clause is to revise the concept just defined by restricting rather than extending it, giving us yet a new concept conveniently called strict causation:

> x strictly causes y if and only if x causes y *and* there is no event z such that z causes x and z causes y.

An objection to the effect that the Humean definition doesn't provide a necessary condition for the causal connection of events might be motivated along the following lines. A paradigm case of causation is the switching on of the light in a room by pressing the switch on the wall. Now we are all familiar with the situation where, on a specific occasion, the switch is pressed but the light doesn't come on. The explanation might be simple – perhaps the tungsten filament in the light bulb has burnt out. Nevertheless, the condition of the regularity theory is broken; there is no constant conjunction between turnings of the switch and lightings of the light. Two lines of reply are available to the regularity theorist. First, he might say that the objection builds on an unwarrantably strict interpretation of the theory. When we say that such-and-such causes so-and-so, it is understood that other factors are operative in addition to those explicitly mentioned as the cause. In the case of the pressing of the switch, these would include the assumption that the mechanics of the switch are in order, that the wiring is in reasonable condition, that the bulb is in working order, etc., where explicit descriptions can be provided of what "being in order" presupposes in each case. In other words, the cause as actually described is not itself sufficient for the effect, but is rather a non-redundant part of a set of relevant conditions which are jointly sufficient for the effect.

This first line of reply is not satisfactory as it stands, however. By Hume's own argument, the notion of sufficiency at issue in causation is not a logical or analytic one. It involves all the relevant factors being present, the absence of one or more rendering the cause insufficient. But of all the states of affairs obtaining in the world, which are to be counted as relevant and which not? If Hume's argument against any special notion of necessary connection holds water, then by parity of reasoning it also applies to the circumstance of being relevant. The fact that one state of affairs is relevant to the explicitly mentioned feature as a part of the total cause is just as unobservable as the original necessary connection. It looks like this first line of reply invokes a notion of relevance which, if not synonymous with necessary connection, is of much the same kind, and is at odds with the spirit of the regularity analysis. This point was further emphasised by Nelson Goodman in his discussion of counterfactual conditionals (Goodman, 1947).

This first line of reply errs, it might be thought, in accepting too much of the presuppositions of the objection. A second line of reply insists on the relational analysis of causation, the terms "cause" and "effect" referring to definite entities called events which exist in space and time even if they don't persist over time as do ordinary physical objects like tables and chairs. Now there is no question of an ordinary description of a persisting physical object – say "that man standing in front of the shop window" – mentioning all, or even an appreciable number, of the properties actually possessed by the object in question. Similarly, the fact that the actual description of the event said to cause another in a singular causal statement only mentions some feature of the event doesn't detract from the fact that the event actually has innumerable other features, some of which are necessary for the bringing about of the effect. Davidson – a well-known proponent of events in the sense required for this second line of reply – answers Mill's objection that a fall cannot be the cause of death because the circumstance of his weight must be included in the cause with the comment that

> . . . if it was Smith's fall that killed him, and Smith weighed twelve stone, then Smith's fall was the fall of a man who weighed twelve stone, whether or not we know it or mention it. (Davidson, 1967, p. 150)

This second line of reply emphasises the difference between taking the notion of an event seriously and merely using it as a *façon de parler* without commitment to the existence of entities it ostensibly refers to. Probably Hume wasn't himself so sure on this matter. For if he was, he wouldn't have begun his critique of the concept of causal necessity by first insisting that no logical connection could be involved between cause and effect. Davidson (1963) points out that logical connections are sustained between descriptions of things, and it is perfectly possible on the relational view of causation that the description of the cause entails the existence of the effect, for example in "The cause of fire caused the fire". Identity, on the other hand, is a relation sustained by the entities themselves, and Davidson emphasises that the cause is always distinct from (i.e. non-identical with) the effect, even when a description of the one entails the existence of the other.

Objections might still be raised. Kant suggested that the causing of a depression in a pillow by a heavy object provides an example of simultaneous causation. A similar example is the rotation of a ball on a string causing a tension in and elongation of the string. Statics provides examples where no motion is involved: a pillar standing in the appropriate position holds up a bridge; a person leaning against a spring-door holds it open; and so on.[3] If these counterexamples are allowed and temporal precedence abandoned, another criterion of causal priority – what distinguishes cause from effect and renders the causal relation asymmetric – is needed. Reichenbach (1928) thought this was needed in any case because he wanted to carry through Leibniz's idea of reducing temporal order to causal order. Leibniz (1715, pp. 201–202) doesn't seem to realise that if "earlier than" is to be defined in terms of "causes" without circularity, the asymmetry of the latter must be given an independent explanation. The solution Reichenbach proposed

[3]Irreversible thermodynamics provides further examples. "For certain systems the fluxes at a given instant depend only on the values of the affinities at that instant. Such systems are referred to as 'purely resistive'. . . . a very large fraction of the systems of interest . . . are purely resistive." (Callen, 1985, p. 312).

– the same as his solution to the common cause problem – has not gone without criticism; but some philosophers believe that he was right in thinking that causal priority should be accounted for independently of temporal precedence.

The examples from statics might be construed as counterexamples of a kind which are, *prima facie*, singular causal statements, but which are difficult to cast in the relational mould. Leaning against a door is a state a person is in rather than an event in which he participates, and it seems far-fetched to represent the proposed causal situation as a relation between two events. Opinions may differ on the appropriate analysis of putative counterexamples of this kind; but one source of examples is particularly embarrassing for the relational theorist. Consider

(1) The fact that the earth and moon have such and such masses and stand at such and such a distance causes it to be the case that they revolve around a common centre of gravity,

which is one way of expressing Newton's thesis that gravitational force causes the moon's orbital motion. Newton speculated on an abundance of forces in nature by which objects attract and repel one another, and interact chemically, although he only succeeded in formulating the gravitational force. As already mentioned, the intelligibility of gravitation was hotly contended on the grounds that it involved action at a distance. Nevertheless, the law survived the imputations of the Cartesians as the only serious contender in the field, and determined the pattern of development of natural science from the eighteenth century on. More than two hundred years were to elapse before the first coherent alternative was formulated; and even then, Newtonian theory retains a position other scientific theories can hardly rival in their own spheres of application. Yet here we have the spectacle of a philosophical theory of causation, supposedly built upon the foundations of observation and recognising no non-analytic *a priori* truths, which doesn't seem able to accommodate gravitational attraction. Even if (1) could somehow be cajoled into relational form by some appropriate construal of events (we haven't yet been provided with any clear criteria for identifying and distinguishing events beyond Hume's directive that they be "susceptible of juxtaposition"), the analysis precludes cause and effect being simultaneous. But Newton's law requires that the orbital motion caused by the gravitational force occur when the force operates; if mass were miraculously divested of its attractive power, the moon would instantaneously fly off its orbit at a tangent. Surely no adequate *concept* of causation can reasonably conflict with scientific theory of any sort, let alone one of the most successful theories of them all.

Newton's contemporary Leibniz opposed the idea of action at a distance with another proposal, the apparent action being in reality the occurrence of simultaneous events in distant bodies standing in preestablished harmony. This doesn't call upon action by contact to render the effects intelligible, and as Duhem (Duhem, 1893, pp. 125ff.) points out, scientists after Leibniz generally agreed that the action of one body on another remains as much in need of explanation when bodies are in contact as when they are not, even if they saw little point in denying the reality of the action in favour of preestablished harmony. More modern views treat the contact of two bodies in collision as a macroscopic description which is not preserved at the microscopic level, where there is no question of massive bodies literally touching one another.

The requirement of contiguity has not always been preserved by empiricist-minded philosophers who have sought to retain what they regard as the essential insight of Hume's regularity account, namely the avoidance of modal concepts in the analysis of the concept of a general law, without the specific ontological commitment to events. The following section describes the analysis developed by Hans Reichenbach towards the end of his life. It is representative of the kind of extensional analysis typical of the logical positivists.

4. Reichenbach's Analysis of Laws

Reichenbach outlined a fairly sophisticated view of lawlikeness in *Elements of Symbolic Logic* (1947) which, after a certain amount of criticism, he adjusted and developed in considerably more detail in his posthumously published *Nomological Statements and Admissible Operations* (1954). The latter book received an unfavourable review on publication by Hempel (1955). But in a general review of the problem of law, Jobe (1967) distinguished Reichenbach's theory as by far the best then available. Reichenbach lays down a number of formal conditions for lawlikeness. A simplified presentation the sort of thing involved is given in the next section. But he recognised that formal conditions are not sufficient, and elaborated a further notion of verifiability, which is discussed first in this section.

The first requirement Reichenbach lays down is a non-formal one, that a fundamental law be *verifiably true*, which he explains as meaning "it is verified as practically true at some time during the past, present or future history of mankind" (Reichenbach, 1954, Def. 1, p. 18). Reichenbach was clearly concerned to eliminate the modal character of an undefined dispositional term "verifi*able*" by explaining it in terms of being confirmed at some time – past, present or future. But these words have led to some unfortunate misunderstanding of Reichenbach's intentions – an unhappy fate for the first major concept of his book.

The emphasis on confirmation led several critics to interpret him as saying that a law is merely confirmed to a high degree *rather than* being true. So Hempel read him in his review, and Carnap followed suit:

> My friends argued that they would prefer to say, instead of "true", "confirmed to a high degree". Reichenbach, in his book *Nomological Statements and Admissible Operations* ... comes to the same conclusion, although in different terminology. By "true" he means "well established" or "highly confirmed" on the basis of available evidence at some time in the past, present or future. But this is not, I suspect, what scientists *mean* when they speak of a basic law of nature. By "basic law", they mean something that holds in nature regardless of whether any human being is aware of it. (Carnap, 1966, p. 213)

Although there is an important point in the last sentence of this passage, the initial part interprets Reichenbach incorrectly. Reichenbach actually says in his introduction that

> Being laws of nature, nomological statements, of course, must be true; they must even be verifiably true, which is a stronger requirement than truth alone. (Reichenbach, 1954, p. 11)

It couldn't be more clearly put that truth is part of what is involved in a law of nature. The point is driven firmly home by Jobe, 1967, pp. 374–375, and to avoid any misunderstanding the expression "verifiable and true" is used here instead of Reichenbach's "verifiably true".

Carnap sought to characterise laws by trying to distinguish conditions on logical form necessary for laws of nature, and counting as basic laws those statements which fulfil the conditions on logical form and are true. Lawlikeness, in other words, is conceived as a purely formal matter. "The problem of defining 'basic law' has nothing to do with the degree to which a law has been confirmed ... the problem is only concerned with the meaning that is intended when the concept is used in discourse by scientists" (Carnap, 1966, p. 213). Reichenbach's approach is fundamentally different from this. Taken in isolation, formal conditions are not sufficient to guarantee "generality in a reasonable sense", which Reichenbach thought must be broad enough to include a notion of inductive extension. For accidental generalisations

> may obtain even if no reference to individual space-time regions is made; for instance, the statement "all gold cubes are smaller than one cubic mile", may possibly be true. (Reichenbach, 1954, p. 11)

Thus, "All the coins in my pocket are silver" is not universal in Reichenbach's sense of containing no individual term "defined with reference to a certain space-time region, or which can be so defined without change of meaning" (Reichenbach, 1954, Def. 24, p. 32), and so disqualified as a law. But "All gold cubes are smaller than one cubic mile" is universal, and satisfies his other formal conditions too, yet is not a law. What it lacks, in Reichenbach's view, is inductive generality:

> when we reject a statement of this kind as not expressing a law of nature, we mean to say that observable facts do not require any such statement for their interpretation and thus do not confer any truth, or any degree of probability, on it. If they did, if we had good inductive evidence for the statement, we would be willing to accept it. For instance, "all signals are slower than or equally fast as light signals", is accepted as a law of nature because observable facts confer a high probability upon it. It is inductive verification, not mere truth, which makes an all-statement a law of nature. In fact, if we could prove that gold cubes of giant size would condense under gravitational pressure into a sun-like ball whose atoms were all disintegrated, we would be willing also to accept the statement about gold cubes among the laws of nature. (Reichenbach, 1954, pp. 11-12)

What, then, does Reichenbach put into this notion of verifiability which he thinks so important? A high probability, for one thing; but that is not sufficient. Reichenbach formulates other conditions on the confirmation of an all-statement $\forall x(Px \supset Qx)$ in terms of the conditional probability of something's being Q given it is P. The formal conditions preclude vacuous antecedents, and the exclusion of reference to specific spatio-temporal regions precludes verification by only a small number of P's. The class of P's must be *open* in the sense that there are many more of them than those which happen to have been examined for the property Q.

Reichenbach requires that not only the conditional probability of something's being Q given it is P must be high. The conditional probability of something's being $\sim P$ given that it is $\sim Q$, $p(\sim P/\sim Q)$, must also be high. The one doesn't necessarily follow from the other, and so the requirement is a substantive one. Reichenbach shows that the deviation, d, from 1 of $p(Q/P)$ (i.e., $p(Q/P) = 1 - d$) is related to the deviation, d', of $p(\sim P/\sim Q)$ from 1 by

$$\frac{d}{d'} = \frac{1 - p(Q)}{p(P)}$$

For example, the probability of "Houses are red" is not high, although the probability that something not red is not a house is high. Since the probability of not being red, $1 - p(Q)$, is high, and that of being a house, $p(P)$, is low, the ratio d/d' is high. A high probability of the conditional probability directly related to the contraposition of a hypothesis is therefore compatible with a low conditional probability directly related to the hypothesis originally formulated. This, Reichenbach claims, supplies an answer to Hempel's paradox of confirmation (Hempel, 1965), explaining why what confirms "All non-Qs are non-Ps" doesn't confirm "All Ps are Qs".

It also serves, he claims, to block "All gold cubes are smaller than one cubic mile", which doesn't satisfy the condition because there is very little direct evidence to support the contrapositive "Anything at least one cubic mile in volume is not gold" – i.e. there are no grounds for assigning $p(\sim$ gold / $\sim$ less than one cubic mile in volume) a high value. It has a low probability because we can quite easily imagine such a large piece of gold; no facts seem to count against this possibility. Compare "All objects made by man are under 400 meters high", which can't be assigned a high probability because it might well not be true for all time. On the other hand, the probability of an object over 400 meters high not being man-made is high – the observation of high mountains, for example, provides strong support – but this doesn't influence the low estimate of the probability of the original statement, which is therefore not lawlike either. It is a moot point, however, whether the interpretation of probability here is consistent with the general restriction to an extensionalist account.

Finally, a high value of $p(Q/P)$ provides no guarantee that there are no exceptions to "All Ps are Qs". "It would be too strong a condition to require that *there be* no exceptions", Reichenbach says. "In some sense, there exists general evidence that exceptions will occur, because too many laws of physics have later turned out to be merely approximately true. But we must be unable to describe conditions upon which an exception ... could be expected. In other words, there should be no *specific evidence* that the general implication considered is subject to exceptions" (Reichenbach, 1954, p. 132). Accordingly, a condition to the effect that there is no evidence of a property C such that the degree of confirmation of $\forall x((Px \wedge Cx) \supset Qx)$ is less than that of $\forall x(Px \supset Qx)$ is imposed. There should, in fact, be evidence that there is no such property C. Thus, the probability that swans are white is high, but some swans in Australia were found to be black. The probability that something is white given that it is an Australian swan is less that it would be given that it is simply a swan. Despite the fact that $\forall x(Px \supset Qx)$ logically implies $\forall x((Px \wedge Cx) \supset Qx)$ might be less than $p(Q/P)$ and independent evidence must be considered to establish just what is the case. (Intuitively, it might be thought that whenever A confirms B and B implies C, then A confirms C since C would seem to say no more than anything implying it. But this principle, though not without some *prima facie* plausibility, is evidently denied by Reichenbach.)

5. Reichenbach's Formal Conditions on Lawlikeness

The notion of verifiability complements a set of purely formal conditions on lawlikeness in Reichenbach's account. Here his strategy is first to define a set of *original nomological statements*, and then to define the *derived nomological statements* and finally, *relative nomological statements*. Statements of the first kind are intended to be the fundamental laws, including the laws of logic. Derived nomological statements are, broadly speaking, logical consequences of the set of original nomological statements, and relative nomological statements are consequences of original or derived nomological statements together with particular factual information. The law that the period of a pendulum is given by $2\pi\sqrt{l/g}$, where l is the pendulum's length and g the acceleration due to gravity, for example, is derived from laws together with factual information about which particular forces are acting.

In order to ensure "generality in a reasonable sense", original nomological statements satisfy a number of conditions. First and foremost, they must be generalisations – *all-statements* – which means that, when reduced to prenex normal form (so that all the quantifiers occur at the front of the formula), there is at least one universal quantifier. But this is not enough, and Reichenbach imposes further conditions. Fundamental laws must be universal, which means that an original nomological statement contains no individual term "defined with reference to a certain space-time region, or which can be so defined without change of meaning" (Reichenbach, 1954, Def. 24, p. 32). But there are several cases of statements that have been counted as laws which do make specific reference to specific bodies, and therefore to specific space-time regions – Galileo's law of free fall and Kepler's laws. Moreover, Goodman (1947) points out that it is always possible to express what these laws say in terms of sentences built up from predicates whose syntactic form gives no indication of reference to specific bodies. Instead of talking about bodies on the surface of the earth, for example, we could talk of terrestrial bodies.[4] Furthermore, general statements which don't make reference to objects can be equivalently expressed in ways that do; for example, by conjoining a tautology "John's hair is blond or it is not".

Regarding the first objection, laws such as Kepler's which deal with planets in the solar system are not nomological statements according to

[4]Ramsey made this point in notes written in 1928 (but first published in 1978). "If we put in enough detail", he says, "we shall (unless the world repeats itself endlessly with just a few details different each time) get a true generalization which mentions no particular portion of space-time but this would not be a law of nature" (Ramsey, 1978, pp. 130–131).

Reichenbach, but only relative nomological statements. A statement is relative to some matter of fact p if it is deductively derivable from p and a nomological statement, s. Kepler's laws are derivable from Newton's laws, which are universal, together with certain information about the relative masses and velocities of the planets and the sun. Against this, Nagel (1961) argues that if non-lawlike premises are allowed to figure in the derivation of laws from others, then we must face the consequence that a statement such as "All the screws in Smith's car are rusty" is lawlike, since it is presumably a law that iron screws rust when exposed to oxygen, etc., and this conjoined with the additional premises that all the screws in Smith's car are iron and have been exposed to oxygen, etc., implies the unwanted conclusion. But Jobe counters that logically consistency appears to be on the side of the explication. For a law is not the sort of thing that can be falsified by an accident, and yet

> . . . a mere accident in the form of, say, a near approach or collision with an interstellar body could impart to one or more of the planets velocities such that, contrary to Kepler's first law, they would pursue hyperbolic rather than elliptical orbits with respect to the sun. (Jobe, 1967, p. 380)

Nevertheless, Kepler's laws are nomological statements relative to conditions of great stability and permanence, at any rate on a human time scale, whereas "All the screws in Smith's car are rusty" is nomological relative to quite ephemeral circumstances. This is quite sufficient to mark a difference between the two cases. A similar tale can be told about Galileo's law, which depends upon the earth's being a sphere so that all its mass can be regarded as being concentrated at its centre. If the rate of rotation of the earth were greater than it is, the deviation from a sphere would be greater than its present negligibly small deformity.

On the second point, Reichenbach is aware that reference to specific bodies or regions must be interpreted as an essential feature of accidental generalisation but an inessential feature of laws. Accordingly, the test of lawlikeness must be whether there is an appropriate formulation which does not make specific reference to particular bodies or regions, rather than that all equivalent formulations lack such reference. He therefore defines a synthetic statement as universal if "it cannot be written in a reduced form which contains an individual term" (Reichenbach, 1954, Def. 25, p. 33). The notion of a reduced form is explained in a series of definitions, the effect of which is to eliminate redundant parts and to contract a statement to a shorter equivalent form. Thus, "Peter weighs as much as Paul" is discounted as a fundamental law, although it is equivalent to the general statement "Everyone weighs as much as Peter if and only if he weighs as much as Paul". But Reichenbach recognises that other counterexamples are forthcoming unless laws are defined only for a language which, as he puts it, can reasonably be regarded as scientific. The following passage illustrates the sort of restriction Reichenbach had in mind.

> A certain ambiguity arises because a natural language is often capable of different rational reconstructions. The term "polar bear", for instance, can be interpreted as meaning a bear living in the polar regions of the Earth, in which interpretation it would be an individual-term. It could also be defined as a biological species with certain general characteristics, for instance, as a bear with a white skin, etc. In such cases, we have two rational reconstructions which are not logically equivalent, though perhaps practically equivalent. As a consequence, a statement which in one rational reconstruction is nomological, may not be so in another reconstruction of conversational language. This ambiguity, however, offers no difficulties, since the class of nomological statements is defined only for a certain reconstruction of language. If a statement of conversational language is given, it would be meaningless to ask: is this statement really nomological? There is no such thing as an absolute meaning of the terms of a natural language. A classification of statements such as expressed in categories like "analytic" or "nomological" refers to a given rational reconstruction of language. Whether this reconstruction is adequate, is to be investigated separately. (Reichenbach, 1954, p. 34)

A further condition Reichenbach imposes is that of *exhaustiveness*. This is intended to rule out vacuously true statements such as "All unicorns are pink", which have antecedents not true of anything, but also other types of vacuousness. For example, consider

(2) $\forall x \exists y (Fxy \supset Txy)$

where Fxy means "x is the father of y" and Txy means "x is taller than y". Fxy is not always false (for every value of x and y), but it is true that

(3) $\forall x \exists y {\sim} Fxy$

and so (2) is vacuously true.

The requirement works along the following lines. The quantifiers are assumed to have been moved to the front of the formula. A statement's being *exhaustive in major terms* is defined first, for which purpose the main binary truth functional operator is expanded in disjunctive normal form. $\forall x(Ux \supset Px)$, for example, ("All unicorns are pink"), which already has its quantifiers at the front, has a major truth functional operator "$\supset$" and can be expanded thus:

(4) $\forall x((Ux \wedge Px) \vee ({\sim}Ux \wedge Px) \vee ({\sim}Ux \wedge {\sim}Px))$.

A *residual in major terms* is then defined as any statement obtained from the expanded form (i.e. (4) in the case of the present example) by removing one or more of the disjuncts. A statement is then said to be *exhaustive in its major terms* if none of its residuals in major terms is true and verifiable. The residual

(5) $\forall x((\sim Ux \wedge Px) \vee (\sim Ux \wedge \sim Px))$

of (3), for example, is true and verifiable since there are no unicorns, and therefore $\forall x(Ux \supset Px)$ is not a law. A further notion of being *exhaustive in elementary terms* is then defined along the same lines, except that the reduction to disjunctive normal form is carried through for all connectives and not just the principal binary operator. Finally, a statement is said to be *exhaustive* if it is exhaustive in both major and minor terms. The effect of this requirement is that a law cannot say too little; if a stronger statement can be made, then the weaker statement obtained by adding an extra, vacuous disjunct cannot be called a law.

Against this procedure it might be objected that some of the most fundamental laws of nature are in fact vacuous, dealing with ideals of natural order rather than the real thing. The Charles-Boyle, or ideal, gas law $PV = nRT$, for example, is appropriate for ideal gases which real gases approach under low pressure and high temperatures. Raoult's law, stating that the vapour pressure of a solvent over a solution is reduced from the vapour pressure of the pure solvent in proportion to the mole fraction of solvent in solution, deals with ideal solutions, which like ideal gases, may be approached in certain limiting cases. The Hardy-Weinberg law states that the gene ratios in a population remain constant over the generations in the absence of any influences favouring the selection of particular genetic characteristics. But Darwin's principle of natural selection denies that populations remain undisturbed. Newton's first law of motion concerns bodies not acted upon by any external forces; but given his law of gravitation, there are no such bodies. Laws such as these seem to be about ideal entities rather than the real objects over which the quantifiers in Reichenbach's explication presumably range. Doesn't the exhaustiveness requirement make the extensional analysis too idealistic an account of scientific law?

It has been maintained that appeal to picturesque idealisations is not essential, however, and the function of these laws is certainly in the explanation and prediction of the behaviour of real objects. Newton's first law, for example, can be expressed in the form "For any body x, if no resultant force acts on x, then it is at rest or in uniform motion (relative to the fixed stars)". A body is subject to a resultant force if all the forces acting on it are such that they don't cancel one another completely and there is a net overall force in a certain direction. Arthur Pap (1958) objected to this reformulation that the law is actually used in its vacuous formulation, for example when calculating the tangential velocity of a body moving under the influence of a central force. The tangential velocity at a given instant is the velocity the body would have if the central force were removed at this instant and the body continued in a straight line with its inertial movement. This counterfactual would not follow, Pap maintains, from the suggested reformulation, but only from the vacuous statement of the law. But, as Jobe (Jobe, 1967, p. 379) points out, this argument is based on a fallacy that being acted on by no force, as the antecedent of the counterfactual states, is being acted on by no resultant force. Perhaps Pap thought "acted on by no resultant force" defines a subset of the bodies acted on by no forces, rather than vice verse, because "acted on by a resultant force" defines a subset of the bodies acted on by a force. However that may be, it seems that vacuous formulations can be dispensed with and the exhaustiveness requirement upheld. (In more recent times, Nancy Cartwright (1983) has presented a different case for the vacuousness of fundamental laws.)

Reichenbach goes on to extend the concept of exhaustiveness to deal with another problem, closely connected with the problem of individual terms and restricted spatio-temporal regions which led him to the universality requirement. The problem is best illustrated with an example. Helmholtz was the first man to have seen a living human retina, and this property, unique to Helmholtz, can be used to form a definite description of him. But a sentence such as

For all x, if x is a man that has seen a living retina, and no other person has seen a living retina before x, then x contributed to the establishment of the principle of the conservation of energy,

which contains a tacit definite description of Helmholtz, is not universal according to Reichenbach's definition because it can be reduced to a form which makes the individual term explicit as a definite description. By using some, but not all, of the information employed in the description, however, the basic problem of the occurrence of a disguised reference to a restricted spatio-temporal region is still with us in the form of a statement no longer equivalent to one in which an individual term occurs. For example, "All stars seen by any man who saw a living human retina before any other man were at least of the 11th magnitude" expresses no more than a technological limitation on the telescopes available at the time of Helmholtz and can hardly be regarded as a law of nature. But the statement does not imply the existence of such a man. It therefore cannot be equivalently transformed into a statement with the definite description operator and is, accordingly, universal. Another restriction is necessary to eliminate such cases.

To deal with his problem Reichenbach requires that original nomological statements be *unrestrictedly exhaustive*, a notion defined in terms of properties Rx specifying that x occupies some restricted spatio-temporal region, and the basic notion of exhaustiveness already defined. The spatio-temporal predicates are introduced in the following way. Suppose Lx stands for "x saw a living human retina before any other man" and Rx for "x exists in such and such a restricted spatio-temporal region". The statement

(6) $\forall x(Lx \supset Rx)$

is true and can be verified since the only person to do L satisfies R. In view of (6), then, any generalisation which contains the predicate L should not count as an original nomological statement. An example of a generalisation which does contain L is

(7) $\forall x \forall y((Ty \wedge Sxy \wedge Lx) \supset My)$.

where Tx means "x is a star", Sxy means "x sees y" and My "y is of at least the 11th order of magnitude". This statement is now expanded according to the procedure already outlined for exhaustiveness in major terms, with the difference that an additional disjunct Rx is added, thus:

(8) $$\forall x \forall y(Rx \vee (\mathtt{Ant}(x,y) \wedge My) \vee ({\sim}\mathtt{Ant}(x,y) \wedge My) \vee ({\sim}\mathtt{Ant}(x,y) \wedge {\sim}My)),$$

where $\mathtt{Ant}(x,y)$ abbreviates the antecedent of (7). (8), formed by adding Rx in this way, is called an R-expansion of (7). The residual obtained from (8) by removing Rx is clearly true and verifiable, since it is just a reformulation of (7), and (8) (i.e. the R-expansion of (7)) is therefore not exhaustive. This means that the term "Rx" is redundant in (8) and therefore implicit in (7). The unreasonableness of the predicate L thus finds expression in the result that the R-expansion of (7) is not exhaustive. More generally, Reichenbach requires an original nomological statement to be unrestrictedly exhaustive, which means that there is no property R of being in a certain unrestricted space-time region such that, for any of the variables in the law, the R-expansion is not exhaustive.

The list of principal formal requirements on what is to count as a fundamental law or original nomological statement is completed with the condition that a fundamental law must be *general in self-contained factors*. The effect of this is to eliminate the possibility of simply adding to a statement which would otherwise count as a fundamental law a conjunct which is known to be true, such as "There are people".

Reichenbach went on to develop a concept of *admissible statements* upon which he builds a theory of counterfactuals, but the considerable detail involve won't be pursued here. We have seen something of the formal requirements Reichenbach imposed to exclude specific reference, whether explicit or implicit, and to preclude vacuous antecedents, as well as the distinction he draws between statements nomological relative to some particular matters of fact and fundamental nomological statements. Formal requirements are, however, at best only necessary conditions of lawlikeness. A sufficient condition for lawlikeness, in Reichenbach's view, calls upon verifiability together with the formal requirements.

Reichenbach's theory provided a natural starting point for Goodman's work on the problem of lawlikeness, which also emphasises the inadequacy of purely formal restrictions but points to severe difficulties with the notion of verifiability. Goodman concludes his comments on Reichenbach's initial attempt at a characterisation of laws by saying "the requirements Reichenbach sets up for nomological statements will be effective only if one places substantial and perhaps question-begging restrictions on the kinds of predicates that may be used" (Goodman, 1948, p. 414). We saw that Reichenbach intended his explication to apply only within a restricted language that can be "reasonably regarded as scientific", and the question arises whether some important problems have been swept under the carpet by assuming that a certain set of predicates is given in this way. Goodman certainly came to think so, and he developed his problem of the question-begging assumption that a range of appropriate predicates is given into his new riddle of induction in *Fact, Fiction and Forecast*.

6. On the Logical Character of Scientific Laws

The Achilles heel of any Humean analysis of scientific laws is the distinction between accidental and nomic universality. This is partly due to the general belief that extensional logic is the only valid logic available for representing the logical form of lawlike sentences. In 1948 Carl G. Hempel and Paul Oppenheim put forward their deductive-nomological model of explanation in which laws of nature play a crucial role.[5] For this purpose they adopted a rather strict notion of laws, according to which only true sentences can express laws. Since there are many lawlike sentences that have all the characteristics of a law except truth, however, every law can be represented by a lawlike sentence, but the converse does not hold. The logical structure of laws was thus distinguished from the empirical question of truth.

[5] All references are made to the reprint.

Hempel and Oppenheim's analysis seeks to provide individually necessary and jointly sufficient conditions for a sentence being lawlike and thus, by adding truth, to express a law: "Apart from being true, a law will have to satisfy a number of additional conditions. These can be studied independently of the factual requirement of truth, for they refer, as it were, to *all logically possible laws*, no matter whether factually true or false" (Hempel and Oppenheim, 1948, p. 265). According to Hempel and Oppenheim there are four such conditions which a sentence must meet to be lawlike: (1) it must have universal form, (2) it must be unlimited in scope, (3) it must make no reference to particular objects, and (4) it must contain only purely qualitative predicates.

The motivation behind these requirements seems to have broad intuitive appeal. First of all, lawlike sentences are statements of universal form, exemplified by 'All robins' eggs are greenish-blue' or 'All metals are conductors of electricity'. The first condition reflects the obvious idea that a law exists only if entities of a certain kind are all objects of such a law. If only some metals conducted electricity, we could not use this information to explain or predict anything about a particular piece of metal. But it is certainly not sufficient to characterise a lawlike sentence as having a universal form because some sentences are both true and of universal form but restricted to a certain place and a certain time. For instance, sentences like 'Every apple in this basket b at time t is red' have a limited scope by virtue of a reference to a specified object.

The second condition seems to yield a solution. A lawlike sentence must cover all objects in question, past, present, and future, in the universe. All robins' eggs are greenish-blue at all times, whereas every apple in a particular basket at a particular time is confined in space and time. Thus the scope of predication in a lawlike sentence must not be limited by any reference to a specific time or a specific place.

Further, a lawlike sentence should make no reference to particular objects. The idea of the third condition is that a law applies to an object irrespective of how we may conventionally name or classify this object. But there are many lawlike statements which we normally take to express laws of nature of which the third condition does not hold true . The first of Kepler's laws, "All planets move in elliptic orbits with the sun at one focus of the ellipse", for example. Hempel and Oppenheim attempt to cope with this problem by arguing that in contrast to sentences expressing mere accidental universality, Kepler's laws are known to be consequences of more comprehensive laws whose scope is unlimited and whose designators are not essential. Kepler's first law has a status of expressing a law of nature only if it is logically derivable from a fundamental law like Newton's laws. Thus, Hempel and Oppenheim follow Reichenbach in making a distinction between *fundamental* and *derivative* laws, and similarly between fundamental and derivative lawlike sentences. The latter is derivable from the former. The former, however, satisfy a certain condition of unrestricted scope and one of containing no uneliminable names for particular objects.

Nevertheless, problems remain. Nelson Goodman (1947) pointed out the need to impose certain restrictions upon predicates that are permissible in lawlike sentences. Consider the following sentence:

> Everything that is either a red apple in basket b at time t or a sample of ferric oxide is red.

Now, we may replace the predicate 'is either a red apple in basket b at time t or a sample of ferric oxide' with any arbitrary synonymous predicate such as 'is ferple'. This means that the above sentence can be expressed in the form

> Everything that is ferple is red.

This sentence is of universal form and contains no designation of particular objects, nor is it limited in scope. Yet, it is no more a fundamental lawlike sentence than the other sentence.

The solution, according to Hempel and Oppenheim, is to restrict predicates in a fundamental lawlike sentence to a purely qualitative ones, that is, ones where the explication of its meaning does not require reference to any particular object or spatio-temporal location. They mention terms like 'soft', 'green', 'warmer than', 'as long as', 'liquid', 'electrically charged', 'female', 'father of' as purely qualitative predicates, whereas 'taller than the Eiffel Tower, 'medieval', 'lunar', 'Arctic', and 'Ming' are not. They admit, however, that such terms suffer from a certain amount of vagueness since English as a natural language neither provides explicit definitions nor states unequivocally the meaning of its terms. The attempt to overcome the problem by introducing a formalised language will only help with respect to those predicates whose meanings are determined by definitions within the language. When it comes to the semantic interpretation of the primitive terms of a formal language there are no rigorous criteria for the distinction between permissible and nonpermissible interpretations. In spite of these difficulties of interpretation they conclude that there can be no doubt that a large number of purely qualitative predicates can be recognised to exist and that they are permissible in the formulation of fundamental lawlike sentences. Then Hempel and Oppenheim go on to give us a rigorous model theoretical characterisation of lawlike sentences based on the idea of purely qualitative predicates. But we will not go into the details here.

In his book *The Structure of Science* Ernst Nagel opposes the analysis proffered by Hempel and Oppenheim. He offers four kinds of considerations which seem relevant in classifying statements as representing laws of nature. These are (1) syntactical considerations in relation to the form of lawlike statements; (2) the logical relations of statements to other statements in a system of explanations; (3) the function assigned to lawlike statements in scientific inquiry; and (4) the cognitive attitudes manifested toward a statement because of the nature of available evidence. However, he doesn't claim that the conditions resulting from these considerations are sufficient or even, in some cases, necessary as a characterisation of a law of nature.

> Undoubtedly statements can be manufactured which satisfy these conditions but which would ordinarily not be called laws, just as statements sometimes called laws may be found which fail to satisfy one or more of these conditions. For reasons already stated, this is inevitable, for a precise explication of the meaning of "law of nature" which will be in agreement with every use of this vague expression is not possible. Nevertheless, statements satisfying these conditions appear to escape the objections raised by critics of a Humean analysis of nomic universality. (Nagel, 1961, p. 68)

What, then, are these conditions?

Nagel focuses on the question of the modal import of laws as consideration (1). He rejects the idea that nomic universality can be captured in terms of logical necessity or in terms of irreducible modal notions like "physical necessity". The first notion has the advantage that its meaning is transparent, but it faces grave difficulties since the formal denial of a law statement is demonstrably not self-contradictory, whereas the second notion is essentially obscure. He admits that there are contexts in which scientific laws are treated as if they are logically necessary and others where they are regarded as contingent. This is true of every sentence that can be associated with quite different meanings. But this does not tell us anything about the nature of scientific laws. Rather, it reflects the progress of science: "...the shifts in meaning to which sentences are subject as a consequence of advances in knowledge are an important feature in the development of comprehensive systems of explanation" (Nagel, 1961, p. 55). This assertion is reminiscent of the contextual view of laws. The difference is that Nagel thinks of it merely as a feature of our belief systems, whereas the contextualists, as we will see, take it to indicate something about the ontological relativism of scientific laws.

How, then, can the Humean, according to Nagel, distinguish between accidental and nomic universality? An answer to this query prompts consideration (2). He should impose a number of logical and epistemic requirements upon universal conditionals like having the form: for all x, if x is G, then x is H. The first, and most obvious, constraint is that it should be an *unrestricted* universal. This means that its scope of predication is not restricted to objects falling into fixed spatial regions or particular periods of time. Nagel rejects Hempel and Oppenheim's attempt to solve the problem by building on a semantic distinction between predicates that are "purely qualitative," whose meaning does not contain a reference to any particular object or space-time region, and predicates which are not purely qualitative. In addition, as we just saw, Hempel and Oppenheim introduced a further distinction between fundamental lawlike sentences, which contain only purely qualitative predicates, and derivative lawlike sentences. But Nagel finds this solution unsatisfactory partly because there was no fundamental lawlike statement from which Kepler's first law could be derived when he made his discovery, and partly because Kepler's first law is not derivable from fundamental laws alone. What is needed for such a derivation is "additional premises whose predicates are not purely qualitative" – premises which state the relative masses and the relative velocities of the planets and the sun.

According to Nagel, it is impossible on purely syntactic or semantic grounds to decide whether or not a universal conditional is unrestricted. The scope of predication may be finite, but the fact that it is finite cannot be inferred from the terms in the universal conditional that determine the scope of predication. The finite application must be established on the basis of independent empirical evidence. More important, however, is Nagel's recognition of a pragmatic identification of the scope of predication. He observes, as Hempel and Oppenheim did, that any restriction on the predication of attributes to an object could always be given a new name. His example is:

> For any x, if x is a screw in Smith's car during the time period a, then x is rusty during a.

A predicate "being a screw in Smith's car in a period of time a" may be replaced with the predicate like "being a scarscrew." Hence, we could reformulate the above conditional with an unrestricted universal:

> For any x, if x is a scarscrew, then x is rusty.

Nagel's point is that the syntactical structure of the new sentence does not reveal that the scope of predication is limited to objects satisfying a given condition during a finite period of time.

The unrestricted universality is only a necessary condition for a statement to express a law of nature. One would therefore expect that a candidate for being a law statement must satisfy further conditions. This search engenders consideration (3). If we look once again at Smith's car, we notice that it contains a finite number of rusty screws in a finite period of time. We are therefore in a position where we can hope to determine the truth-value of the conditional in question because we only have to examine a finite and fixed number of screws. Nagel thinks, furthermore, that if there is an indefinite number of screws in Smith's car, we may establish the truth of such an accidental universal conditional in two ways – either deductively from knowledge about all screws, for instance that they are made of iron and that they have been exposed to free oxygen, or inductively from the knowledge of a fair sample of screws in Smith's car. But this seems not to be the case concerning the evidence of unrestricted universal conditionals. Here, according to Nagel, it is a plausible requirement for an unrestricted universal statement to be called a law that the evidence for it is not known to coincide with the scope of predication and that its scope is not known to be closed by any further augmentation. This condition is important. It excludes an unrestricted universal conditional that is identical with a conjunction of statements expressing its total evidence from being a law. A law has the role of explaining and predicting. But it makes little sense to claim that a law explains or predicts a phenomenon if the law does not assert more than the evidence for it. If a law already contains a description of the phenomenon to be explained or predicted, it fails to explain or predict this phenomenon in any proper sense. What is also needed for calling a presumably true unrestricted universal statement a law is the assignment of a certain explanatory and predictive function to it. This rules out that the evidence on which a law is based is assumed to constitute the total scope of its predication.

In his last consideration (4) Nagel observes that laws can usually be recognised due to their special position in the corpus of our knowledge and due to the cognitive attitude we manifest toward them. He points out that the evidence by which we characterise a lawlike statement L as a law can be obtained directly or indirectly. The "direct" evidence consists of instances, whose properties fall within the scope of predication of L, whereas "indirect" evidence consists of instances that directly confirm other laws to which L is somehow inferentially connected. For instance, one sense of "indirect" evidence would be the case where direct evidence of Kepler's laws, Galileo's law, etc., serves as indirect evidence for Newton's laws. So a lawlike statement is called a law if both direct and indirect evidence support it.

Thus, according to Nagel, the Humean analysis of nomic universality should bring to light that a statement is often taken to express a law of nature because the statement occupies a distinctive position in the system of explanation in some area of knowledge, and because some evidence, satisfying certain specifications, supports the statement. Our attitude to such a universal statement is therefore more firmly settled than is our attitude to a universal statement without the required position and support of indirect evidence. We are not ready to abandon a universal conditional, which is considered to be a law, in the face of apparently contradictory evidence. This is due to the fact that laws are not only used as premises from which consequences are derived in accordance with the rules of formal logic but also function as rules of inference. When "a law is regarded as well-established and as occupying a firm position in the body of our knowledge, the law may itself come to be used as an empirical principle *in accordance with which* inferences are drawn" (Nagel, 1961, p. 67). His example is that the conclusion that a given piece of wire a is a good electric conductor can be derived from two premises, that a is copper and all copper is a good electric conductor. But the same conclusion can also be obtained from the single premise that a is copper if the principle of inference contains the rule that ' x is a good electrical conductor' can be derived from the statement ' x is copper'. This tendency concerning well-established laws may explain the view that lawlike statements express relations of logical necessity.

The Structure of Science is said to mark the end of positivism and logical empiricism. Nevertheless, as we have already noticed, in Nagel's analysis of laws there are certain elements which point in the direction of the contextual approach. Universal statements play different roles depending on the place these statements have in our system of knowledge and on the cognitive attitudes we have towards them. But the idea that the structure and organisation of our belief systems are important in distinguishing universal statements, which are accepted as laws, from universal statements that do not merit such a title, is something we also find in the Ramsey-Lewis account.

7. The Ramsey-Lewis Account

The problem of what is a law of nature and what form of sentences expresses such laws is not, as we've seen, merely a question of establishing syntactic and semantic criteria. The three criteria of being true, contingent, and a generalisation of the kind $\forall x(Px \supset Qx)$ are certainly not sufficient for being a law. Theory change of any kind may lead scientists to consider what were formerly accepted as laws to be mere generalisations, and vice versa, even though the sentences in question satisfy the syntactic and semantic criteria.

David Lewis (1973) develops a conception of laws of nature which copes with these and other difficulties. Lewis' main idea is due to Ramsey (1928, see Ramsey, 1978), and models a certain pragmatic feature: Think of theories as deductive systems in an axiomatic manner. One can have many true theories, some of them *more simple* than others, some of them *more informative* than others. Very simple theories have few and simple axioms, which they achieve at the cost of a poor informational content, while very rich theories are axiomatised with many complicated axioms. Now, according to Ramsey, laws are those sentences which are consequences in a deductive system which is axiomatised in the most simple way under the condition that the system captures everything worth knowing. In a sense, such a formulation does not only involve omniscience; as Lewis indicates, it appeals to God's standards for strength and informativeness and our standards for simplicity. Real scientific practice makes do with a trade-off between strength and simplicity. Accordingly, Lewis reformulates Ramsey's idea thus:

> 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. (Lewis, 1973, p. 73)

This account explains many of the questions bedevilling the simple regularity theory. Why is one of two sentences with identical syntactic and semantic characteristics (equally general, in particular) a law of nature and the other not? They play different roles in systematising knowledge, differ in explanatory power or in bringing about simplicity of the whole construction. Why might the same sentence be a law in one theory but not in the other? Different theories about the same realm might be thought of as belonging to different possible worlds. Whether a sentence is a law of nature or not depends on which other sentences are true and take part in systematising the knowledge. Why are we inclined to consider generalised conditionals as candidates for laws of nature? All of them may play an important role in building up a deductive system.

The role of Lewis' conception of laws of nature in the discussion of whether differences in laws or differences in facts having greater bearing in determining overall differences in worlds is well known. Lewis himself argues that "small miracles" – that is, violations of laws of one world in another world – may be compatible with greater similarity of two worlds than a huge number of differences in facts do (Lewis, 1973, pp. 74ff.; Lewis, 1979). In order to understand why this question is important, one has to remember that Lewis' theory of counterfactuals is formulated semantically in terms of similarity between worlds. Lewis uses a comparative similarity relation which forces him to consider the question of what kind of differences make worlds more or less similar to others. Lewis also analyses causation in terms of counterfactuals, and in this way a theory of lawhood becomes crucial for counterfactual dependence and causality. Again, a well established and understandable notion of (comparative) similarity between worlds would allow for a better understanding of scientific practice. What does it mean to say that one theory is more true than another? If it is not true, it is false, so how are such claims to be understood? Lewis believes that an almost true theory consists of laws true of a world which is not so far (in the similarity metric) from ours (Lewis, 1986a, p. 24).

Blowing new life into the idea of an axiomatic theory of laws has found sympathy in certain circles, especially amongst those who wouldn't think of themselves primarily as philosophers of science. But two main objections can be raised against such an attempt. First, it would seem that we can only hope to be able to axiomatise some very general parts of physical theories. Most scientific knowledge cannot be handled within an axiomatic system. Second, it is only if one thinks in traditional analytical terms that one can hope to get an understanding of laws of nature based on an understanding of the structure of formal languages. Most philosophers of science would probably argue that laws exist objectively in nature, and this is not reflected in how we might characterise or organise our belief systems.

8. The necessity view

Some philosophers find the regularity view quite unsatisfactory, and this attitude often goes hand in hand with a strong metaphysical predilection. They believe that universal laws transcend experience; so an empirical analysis cannot provide a proper understanding of laws of nature. A metaphysical account is required. The positivists thought that a law of nature could be expressed in terms of a universal statement like 'All planets move in ellipses'. Although observation cannot prove such a claim since it is practically impossible to experience all planets in the universe, the expression says no more than what could in principle be observed if we had the power to scrutinise the entire cosmos. Thus, a statement like 'All planets move in ellipses' is appropriately confined to what goes on in the actual world. But, so the objection goes, there might be a regular correlation between two properties A (being a planet) and B (moving in an ellipse) by pure chance even if that the regularity holds without exception in the whole universe, whereas if it is a law that A is B, then it cannot be accidental that A is related to B. The claim that it is a law that all planets move in ellipses excludes the possibility that the uniformity of this regularity could be otherwise. The obvious answer is therefore that a law of nature does not connect two properties in a regular but possibly accidental way. There must be ways of interpreting the content of 'All planets move in ellipses' so that some kind of necessity is involved in relating being a planet and moving in an ellipse.

There are three different notions of necessity. The strongest is *logical necessity*. The usual definition claims that a sentence is logically necessary if the negation of the sentence entails a contradiction. This means that necessity holds in all possible worlds in terms of a definition. Laws of nature, however, do not seem to relate to anything by definition. Copernicus thought that the planets move in circles, and no one could correctly accuse him of contradicting himself. Moving in ellipses is not a part of our concept of a planet.

A weaker sense of necessity is that in which a law relates what co-exists in every possible world. We may call this *metaphysical necessity*. This sense is what Saul Kripke has in mind when he argues that terms like 'the morning star' and 'the evening star' rigidly refer to the same planet in every possible world. Think of planets and the eccentricity of their orbits. The metaphysical notion of necessity would imply that there is no possible world in which there are planets and they move in circles. But Copernicus might have been right had the world been different. The law of inertia might have been such that a body subject to no external force would not change its position (rather than its velocity). In this case a planet would move in circles. Thus metaphysical necessity also seems too strong.

The weakest sense of necessity is *natural* or *physical necessity*. The content of this notion is usually specified by saying that something is naturally necessary if, and only if, it holds in every possible world in which the laws of nature are valid. But obviously, such an explication is circular if an account of laws of nature is to be provided in terms of natural necessity. A philosopher who wants to appeal to natural necessity must avoid any charge of being incoherent.

The positivists believed that a law statement like 'All planets move in ellipses' could be expressed formally in an extensional language such as

(9) $\forall x(Ax \supset Bx)$.

But according to the necessity view, (9) does not suffice as a formal expression of the law in question. What we need is some sort of modal representation to express the lawful relation between being a planet and moving in ellipses. There are at least two ways of doing this: either a modal operator can be placed in front of the entire expression or in front of the consequent, thus:

(10) $\Box\forall x(Ax \supset Bx)$

(11) $\forall x(Ax \supset \Box Bx)$

Formula (10) states that the conditional is a necessary truth, i.e. that the consequent logically follows from the antecedent, whereas formula (11) means that for any object, if the antecedent is true of it, then the consequent is necessarily true of it. The readings are standard whenever the scope of the modal necessity operator differs. And the necessity operator is open to interpretation in each of the above senses of necessity. Which of these two statements, if any, does in fact reflect the nomic connection? Among the necessitarians there is little agreement about which of these interpretations gives us the correct understanding of laws of nature.

The regularity view on laws came under heavy fire in 1950s. William Kneale (1950) was one of the first to directly attack the positivist view of laws as universally quantified material implications. His criticism focuses on the unrealised physical possibilities. If laws of nature were merely to consist of uniform regularities, then laws could not explain the contrast between possible and impossible physical facts. But they can explain the difference. Although we have never seen a solid sphere of gold with a diameter of more than one mile, we believe that such a possibility cannot be logically ruled out on the basis of laws of nature. It is a physical possibility. We have never seen a sphere of enriched uranium 235 with a diameter of more than one mile, on the other hand, but in this case we are able to dismiss the idea as physically impossible because of the law of critical mass. In other words, we can formulate two universal statements concerning spheres of gold and spheres of uranium 235, to the effect that every such sphere is less than one mile in diameter. But only the one concerning the sphere of uranium 235 that is necessary.

In the attempt to find a replacement for the view that any universal statement represents a law of nature Kneale claimed that statements of laws of nature imply counterfactuals whereas universal material implications do not. In (Kneale, 1961) he argued that laws of nature are logically contingent but still necessary in the sense that they allow no alternative. A statement like ‘All planets necessarily move in ellipses’ excludes possible alternatives. So the kind of necessity he had in mind is not logical necessity, it seems, but “a generalisation which holds for all possible worlds of some kind” (p. 63). Some kind of nomological necessity is thus involved. Apparently, he regarded this modal component to be irreducible and not further explainable.

Karl Popper agreed with Kneale “that there exists a category of statements, the laws of nature, which are logically stronger than the corresponding universal statements” (Popper, 1959, p. 432). He accepted that laws of nature set certain limits to what is possible. If, say, it is a law that all planets move in ellipses, then it *would not be possible* for any planet to move in a circle. Moreover, a statement like 'All planets *necessarily* move in ellipses' belongs to the appropriate category of sentences that express this impossibility. Popper seemed to make no distinction between (10) and (11) as the correct formal representation of a law statement, however, since he says "'If a, then necessarily b' holds if, and only if, 'If a, then b' is necessarily true" (Popper, 1959, p. 433).

What kind of necessity are we dealing with here, according to Popper? He himself maintained that a law of nature contains a natural or physical necessity that prohibits planets from moving in circles. It is conceivable, he said, that they do move in circles in some possible worlds but not in those where the law holds. The delicate question therefore is how he defines natural necessity. In *The Logic of Scientific Discovery* he suggested the following definition:

> (N^O) A statement may be said to be naturally or physically necessary if, and only if, it is deducible from a statement function which is satisfied in all worlds that differ from our world, if at all, only with respect to initial conditions. (Popper, 1959, p. 433)

Unfortunately, as we shall see, this definition did not enable Popper to avoid the threat of circularity. He even realised this himself:

> Nevertheless, the phrase in (N^O) 'all worlds which differ (if at all) from our world only with respect to the initial conditions' undoubtedly contains implicitly the idea of laws of nature. What we mean is 'all worlds which have the same structure – or the same natural laws – as our own world'. In so far as our *definiens* contains implicitly the idea of laws of nature, (N^O) may be said to be circular. But all definitions must be circular *in this sense* ... (Popper, 1959, p. 435)

First, we should notice that the first quotation describes a statement as being naturally or physically necessary. Popper wrote as if *laws of nature* are statements. He also said that he regarded 'necessary' as a mere word, a label for distinguishing the universality of laws from 'accidental universality'. Any other label could be used since the idea is not much connected with logical necessity. And Popper confirmed his agreement with "the spirit of Wittgenstein's paraphrase of Hume: 'A necessity for one thing to happen because another has happened does not exist. There is only logical necessity'" (Popper, 1959, p. 438). There are indeed good reasons to be puzzled by Popper's confused ways of expressing himself. Kneale (1961) correctly accused him of being inconsistent. The kind of necessity Popper had in mind cannot, if it is to make any sense, be a mere *de dicto* form of necessity. It does not make sense to claim that *physical* necessity holds among sentences but not in the real world. Such a notion requires that the laws of nature are also valid with respect to states of affairs other than the actual. The correct way of talking for someone who holds that laws of physics are physically necessary would be to say, "A statement may be said to express a natural or physical necessity ...". As an empiricist he cannot both be terrified by essentialism and, at the same time, operate with a notion of natural or physical necessity defined in terms of possible worlds.

Second, the first quotation characterises different possible worlds in terms of initial conditions and not directly in terms of physical laws. The consequence, however, is the same. Any talk of initial conditions makes sense only with respect to a given set of law statements. Natural or physical necessity is being defined in terms of laws of nature and, therefore, laws of nature cannot be defined with the help of natural or physical necessity. Popper does not think so because he holds that all definitions are circular in this sense. It may be true in cases where we consider a huge segment of a certain vocabulary and do not allow primitive terms in this vocabulary. But the circle in question is based on a very small segment, and Popper did not, it seems, hold the notion of law to be primitive. The upshot is that it is rather vacuous to distinguish universal statements from law statements in virtue of an appeal to natural or physical necessity since the meaning of the latter cannot be understood independently of the meaning of the former.

In the late 1970s David Armstrong (1978), Fred Dretske (1977), and Michael Tooley (1977) suggested that laws of nature are relations holding between universals. To say that it is a law of nature that Fs are Gs means that F necessitates G. The basic idea behind this view is that a law itself is not a necessity but accounts for necessity. Armstrong's book *What is a Law of Nature?* (Armstrong, 1983) is the most well-developed analysis of the three, and his account will be taken up here.

Like Kneale and Popper before him, Armstrong rejects the idea that a statement such as 'It is a law that all planets move in ellipses' is equivalent to 'All planets moves in ellipses'. Instead he considers it to be equivalent to 'It is physically necessary that all planets move in ellipses' (p. 77). This immediately raises two questions. How should this explication be understood formally, and what kind of necessity is involved?

If we take the first question first, it seems clear that Armstrong takes the necessity term to have the broadest possible scope. We should therefore expect him to take (10) to provide us with the correct formal representation of 'It is a law that Fs are Gs'. But he argues that even if (10) corresponds logically to what it means to be a law of nature, it doesn't do so metaphysically. Placing the necessity operator in front of (9) is, he says, "merely a technical solution unaccompanied by metaphysical insight" (Armstrong, 1983, p. 87). Moreover, he believes that (10) also faces internal problems in view of the connective – the material implication raises the well-known Paradoxes of Confirmation (pp. 87–88). As an alternative formulation, he proposes

(12) $N(F, G)$,

where (12) implies (9), *mutatis mutandis*, but not vice versa. So 'N' replaces '$\supset$' as the logical connective. Armstrong considers N to be a relation of nomic necessitation that connects two universals F and G. Moreover, $N(F, G)$ is itself a universal, a second-order universal, and Armstrong says that seeing this helps us to accept that N is a primitive relation. 'N' stands for "a real, irreducible, relation, a particular species of the necessitation relation, holding between the universals F and G" (Armstrong, 1983, p. 97).

So how should (12) be interpreted? Armstrong is clear on this point. It should be read as 'Something's being F necessitates that same something's being G, in virtue of the universals F and G' rather than 'For all x, x being F necessitates that x is G'. (Armstrong, 1983, p. 96) The former statement reflects the idea that the same relation of necessitation holds between sorts of states of affairs. The latter statement expresses, according to Armstrong, merely a more advanced form of the regularity view because it is assumed that a singular necessitation holds between particular states of affairs. Hence, it is equivalent to the formula:

(13) $\forall x N(Fx, Gx)$.

Their dependency is such that (12) entails (13), which again entails (9), but the reverse entailments do not hold.

This brings us to the second question. How can we identify the relation of necessitation? Armstrong believes that there are purely singular relations of necessitation without laws, and some of them are cases of singular causation which exist independently of a law. But can we be sure that singular necessitation corresponds to something in the real world since it does not correspond to the fact that a being F co-occurs with a being G? How can we identify the state of affairs that corresponds to the necessitation relation other than by invoking a claim such as effects follow their causes? Is it possible to identify the truth-maker of a singular necessitation statement independently of the claim itself? Even if we get a satisfactory answer to these questions, we cannot, without further arguments, use it to explain why one type of state of affairs (the universal F) necessitates another type of state of affairs (universal G) because (12) is not merely a generalisation of singular necessitation. The fact is that Armstrong does not tell us how to identify necessitation.

Are laws of nature necessary or contingent? Armstrong excludes strong necessity, which he represents as

(14) $\Box N(F, G)$.

This formula is analogous to (11). He takes it to mean that in every possible world the relation N relates F and G. So his understanding of the necessity operator is that of logical necessity. But, as we have seen, the necessity operator has alternative possible interpretations. Nevertheless, he rejects the proposal because this would make not only laws but also universals *necessary beings* and there are, he argues, definitely worlds without Fs and Gs (Armstrong, 1983, p. 164). Hence F and G are contingent beings. Moreover, laws of nature might have been different from what they are, and they cannot therefore be strongly necessary.

Armstrong also discusses another formulation as a candidate for 'It is a law that Fs are Gs' which he takes to express a weaker form of necessity. Here the formulation combines the contingency of universals with the necessity of laws:

(15) $\Box($ the universal F exists $\supset N(F, G))$.

Apart from the necessity operator in the front, (15) bears a certain resemblance to (11). Spelled out in ordinary terms, it says that in all those worlds which contain the universal F, it is a law that Fs are Gs. This view requires the existence of irreducible powers, Armstrong argues, but it does not fit well with his hopes of an actualist metaphysics.

> Although I do not believe in the literal reality of possible worlds, or even in the literal reality of ways things might have been but are not, I know of no way to argue the question before us except by considering possible worlds. It may be that the necessary/contingent distinction is tied to a metaphysics which recognizes possibility as a real something wider than actuality. If this could be shown, then my inclination would be to abandon the necessary/contingent distinction and declare our present question about the status of the laws of nature unreal. But I cling to the hope that an account of 'possible worlds' can be given which does not assume the existence of *possibilia*. (Armstrong, 1983, p. 163)

Whether or not such an actualist account can be given remains to been seen. For laws are logically contingent, according to Armstrong, but also physically necessary (Armstrong, 1983, p. 77). He also mentions it as a contingent necessity that being F necessitates being G. One philosopher, as we shall see later, has bitten the bullet and declared not only that possible worlds are fictions, but also that laws of nature are unreal.

9. Conventionalism

A very different approach to understanding laws of nature is found among conventionalists. They focus on the idea that fundamental law statements, like Newton's three laws of motion, resist falsification. Henri

Poincaré, the father of conventionalism, emphasised this in particular by saying that an empirical law is always subject to revision but no one seriously believes that any of Newton's laws will be abandoned or amended. Why is this so?

Explaining it, we should notice that Poincaré distinguished, in his Preface to *Science and Hypothesis*, between three kinds of laws, or rather three kinds of hypotheses expressing such laws. These are (1) *experimental laws*, i.e. hypotheses that "are verifiable, and when once confirmed by experiment become truths of great fertility"; (2) *principles*, i.e. hypotheses "useful to us in fixing our ideas"; and (3) *definitions*, i.e. hypotheses "that are hypotheses only in appearance, and reduce to definitions or to conventions in disguise". Although he did not at this place use the terms "experimental laws", "principles" and "definitions" himself, they can be found elsewhere in the text. Furthermore, he believed that the fundamental laws of classical mechanics and the principle of the conservation of energy belong to the category of principles and definitions.

Principles are conventions or definitions in disguise. They are deduced (generalised) from experimental laws in the sense that these laws have been elevated to principles, which the scientific mind attributes an absolute status for the time being. Such conventions are not absolutely arbitrary. We accept them because certain experiments have shown us that they are convenient. Also Poincaré gave an explanation of how a law can become a principle. First, we have a hypothesis that expresses a relation between "two real terms" A and B. It does not state a rigorous truth but only an approximation. Second, we arbitrarily introduce an intermediate term, C, which he characterised as more or less imaginary. The term C is therefore a result of abstraction and idealisation. Now C is "*by definition* that which has with A *exactly* the relation expressed by the law". And he continued:

> So our law is decomposed into an absolute and rigorous principle which expresses the relation of A to C, and an approximate experimental and revisable law which expresses the relation of C to B. But it is clear that however far this decomposition may be carried, laws will always remain. (Poincaré, 1952, p. 139)

An example is the experimental law that the sum of the kinetic and potential energy, $T + U$, is constant. Kinetic energy is proportional to the square of the velocity and the potential energy is independent of the velocities. We might think that the conservation of energy could be gained experimentally by adding the internal molecular energy to the sum of kinetic and potential energy $T + U + Q$ where Q is independent of the position and velocity. But Poincaré argued that this is not possible since the internal molecular energy is not only dependent on their internal state. The electrostatic energy of electric charges depends on both their positions and their velocities. Therefore, the three terms in $T + U + Q$ are not absolutely distinct. So it is not only true that $T + U + Q$ is constant; the same is true of any function $\varphi(T + U + Q)$ whatsoever, and "among those functions that remain constant there is not one which can rigorously be placed in" the particular formula of three distinct terms. The consequence is that the conservation of energy can never be equivalent to the empirical law that $T + U + Q$ is constant.

Poincaré did not claim that, say, Newton's laws of motion couldn't be disconfirmed because these statements are a priori true. If, say, the law of inertia were imposed on us a priori, it would be impossible to understand why the Greeks never got it right. Furthermore, one cannot argue that velocity does not change unless acted upon by saying that this is the only law compatible with the principle of sufficient reason. For the world might well have been different in such a way that it is a law of nature that the position or the acceleration of a body would be unchanged if it were not acted upon by a force.

But neither does the law of inertia express an empirical fact because it is impossible to conceive evidence against it. It is impossible, Poincaré says, to make experiments on bodies on which no forces act, and even if it were possible, we would have no means to know that no forces were acting on such a body. Thus, he called the law of inertia *the principle of inertia*, indicating that such a statement should be considered basic or superior in our thinking, expressing a rule which our thoughts obey in the description of nature. We do not have to adhere to them, but we do for reasons of convenience since it is "useful to us in fixing our ideas".

Poincaré called Newton's second law of motion the law of acceleration. We can measure acceleration, he argued, but we cannot measure force or mass, and we don't know what they are. We have one equation with two unknowns. If we claim, for instance, that "force is the cause of motion, we are talking metaphysics" (Poincaré, 1952, p. 98) because we cannot establish what makes a force F equal to a force F^*. This is necessary if we are to check that the same numerical forces produce the same numerical accelerations when applied to the same numerical masses. We may believe that it is possible to measure masses independently by their weight, but the weight of the same mass varies with respect to gravitation. We need a third definition, Newton's third law, which equates action and reaction. But, as Poincaré pointed out, "we are compelled to bring into our definition of the equality of two forces the principle of equality of action and reaction; *hence this principle can no longer be regarded as an experimental law but only as a definition*" (Poincaré, 1952, p. 100).

The third law then defines what it means for a force to be equal to another. Assume two bodies, A and B, act on each other. The acceleration of A times its mass is then said to be equal to the action of B on A, and in the same way the acceleration of B times its mass is equal to the action of A on B. But two bodies are never alone and cannot be abstracted from the rest of the world. We have no means of distinguishing the action between A and B from the acceleration due to all other bodies, and this makes the decomposition of the various central forces involved impossible. The upshot is that "masses are coefficients which it is found convenient to introduce into calculations" (Poincaré, 1952, p. 103).

So Poincaré believed that the law of acceleration and the decomposition of forces were conventions, although not arbitrary conventions, because their adoption was based on experiments. Some philosophers have similarly argued that the basic laws of nature should be regarded as definitions, whereas others have opted for a view according to which it is the specific situation that determines whether laws of nature should be considered definitions or empirical hypotheses.

10. The contextual view

One of Poincaré's contemporary countrymen, Pierre Duhem, briefly discussed conventionalism in his *The Aim and Structure of the Physical Theory* which was published a year after *Science and Hypothesis*. According to one widespread interpretation, he himself regarded laws of physics as neither true nor false but approximate, in virtue of which he says they are relative, and they are provisional because they are symbolic representations. On this interpretation, he was sympathetic to the insight behind the conventionalist view that the fundamental laws such as Newton's laws of motion may act like definitions and therefore be almost immune to falsification. But he also stated that such confidence in a law of nature is not "analogous to the certainty that a mathematical definition draws from its very essence" (Duhem, 1906, p. 211). If the result of an experiment disagrees with a certain theory, we do not know which part of the symbolic representation that has to be rejected but we know that some part must be discharged. A single experiment can never condemn a hypothesis in isolation, it can only question a whole theoretical system of laws. He also thought that, whenever physicists of a certain epoch look for possible modifications, there is always a certain number of laws which they agree to accept without further test because they consider them beyond dispute. But Duhem warned against believing that physicists are forced to act in this way because of logical necessity. They do so because to act otherwise would be irrational.

So Duhem accepted that laws might be treated as conventions.[6] He wanted, however, to warn against considerations which would treat them as analytic truths. He concluded:

> . . . we must really guard ourselves against believing forever warranted those hypotheses which have become universally adopted conventions, and whose certainty seems to break through experimental contradiction by throwing the latter back on more doubtful assumptions. The history of physics shows us that very often the human mind has been led to overthrow such principles completely, though they have been regarded by common consent for centuries as inviolable axioms, and to rebuild its physical theories on new hypotheses. (Duhem, 1906, p. 212)

Duhem knew the history of physics better than anyone did. He therefore spoke with the insight of an active scientist as well as that of a historian of science. But one should also remember that Poincaré knew better than most other physicists that conventions could be discarded as inadequate since he, and Einstein, took the lead in the revolution of relativity overthrowing the Newtonian physics. So Poincaré was very open-minded concerning the exchange of one set of conventions with an alternative formulation of them.

When discussing fundamental laws as stipulative definitions a distinction should be kept in mind. The notion of definition is very sensitive to a context and relative to a set of interest. Hence, scientific laws can be divided into definitions, principles and experimental laws without any of these being completely sacrosanct whenever a theory runs into trouble. A law being a definition means being analytic-in-a-theory and does not mean being analytically true. The difference is that an analytic statement is true by virtue of synonymous meaning, and is traditionally regarded as an example of priori knowledge, whereas definitions as analytic-in-a-theory are meaning-constitutive for that theory but their truth-values are empirically determined. In fact it is not definitions themselves that are true or false, but rather the truth-values of concrete sentences expressed according to these definitions. Definitions as meaning-constitutive stipulations are more or less adequate. Laws that are regarded as stipulative definitions will therefore resist empirical revision much longer than experimental laws.

---

[6] One of us has questioned the antirealist interpretation of Duhem and argued against the conventionalist interpretation (Needham 1998, 2000). On this view, Duhem's argument against Poincaré was to the effect that the holistic nature of testing doesn't allow for any distinction in principle between ordinary laws and those like the principles of mechanics which Poincaré held to be conventions.

Ideas similar to those of Duhem prompted philosophers, like Russell Norwood Hanson (1958) and Thomas B. Kuhn (1962/70), to propose a highly context-dependent notion of fundamental laws. Russell Hanson argued that expressions like 'Newton's second law' and 'The law of gravitation' should be considered as 'umbrella-titles' since the formula '$F = m\mathrm{d}^2x/\mathrm{d}t^2$' and '$F = \mathrm{k}(m_1m_2)/r^2$' can be used as definitions, a priori expressions, heuristic principles, empirical hypothesis, rules of inferences, etc. (Hanson, 1958, p. 112). Which of these distinct uses a physicist would take to be relevant depends on the concrete situation and the general purpose he wants to pursue. In the discovery of the non-visible planets, Uranus, Neptune and Pluto, Newton's law of gravitation was treated more as a definition than anything else. Russell Hanson mentions an example where Newton's laws were put to test as an empirical hypothesis, however. In 1784 the English physicist George Atwood wanted to show that Bernoullis' and Leibniz's attacks on the Newtonian mechanics were ill founded, and he conducted an experiment to prove that Newton's laws were consistent with the facts.

Kuhn held a view similar to Russell Hanson's. He distinguished between exemplars and symbolic generalisations. Exemplars are the application of the symbolic generalisations to specific types of situation in accordance with so-called standard models. However, much like Duhem, he thought of Newton's laws, Coulomb's Law, Ohm's law, and Maxwell's equations as symbolic generalisations rather than empirical hypotheses because in their most general formulation they don't say anything about concrete physical systems. They are kinds of abstract schemata which physicists have to concretise before they can apply them to particular cases. According to Kuhn, "they function in part as laws but also in part as definitions of some of the symbols they deploy. Furthermore, the balance between their inseparable legislative and definitional force shifts over time" (Kuhn, 1962, p. 183). In other words, the historical context has an important say in the way physicists would treat symbolic generalisations. In the beginning physicists would look at them more as empirical hypotheses; later on, when these have become parts of a successful paradigm, more as definitions. He summarised his opinion in the following way: "I currently suspect that all revolutions involve, among other things, the abandonment of generalizations that force of which had previously been in some part that of tautologies" (Kuhn, 1962, pp. 183-184).

The contextual view sees certain basic laws as symbolic representations whose function is determined by the role which physicists assign to them in a certain historical situation. As law-schemas they are idealisations that do not apply directly to a physical system, but must be mediated through models of application. Therefore this view entertains a certain kinship to the *ceteris paribus* account of laws. Many will, nonetheless, feel a strong reservation against contextualism because of, as they see it, its ontological shortcomings. For contextualists must hold that law statements do not carry a literal meaning since the linguistic function of law statements depends on the context in which they are asserted and their ontological status is relative to this function. Hence there are no real matters of fact corresponding to a law statement and described in terms that are literally true or false. Theories cannot be true or false, as advocated by Duhem, and some kinds of instrumentalism or constructive antirealism must follow.

11. What if there are no laws?

The contextual view threatens to render a substantial notion of laws of nature obsolete and allow laws once again to be reduced to regularities. There are no laws, on this view, only restricted regularities. This is Bas van Fraassen's position. In his book *Laws and Symmetry* he takes issue with the traditional, and metaphysically based, notion of laws as it traces back to Descartes in the seventeenth century. Such a notion no longer belongs to natural science, he maintains. Today it figures only in philosophical writings, where the aim of science is held to be the discovery of laws of nature, which are therefore a central ontological concern for the philosophy of science.

Within the metaphysical tradition, van Fraassen recognises two arguments for a robust notion of laws. One argues that there are pervasive and stable regularities in nature, but that no such uniformity can exist by chance. Why should there be such regularities if is wasn't for the existence of laws of nature? The other argument holds that denying that there are reasons for a regularity leads to scepticism because without laws of nature it is not rational to expect the future to be like the past. But he concludes:

> I can quite consistently say that all bodies maintain their velocities unless acted upon, and add that this is just the way things are. That is consistent; it asserts a regularity and denies that there is some deeper reason to be found. It would be strange and misleading to express this opinion by saying that this is the way things are by chance. But that just shows that the phrase 'by chance' is tortured if we equate it to 'for no reason'. (van Fraassen, 1989, p. 21)

Undoubtedly, the metaphysician will object that laws and truths in general differ, that there are criteria, which a law of nature can meet, but which a mere regularity cannot, such as universality, necessity, and objectivity. van Fraassen discusses these criteria one by one. He rebuts the demand for universality by quoting an example from Reichenbach and Hempel:

1 All solid spheres of enriched uranium (U235) have a diameter of less than one mile

2 All solid spheres of gold (Au) have a diameter of less than one mile.

Although both statements are true, van Fraassen agrees that it is only the first expresses a putative law because of uranium's critical mass; the second states an accidental fact. Nevertheless, both are universal to the same degree. Moreover, they are in general syntactically and semantically indistinguishable, and it is therefore impossible to identify laws as the true "law-like" statements. We cannot separate a law statement from an accidental statement in virtue of language. He also points out that it is almost impossible to specify generality of content, and that it might even be the case that universality is not a requirement for being a law.

Necessity is also a general feature associated with laws of nature. We usually think that what participates in such a law must obey it. van Fraassen distinguishes between four kinds of necessity: inference, intensionality, *the necessity bestowed* and *the necessity inherited*. Inference and intensionality are matters of logic, which is a matter of what conclusions follow from premises. Those are the only kinds of necessity empiricists can accept. The notion of necessity bestowed, according to which "It is a law of nature that A is true" is considered equivalent to "It is necessary that A is true", is a notion that they reject. Even more so, they reject a stronger notion according to which the necessity is inherent in the laws themselves. Such features of necessity are not immediately perceptible, and no inference such as inference to the best explanation has the strength to take us from the sort of facts that actually can be observed to a claim that unobservable facts of necessity provide us with the best explanation of regularities.

Lawfulness is commonly linked to counterfactuals in so far as giving warrant for counterfactual conditionals is regarded as a criterion of law. In the mid-1940s Nelson Goodman and Roderick Chisholm realised that counterfactuals do not reflect the same principles of reasoning that hold for strict or necessary conditionals. But van Fraassen thinks that the semantic analysis of counterfactuals, which Robert Stalnaker and David Lewis carried out in the late-1960s, shows that the behaviour of counterfactual conditionals deviates from that of strict ones because of context-dependence. This means that the most interesting counterfactuals do not derive from necessities alone "but also from some contextually fixed factual considerations". van Fraassen denies, however, that science itself implies any interesting counterfactuals. So he argues that if laws did in fact imply such conditionals then they would have to be indexical statements, which conflicts with the idea that they have a purely objective content.

Apart from having to address these criteria of lawfulness, van Fraassen believes that any philosophical account of laws of nature must face two major problems, which he calls *the problem of inference* and *the problem of identification*. An easy solution to one of them creates unavoidable difficulties for the other. Take the question of inference. It is a reasonable requirement of any account to say that if it is a law that A, then it implies that A. A simple solution that makes such an inference possible is to equate *It is a law that A* with *It is necessary that A*, relying on the logical principle that necessity implies actuality. But, as van Fraassen points out, we now have a problem of identifying what sort of facts makes such a claim true. If the problem is met by maintaining that necessity is itself a primitive fact, however, then it is not clear why necessity should be thought to include actuality. Conversely, if we first identify those actual regularities that seem to lead us to a claim that it is a law that A, there is little or no room for an inference that there is a law that A, and therefore that A because we are haunted by the problem of induction.

The only tenable solution, according to van Fraassen, is to claim that there are regularities in the world but no laws. His constructive empiricism is well known. The central tenet of this view, as presented in his *The Scientific Image* (1980), is that the aim of scientific theories is not to yield true descriptions of the world, but to give us empirically adequate descriptions of phenomena. He also denies that we accept scientific theories because we believe that they are true. Rather, we accept them because we believe that they are empirically adequate. A description is said to be empirically adequate if it is true with respect to what can be observed, and only to what can be observed. As a consequence, van Fraassen views with great suspicion any position that tries to vindicate the existence of hypothetical entities or lawful relations, which cannot be seen by the naked eye. Whether the argument focuses on the reality of unobservable entities or the reality of laws, it makes, unjustifiably, a leap from what can be actually known to be to what may possibly be. Empiricists, like van Fraassen, have always had their doubts about *real* possibilities other than actualities.

Most other accounts of natural laws introduce modality as something in virtue of which laws can be characterised in order to distinguish them from mere regularities. A constructive empiricist, however, cannot accept such accounts in terms of *de re* modalities since necessities and possibilities are inaccessible to the faculty of perception. As van Fraassen puts it in *Laws and Symmetry*: "From an empiricist point of view, there are besides relations among actual matters of fact, only relations among words and ideas. Yet causal and modal locutions appear to introduce relations among possibilities, relations of the actual to the possible" (van Fraassen, 1980, p. 213). Rather than being part of nature, modalities are part of language, and a philosophical explication of modality is to be part of a theory of meaning. Thus, van Fraassen solves the dilemma by arguing that there are no laws.

12. The *ceteris paribus* View

A large part of the recent literature on laws of nature, and many of the essays in the present volume, relate directly or indirectly to Nancy Cartwright's works. Discussion centres on her characterisations of fundamental laws either as false or as being *ceteris paribus* laws. In her later works she combines these features with an understanding akin to the necessity view. As she says, "a law of nature is a necessary regular association between properties antecedently regarded as OK" (Cartwright, 1999, p. 49). However, as discussions in this book show, nothing forces a proponent of the *ceteris paribus* view to accept Cartwright's view on capacities. The *ceteris paribus* view can be combined with conventionalism, contextualism and Duhemianism.

A *ceteris paribus* clause expresses that there are some circumstances which must be fulfilled in order for the statement it makes conditional to be true. By adding the clause, the universal applicability of the statement in question is narrowed. Nancy Cartwright does not distinguish between different types of *ceteris paribus* clause. A distinction can be made, however, between those clauses that require the fulfilling of certain abstract or ideal circumstances and those that require the realisation of certain factual or concrete circumstances. Newton's law of gravitation illustrates the first type. It is true only if the bodies to which it is applied are regarded as point masses and only if there are two of them. But such conditions are not of this world. It is therefore often said that Newton's law of gravitation is only true of a model. The other type is exemplified by causal statements. For instance, a causal claim like striking a match makes it light is true just in case certain concrete circumstances are fulfilled: the presence of oxygen, the match is dry, and the right amount of friction is applied when pressing the match against the sulphur, etc. Consequently, causal statements are true of the world but fundamental laws are not.

Cartwright's influence on the discussion is partially explained by her holding a strong and somewhat counterintuitive sounding thesis:

> ... the fundamental laws of physics do not describe true facts about reality. Rendered as descriptions of facts, they are false; amended to be true, they lose their fundamental, explanatory force. (Cartwright, 1983, p. 54)

How should one understand such a challenge to common-sense ideology? Usually, fundamental laws are taken to explain, and to support epistemologically, phenomenological laws (of physics and other sciences). On this view, doubting fundamental laws might be motivated from the traditional antirealist position that focuses on the theoretical terms use in the formulations of fundamental laws. These have to be connected to entities figuring in phenomenological laws. Worries about the status of theoretical terms, about their relationship to experience or holding outright that no claims whatsoever relate to reality, lie behind traditional antirealist positions. Cartwright, on the other hand, does not worry about theoretical terms and nor does she believe that statements can represent facts of nature. Although she first dubbed her position antirealist, later on she singles out fundamentalism as the real enemy (Cartwright, 1999, Ch. 1). The failure lies in the nature of explanation.

The division of laws of nature just mentioned into phenomenological laws and fundamental laws becomes clear from Cartwright's examples. Most biological laws or engineering rules are phenomenological, whereas Coulomb's law and the law of gravitation are fundamental. The point of her arguments consists in an observation about the role fundamental laws must play in scientific practice. In order to be true, they have to describe the behaviour of entities, but in order to explain, the "composite" forces have to be disassembled into more elementary parts, for it is these that are described by the fundamental laws. Cartwright denies that there is something like a law of composition of forces comparable to the rules of vector addition which can be applied to take them apart and put them together. A charged massive body moves neither according to the law of gravitation, nor according to Coulomb's law, nor according to the laws of a third force which would be a resultant of the two aforementioned forces. Fundamental laws are designed to explain phenomenological laws; they are tools to systematise them and it one of their essential features that they explain more than one of them. But precisely this feature prevents them from being true. If they were true, they could not explain; if they explain, they cannot be literally true.[7]

This more critical part of Cartwright's conception amounts to the idea that phenomenological laws – which reflect things as they actually are in nature – cannot be derived from fundamental laws by laws of logic. Neither are fundamental laws true abstractions of phenomenological laws, speaking about the same things in a more general and abstract manner. With the help of many examples, Cartwright argues that the real relationship between the two types of laws has to be understood to be created by a long and complicated process of approximations and emendations. If she is right, the problem consists in a misunderstanding about how the laws of physics explain. Her first suggestion for solving this problem she called a "simulacrum account". This account appeals to two senses in which the term "realistic" might be applied. In a first sense, physical theories may be realistically interpreted if they give a close enough description of what happens in reality. In this sense, the more realistic the theory, the fewer *ceteris paribus* conditions or fewer physically unrealised assumptions are involved. In a second sense, physical theories may be realistically interpreted if they explain what happens in the mathematical apparatus. Physical models may be realistic in many respects. One might wish to calculate a functional relationship with great precision, one might wish to understand a causal relationship in detail, or one might be interested in completely different properties. A physical model is a simulacrum insofar as it shares certain properties with the modelled part of the world (to a greater or lesser extent), although it still has many other properties which are not essential and perhaps even completely arbitrary: "A model is a work of fiction" (Cartwright, 1983, p. 153). Usually, Cartwright criticises, many physicists conclude that even the "properties of convenience" of a model which fits a simple case must be there when it is applied to much more complicated cases. Unfortunately, she is not able develop the simulacrum account in a formal manner, but she describes it as follows:

> It [*the simulacrum account*] says that we lay out a model, and within the model we "derive" various laws which match more or less well with bits of phenomenological behaviour. But even inside the model, derivation is not what the D-N account would have it be, and I do not have any clear alternative. (Cartwright, 1983, p. 161)

[7] That does not mean that there aren't any true explanations. Accepting, for instance, the causal explanation of a fact of nature means simply accepting the cause – even if this involves theoretical terms.

Nature is complex through and through, so simplicity is gained only at the cost of misrepresentation, particularly of misrepresentation of singular causal processes. Since grasping causation seems to be crucial for understanding what is going on "inside the model", Cartwright turns to the problem by finding an account of causality.

Why do causes increase the probability of their effects? Because the single cause, Cartwright says, has the capacity to do so. She even thinks that the metaphysics of capacities provides an answer to questions such as why theoretical laws are applicable in different situations. If causes have the capacity to bring about a certain effect, then they can be expected to carry that capacity from one situation to another. Even the fact that causes increase the probability of their effects *in all causally homogeneous circumstances* shows that capacities exist:

> Just as laws constrain relations between matters of fact, capacities constrain relations among laws. A property carries its capacities with it, from situation to situation. That means that, where capacities are at work, . . . one can infer from one causal law directly to another, without ever having to do more tests. (Cartwright, 1989, p. 146)

Cartwright speaks about "modal levels" where the claims at the higher level constrain what structure the set of claims at the lower level can have (Cartwright, 1989, p. 160):

Levels of Modality:	Ascriptions of capacity
	Causal Laws
	Functional and probabilistic laws
Non-modal Level:	Occurrent regularities

In that picture, capacities play a special role. They are not only modalities; they are in the world. Cartwright would have us believe that nature selects several capacities that different factors carry, and it sets the rules for their interplay. From this, causal laws as well as everything under them (see table) gets shaped. Speaking metaphysically, capacities are natures, or essences; they exist, whereas associations are epiphenomena, and ontologically secondary.

Still, the problem which appeared right at the beginning of Cartwright's discussion remains. Basic facts about phenomenological regularities are reconstructed well enough in low-level laws (as in biology, technology or econometrics). Higher-level laws are designed to explain these phenomenological laws rather than the behaviour of things in the world. Cartwright was able to show that this is done, not by logical deduction and not by deductive nomological reasoning, but rather by putting certain constraints on lower levels. It remains to be explained how this is realised in the case of capacities with respect to fundamental laws. If Cartwright could answer this question she would have solved the problem mentioned at the beginning of this section: How do laws explain?

The key notion of Cartwright's conception of where laws of nature come from is that of a "nomological machine":

> It is a fixed (enough) arrangement of components, or factors, with stable (enough) capacities that in the right sort of stable (enough) environment will, with repeated operation, give rise to the kind of regular behaviour that we represent in our scientific laws. (Cartwright, 1999, p. 50)

A nomological machine is an arrangement of certain capacities, which can be rearranged and collected together in other such machines. Thus, Kepler's law about the motion of the planet Mars along an elliptic orbit is really a law about physical bodies moving in time and space. Newton's law of gravitation does not speak about bodies moving in time and space; rather, it introduces a capacity: in the right circumstances, a force has the capacity to change the motion of a body. This capacity may, of course, be used in other nomological machines, too. In any case, it will shape the character of the regularities we expect to hold.

As can be seen from the example, nomological machines are like models in some respects. They need the right parts – capacities – which have to be arranged in the appropriate manner. They have to be shielded; that is, they will give the results desired only in the stable circumstances mentioned. Hence, all laws stemming from a nomological machine will come with *ceteris paribus* conditions. The very idea of compositionality, which is part of what is involved in the idea of explanation (the movement's characteristics x, y are explained by one reason, y, z by others) is build into the concept of a nomological machine, and since capacities are tendencies or propensities (as distinct from Carnapian dispositions), they can explain without real deductive force.

13. Summary of the papers

The present volume collects papers on the essential character of laws of nature. A variety of conceptions are presented by a dozen authors. Together they give a wide-ranging overview of the state-of-the-art of contemporary discussion on the subject.

Mauro Dorato's paper "Why are (most) laws of nature mathematical?" concentrates on measurement too. He addresses Eugene Wigner's question about the unreasonable effectiveness of mathematics in the natural sciences. Dismissing the so-called "software approach to laws", Dorato arrives at the conclusion that laws of nature are mathematical because they are expressed in a mathematical form isomorphic to the relational structure of the respective natural systems. The essence of this isomorphism, however, emerges from the fact that many mathematical concepts do have empirical origins.

There is a prominent view of laws of nature as linguistic conventions or rules of language, advocated, for example, by Henri Poincaré. Defending a variant of this approach in "How nature makes sense", Jan Faye distinguishes between two sorts of laws: theoretical and causal. Methodologically, theoretical laws precede causal laws – we need the first to formulate the second. He argues, against Nancy Cartwright and others, that theoretical laws don't involve *ceteris paribus* clauses, but causal laws do. It is causal laws that allow "nature's sense" to emerge. According to Faye, causes are grounded in the way we see nature, so causal necessity is bound to possible experience. Theoretical laws, on the other hand, have no descriptive content and are therefore conventional in a sense.

Igor Hanzel analyses in great detail the evolution of the view on laws of one of the most influential contemporary philosophers of science against the approach presented by one of today's most interesting philosophers of science. In his "Nancy Cartwright on laws as lies and as capacity claims" Hanzel demonstrates how Cartwright's lack of knowledge of Leszek Nowak's work predetermined the style of her own considerations. In effect, Cartwright's later project, seeing laws as capacities, also seems problematic in the face of, for example, theoretical physics.

Henrik Hållsten sets out to defend explanatory deductivism. To do so he has to come to terms with probabilistic causes: granting them explanatory force leads to unwanted consequences. In his "The explanatory virtues of probabilistic causal laws" Hållsten argues that a probabilistic cause (if there is such a thing at all) would be neither necessary nor sufficient for its effect. What we have in such a case, essentially, are chances. These chances do not add up to an intuitive cause, whereas the degrees of our rational beliefs do add up.

In his "The nature of natural laws" Lars Göran Johansson discusses the definition of the concept of a natural law. He argues that there are various forms of such laws, each requiring different treatment. Some laws express quantitative relations. These are derivable from a set of fundamental laws, i.e. of implicit definitions of the predicates involved. Some other laws are conservation principles. Johansson identifies them as consequences of objectivity demands on the descriptions of physical systems. Some laws are neither of the above and Johansson's analysis does not touch on them. It does, however, cover quite a number of important cases of natural law.

Geert Keil argues that there are no strict laws of succession, i.e. universally quantified conditionals saying that any event of type c is followed by an event of type e. Because all such statements are subject to exceptions, they are compromised as laws. However, the title, "How the *ceteris paribus* laws of physics lie", clearly expresses the authors opinion that this doesn't apply to all physical laws. In the course of his discussion, Keil presents a very interesting overview of various ways to understand the term "*ceteris paribus*".

Challenging the empiricist tradition, Max Kistler defends the necessity of (at least some) laws of nature. He does so by using subtle metaphysical arguments. His "Necessary Laws" ends with a clear-cut conclusion: laws are second-order relations between properties and they are necessary insofar as they hold in all possible worlds in which the relevant first-order properties exist.

"Laws of nature – a sceptical view" is presented by Uwe Meixner. After examining van Fraassen's sceptical argument as well as the so-called TAD-approach (Tooley/Armstrong/Dretske), Meixner explains his own idea. He sees a way out of the sceptical dilemma, i.e. the tension between laws of nature transcending the phenomena, on the one hand, and keeping these laws within our epistemic reach, on the other hand. The solution, he thinks, can be achieved by 'relativising' laws of nature to our beliefs and decisions. These laws are made by us and they can be rescinded by us.

There is an obvious connection between new scientific discoveries, new laws and new properties. To understand laws we need an understanding of properties, and this is discussed in Johannes Persson's "The law's properties". He presents a framework for distinguishing between properties and fake properties that allows us to handle questions concerning the ontological status of laws. Furthermore, this approach proves suitable in discussing tests for properties suggested by Maxwell, Ramsey and Cartwright.

Gerhard Schurz considers "Laws of nature versus system laws". The first are fundamental laws of physics that hold everywhere in the universe. System laws concern some specific systems in a given time under concrete specification of all forces acting within or upon the system. Schurz draws a three-fold distinction which recognises that: 1) *ceteris paribus* clauses are needed for system laws, not for laws of nature; 2) there are universal conditionals functioning as system laws; and 3) most system laws are fundamental (i.e. non-derived), whereas some laws of nature are derived.

The volume ends with Werner Stelzner's paper on "Psychologism, universality and the use of logic". Here we find another perspective on the overall theme of the book. Assuming both rules of nature and of language, Stelzner asks what is specific about rules of logic? Rules of logic are treated as special language rules that span the realm for explicating and systematising rules of nature. In order to make this clear, he paints a very detailed picture of the rise of modern logic between psychologism and antipsychologism.

References

D. Armstrong. *Universals and Scientific Realism*, 2 volumes. Cambridge University Press, Cambridge, 1978.

D. Armstrong. *What is a Law of Nature?* Cambridge University Press, Cambridge, 1983.

H. B. Callen. *Thermodynamics and an Introduction to Thermostatistics*. John Wiley, New York, 1985.

R. Carnap. *The Philosophical Foundations of Physics*. Basic Books, New York, 1966.

N. Cartwright. *How the Law of Physics Lie*. Clarendon, Oxford, 1983.

N. Cartwright. *Nature's Capacities and their Measurement*. Clarendon, Oxford, 1989.

N. Cartwright. *The Dappled World*. Cambridge University Press, Cambridge, 2000.

D. Davidson. *Essays on Actions and Events*, "Actions, Reasons and Causes", pages 3–19. Oxford, 1980.

D. Davidson. *Essays on Actions and Events*, "Causal Relations". Oxford, 1980.

F. Dretske. Laws of nature. *Philosophy of Science*, 44:248–268, 1977.

P. Duhem. Une nouvelle théorie du monde inorganique. *Revue des questions scientifiques*, 33:99–133, 1893.

P. Duhem. *The Aim and Structure of Physical Theory*. Princeton University Press, Princeton, 1991.

N. Goodman. The problem of counterfactual conditionals. *Journal of Philosophy*, 44:113–128, 1947.

N. Goodman. Review of Reichenbach (1947). *Philosophical Review*, 58:100–102, 1948.

N. R. Hanson. *Patterns of Discovery*. Cambridge University Press, Cambridge, 1958.

D. Hume. *A Treatise of Human Nature*. Clarendon Press, Oxford, 1958.

C. G. Hempel. Studies in the logic of confirmation. *Mind*, 54, 1945. reprinted in *Aspects of Scientific Explanation*, Free Press, Collier-Macmillan, Toronto, 1965.

C. G. Hempel. Review of Reichenbach (1954). *Journal of Symbolic Logic*, 10:54–55, 1955.

Hempel, C. G. (1965). *Aspects of Scientific Explanation*. The Free Press, New York.

Hempel, C. G. and Oppenheim, P. (1948). Studies in the logic of explanation. *Philosophy of Science*, 15:135–175. (Reprinted in Hempel, 1965: 245–290).

E. K. Jobe. Some recent work on the concept of law. *Philosophy of Science*, 34:363–381, 1967.

W. A. Kneale. Natural laws and contrary-to-fact conditionals. *Analysis*, 10, 1950.

W. A. Kneale. Universality and necessity. *The British Journal of Philosophy of Science*, 12, 1961.

T. B. Kuhn. *The Structure of the Scientific Revolutions*. The University of Chicago Press, Chicago, 1962/70.

G. W. Leibniz. *Leibniz: Selections*, "The Metaphysical Foundations of Mathematics". Charles Scribner's Sons, New York, 1951.

D. Lewis. *Counterfactuals*. Basil Blackwell, Oxford, 1973.

D. Lewis. *On the Plurality of Worlds*. Basil Blackwell, Oxford, 1986.

D. Lewis. *Philosophical Papers*. Lewis, 1986b, chapter Counterfactual Dependence and Time's Arrow (incl. Postscripts), pages 32–66.

D. Lewis. *Philosophical Papers*, volume II. Oxford University Press, Oxford, 1986.

E. Nagel. *The Structure of Science*. Routledge & Kegan Paul, London, 1961.

P. Needham. Duhem's physicalism. *Studies in History and Philosophy of Science*, 29:33–62, 1998.

P. Needham. Duhem and Quine. *Dialectica*, 54:109–132, 2000.

A. Pap. Disposition concepts and extensional logic. In M. Scriven H. Feigl and G. Maxwell, editors, *Minnesota Studies in the Philosophy of Science*, volume 2, Minnesota, 1958. University of Minnesota Press.

H. Poincaré. *Science and Hypothesis*. Dover, New York, 1952.

K. R. Popper. *The Logic of Scientific Discovery*. Hutchinson, London, 1959.

F. P. Ramsey. *Foundations: Essays in Philosophy, Logic, Mathematics and Economics*. Routledge & Kegan Paul, London, 1978.

T. Read. *The Works of Thomas Read*, "Essays on the Intellectual Powers of Man". Ed. by Sir W. Hamilton. Edinburgh, 7th edition, 1872.

H. Reichenbach. *Elements of Symbolic Logic*. Macmillan, London, 1947.

H. Reichenbach. *Nomological Statements and Admissible Operations*. North Holland, Berkeley and Los Angeles, 1954.

Reissued under the title *Law, Modalities and Counterfactuals*, University of California Press.

H. Reichenbach. *The Philosophy of Space and Time*. Dover, New York, 1957. trans. of *Philosophie der Raum-Zeit-Lehre*, (Berlin, 1928).

B. Stroud. *Hume*. Routledge & Kegan Paul, London, 1977.

M. Tooley. The nature of law. *Canadian Journal of Philosophy*, 7:667–698, 1977.

B. C. van Fraassen. *The Scientific Image*. Clarendon Press, Oxford, 1980.

B. C. van Fraassen. *Laws and Symmetry*. Clarendon Press, Oxford, 1989.