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6 | COMMENTARY

6.1 | Hempel's D-N Model

The phrase *deductive-nomological* is an apt description of the model that Hempel first proposed in his classic paper published in 1948.[1] For according to Hempel, many scientific explanations are deductively valid arguments having at least one statement of an empirical law in their premises. (The adjective *nomological* is derived from the Greek word *nomos*, meaning "law.") Only later, in 1962, did Hempel focus his attention on statistical explanation.[2] He then proposed his I-S model, according to which statistical explanations are inductive arguments with at least one statement of an empirical statistical law in their premises. Thus, both models are instances of the covering law thesis that all explanations are arguments (either deductively valid or inductively strong) that must involve laws. Before discussing Hempel's models in detail, it will be helpful to consider the underlying motivation for Hempel's proposals.

CARNAP ON THE MOTIVATION FOR HEMPEL'S D-N MODEL

As Rudolf Carnap points out in "The Value of Laws: Explanation and Prediction," scientists and philosophers have not always valued theories for their explanatory power. In the second half of the nineteenth century, for example, philosopher-scientists such as Ernst Mach and Pierre Duhem insisted that the proper function of scientific theories was not to explain phenomena but merely to classify and summarize experimental laws.[3] Duhem defines explanation as follows: "To explain (explicate, *explicare*) is to strip reality of the appearances covering it like a veil, in order to see the bare reality itself."[4] Duhem then argues that if scientific theories are intended to explain (in his own sense of stripping reality bare), then they will inevitably make science subordinate to metaphysics. The kinetic theory of gases and the wave theory of light, for example, posit the existence of atoms and the optical aether as an essential part of the explanations they provide. But neither atoms nor the aether can be observed directly. At best claims about these "bare realities" can be tested only indirectly by seeing what experimental laws they entail. Moreover, these experimental laws are compatible with a wide range of different assumptions about atoms and the aether. Thus, testing will be unable to confirm any particular theory (or the unobservable entities it posits) relative to its rivals. So, Duhem concludes, the assertion that atoms exist or that there really is an aether are metaphysical claims that empirical science can neither confirm nor refute.[5] Explanation is not, and cannot be, one of the aims of scientific theories.

Carnap rejects the claim of Duhem, Mach, and other positivists that science cannot explain; nevertheless he is sympathetic to their skepticism about theories involving metaphysical assumptions about unobservable entities and causes. Carnap insists that scientific theories can give genuine explanations but only if the explanations involve testable, empirical laws. Consider the example Carnap discusses of the German biologist and philosopher Hans Driesch (1867–1941). Driesch, who had done pioneering work on embryology and limb regeneration in sea urchins, discovered in 1891 that prior to the fifth division of a fertilized sea-urchin's egg any cell was capable of developing into a complete embryo. These and other findings led Driesch to espouse a "vitalistic" account, according to which an inner force or purpose in each living organism—what Driesch called an *entelechy*—is responsible for maintaining the integrity of the organism, directing development, and regenerating lost parts. In a number of works, Driesch championed his entelechy theory as an explanation for biological and psychological phenomena. He saw an important similarity between an embryo developing into an adult organism and a person voluntarily deciding to perform one action rather than another. In both cases, he argued, we cannot predict what will happen on the basis of the laws of physics and chemistry. We can explain what has happened after the event, but only because we then know how things have turned out. Driesch regarded the power of a human being to choose which action to perform as a refined manifestation of the teleological, directive power of the entelechy that is in every living creature. And what is an entelechy? It is a nonphysical, nonmaterial, nonspatial inner force that directs everything that an organism does, from cellular processes to voluntary actions. Every living thing has an entelechy, each species has its own distinctive kind, and it is possession of an entelechy that distinguishes living things from machines.

Despite their respect for Driesch's scientific work and his genuine desire that philosophy take science seriously, Carnap and Reichenbach could not accept that Driesch's entelechy theory really explained anything. In defending the entelechy theory against this accusation, Carnap recalls Driesch retorting that his introduction of the term *entelechy* to explain the behavior of organisms was no different from physicists introducing the term *magnetism* to explain the behavior of magnets and bits of iron. After all, we can neither see nor touch the force of magnetism. All we can actually observe is the motion of bodies, which magnetism was posited to explain. Similarly, Driesch maintained, it is legitimate for him to introduce entelechies to explain biological phenomena. Carnap responded by pointing out that the cases are relevantly different. For when physicists introduced the term *magnetism*, they did not simply posit the existence of an unobservable entity; they also specified laws that magnetized bodies must obey. These laws can be used to make predictions that can be tested by experiment and observation. Driesch's entelechy theory specifies no such laws and is thus completely lacking in predictive power. Therefore, Carnap concludes, Driesch's theory does not give genuine explanations.

Our discussion of Carnap's criticisms of Driesch's entelechy theory brings to light one of the central motivations for Hempel's D-N model of explanation, namely, the requirement that every genuine scientific explanation include at least one empirical law in its explanans. Another closely related theme is Carnap and Hempel's insistence that explanation and prediction go hand in hand: every genuine explanation must be capable of predicting its explanandum. Later we will focus our attention on Hempel's defense of what he calls the *thesis of structural identity*, that, in all formal respects, explanations are the same as predictions, and predictions the same as explanations.

HEMPEL'S CONDITIONS OF ADEQUACY FOR D-N EXPLANATION

The requirement that the explanans include at least one empirical law is obviously not sufficient for an explanation for two reasons. First, just including a law, any law, is not enough; the law must be essential to the derivation of the explanandum. Clearly, we would not be much impressed by a purported explanation in which the laws mentioned were entirely irrelevant to the event or phenomenon needing explanation. Second, all by themselves laws do not entail that any specific thing will happen. Thus, when we seek to explain the occurrence of an event, we must also include in the explanans statements of various initial conditions that, in conjunction with the laws, logically imply the explanandum.

We thus arrive at the general scheme of Hempel's D-N model of explanation, which he summarizes in the selection "Two Basic Types of Scientific Explanation":

$C_1, C_2, \ldots, C_k$	Statements of particular facts and initial conditions	Explanans
$L_1, L_2, \ldots, L_r$	General laws	
E	Description of the event, law, or fact to be explained	Explanandum

In his 1948 article, Hempel gave the criteria for a D-N explanation in the form of four conditions of adequacy, which he divided into two groups: logical and empirical.

LOGICAL CONDITIONS OF ADEQUACY FOR D-N EXPLANATION

*R*1 The explanandum must be a logical consequence of the explanans.

*R*2 The explanans must contain general laws, and these must be essential for the derivation of the explanandum.

*R*3 The explanans must have empirical content; that is, it must be capable, at least in principle, of test by experiment or observation.

EMPIRICAL CONDITION OF ADEQUACY FOR D-N EXPLANATION

*R*4 The sentences in the explanans must be true.

Before discussing Hempel's defense of the thesis of structural identity (that is, his thesis that explanation and prediction are formally identical) and his other model of explanation (the I-S model), a few brief remarks about Hempel's criteria of adequacy for a D-N explanation will be helpful in understanding criticisms of his views. We offer brief comments on each criterion in turn.

Why does Hempel require that the explanans logically entail the explanandum? The reason is simple. If we have genuinely explained why the explanandum event has occurred, then we must have given sufficient grounds for expecting that the event in question would occur. After all, the question "Why did E_1 occur?" usually arises when other outcomes seem possible: why did E_1 occur, rather than, say, E_2 or E_3? A completely satisfactory answer to this question would thus show that E_1 was the only event among the possible alternatives that could have occurred. Ideally, then, an explanation must be a logically valid argument, for in a valid argument, if the premises are true, then the conclusion also has to be true. The best kind of explanation is one in which, given the information in the explanans, the explanandum has to be true. If it were still possible, given the explanans, for the explanandum to be false, then we would not have explained, fully and completely, why the explanandum event occurred. As we shall see later in this commentary, Hempel is forced to relax this deductive standard of explanation in order to accommodate statistical explanations of particular events.

Our discussion of Carnap's criticisms of Driesch has already touched on the motivation for requiring that every explanation contain at least one law. Two additional features of this requirement are noteworthy. First, Hempel does not require that the laws be causal. As far as Hempel is concerned, there can be perfectly satisfactory scientific explanations solely in terms of Snell's law, Hooke's law, the equation of state for an ideal gas, or the like. These laws, expressed in the form of equations, are sometimes called *functional laws* because they specify the mathematical function relating the value of one variable (such as pressure) to the value of other variables (such as temperature and volume). (For more on functional laws, see the section "The 'Missing Values' Problem for Functional Laws" in the commentary on chapter 7.) Thus, Hempel deliberately does not limit scientific explanation to causal explanation. A second noteworthy feature of (R2) and the other logical conditions of adequacy is that Hempel does not require that the explanans contain any statements of initial conditions. Of course, such initial conditions must be included in any deductive explanation of the occurrence of a particular event, but Hempel wants his model to cover not only the explanation of events but also the explanation of laws. That is, he wants his model to apply to cases in which one law or a set of general laws (such as Newton's laws of motion and gravitational attraction) explain another law (such as Kepler's second law) by deductively implying it. (The explanation of one law by another, more general, law or theory is discussed in chapter 8. In the present chapter, the focus is on the explanation of events.)

Condition (R2) mentions only general laws, which would include laws of mathematics. While laws of mathematics often play an important role in scientific explanations, Hempel requires that at least one of the laws in the explanans be empirical. It is perfectly legitimate for the explanans to include mathematical laws (such as the Pythagorean theorem or the principles of algebra), but it must also include at least one empirical law that, unlike the laws of mathematics, can be tested by observation or experiment. Strictly speaking, (*R*3) is redundant, since it follows from (*R*1): anything that logically implies the explanandum, which has empirical content, must itself have empirical content. But such is the importance of (R3) to the spirit of Hempel's conception of explanation that it is listed as a separate requirement.

The three conditions (*R*1), (*R*2), and (*R*3) are grouped together under the heading of logical conditions of adequacy in Hempel's 1948 article because he thinks that we can tell whether something satisfies them without our needing any empirical information about the world.[6] But the last condition, (*R*4), is different. This condition requires the sentences in the explanans to be true. But for most if not all of these sentences their truth or falsity can only be determined empirically.[7] Elsewhere, Hempel calls a set of sentences that satisfies the logical conditions of adequacy a *potential explanans*. It is only when those sentences are actually true that we have a genuine explanation. As far as the D-N model is concerned, Hempel's notion of a correct or genuine explanation is strongly objective. A group of scientists may believe that they have given an explanation, and they may indeed have considerable justification for their belief, but unless the potential explanans they provide is actually true, no genuine explanation has been given. Thus, for example, Hempel would deny that the phlogiston theory of chemistry explained why metals burn in air (calcination). Why? Because the phlogiston theory is, as a matter of fact, false. Of course, we can say that phlogiston theory explained the calcination of metals, and this is often the simplest way of expressing ourselves. But if we accept Hempel's empirical truth condition (*R4*) for explanation, then what we really mean is that the phlogiston theory would have explained the burning of metals in air if it had been true.

ELLIPTICALLY FORMULATED EXPLANATIONS

Hempel recognizes that the explanations scientists actually give often fall short of the conditions of adequacy for D-N explanation. But Hempel does not see this failure of his model to describe actual scientific practice as in any way refuting the D-N model as an account of scientific explanation. Partly this is because Hempel's model is intended to be normative rather than merely descriptive: his aim is to articulate an ideal of what a good scientific explanation *should* be like, not simply to summarize or describe the explanations that scientists actually give. Hempel arrives at his model not by ignoring scientific practice, but by reflecting on paradigmatic examples of scientific explanation (such as those mentioned in his articles) and then isolating their essential features. With his D-N model clearly formulated, Hempel thinks he can explain why many actual scientific explanations fail to satisfy his model by appealing to pragmatic factors.

In many respects, Hempel's attempt to clarify the concept of explanation is similar to the analysis of the concept of proof given by logicians and philosophers of mathematics. The actual proofs that mathematicians write down often leave out steps (considered obvious or trivial), but this in no way undermines or refutes the strict, formal concept of proof that serves as our ideal.

The most common way in which the explanations given by scientists deviate from Hempel's D-N model is by being incomplete. For example, why does ice float on water? Most scientists would accept as a satisfactory explanation the assertion that, unlike most substances, water expands when it freezes. This is an instance of what Hempel calls an *elliptically formulated explanation*. Some laws or facts are left out of the explanans of such explanatory arguments. Why? Because the relevant laws and facts (such as Archimedes' principle of buoyancy and the inverse relation between density and volume) are so well known and accepted (at least by other scientists) that to write all of them down would be tedious and a waste of time. Consequently, the explanandum does not actually follow logically from the explanans, but the missing material can easily be filled in, and once it is filled in, the amended explanans does logically imply the explanandum. So this kind of deviation from the D-N model is innocuous and justified on pragmatic grounds. Far from refuting the D-N model, such examples actually confirm it, since the D-N account is able to explain in a plausible way why so many actual explanations are incomplete, and in making them more complete we come closer to the D-N ideal.

PARTIAL EXPLANATIONS AND EXPLANATION SKETCHES

There are other, more radical, kinds of explanatory incompleteness discussed by Hempel, namely *partial explanations* and *explanation sketches*. In a partial explanation, even when all the missing premises are added to the explanans, it still does not logically imply the explanandum. What does follow from the supplemented explanans is not the original explanandum but something more general. For example, we might seek to explain why some kinds of apple turn from green to red as they ripen. Suppose that we give an explanation but, even when our explanans is filled out, what it actually entails is that apples change color as they ripen. Since the change from green to red in some kinds of apple is a special case of this more general sort of change, to that extent, Hempel is prepared to call such explanations "partial." But strictly speaking, a partial explanation does not actually explain the explanandum it was invoked to explain. Hempel offers a similar example from the *Psychopathology of Everyday Life* in which Freud attempts to explain why he wrote the wrong date in his diary. What Freud actually explained (even when his explanans is filled out with the psychological facts and the presumed laws of Freudian psychology that are missing from his account) is not the particular slip he made about the date, but why he performed an action that, in some way or other, symbolically represented the fulfillment of one of his subconscious wishes.

The final category of incomplete explanations, *explanation sketches*, consists of those explanatory accounts that are so vague and incomplete—in short, so sketchy—that they fail to qualify as either elliptical or partial explanations. At best, an explanation sketch provides a general outline that might prove capable of being developed into a satisfactory explanation at some future time.

6.2 | Hempel's D-N Model and the Thesis of Structural Identity

In "The Thesis of Structural Identity," Hempel formulates his thesis as the conjunction of two claims or subtheses:

> every adequate explanation is potentially a prediction; and
>
> every adequate prediction is potentially an explanation.

Because Hempel's thesis asserts that there is a symmetry between explanation and prediction, his thesis of structural identity is often called the *symmetry thesis*, and criticisms of the thesis are instances of the *symmetry* *objection*. In his article, Hempel attempts to defend both parts of his thesis of structural identity against alleged counterexamples.

In order to understand Hempel's defense, several features of his thesis need to be underscored. First, although many instances of the symmetry objection focus on deductive explanations, Hempel believes that his thesis applies to both D-N and I-S explanations.

Second, Hempel uses the term *prediction* in a special sense. Ordinarily, we would say that Jones has predicted the outcome of the Super Bowl if he announces the name of the winning team before the game is played. In this sense, a prediction is simply a statement that need not be accompanied by any supporting reasons or argument. But since Hempel is primarily interested in the explanations and predictions given by scientists, he deliberately restricts the meaning of the term *prediction* to *predictive argument*. Without this restriction, his thesis would be obviously false.

Third, Hempel intends his models of explanation to cover both the explanation of laws and the explanation of events. But it makes little sense to talk about the prediction of laws, since laws are not the sort of things that happen at any particular time. So Hempel implicitly restricts his symmetry thesis to the explanation and prediction of events.

Finally, Hempel recognizes that we can sometimes use laws and theories to make inferences about what has happened in the past on the basis of initial conditions and particular facts that hold at a later time. These inferences are often called *postdictions* or *retrodictions*, and scientists ordinarily regard them as a species of prediction, for just like predictions about the outcome of an experiment, postdictions involve the deduction from a law or theory of something whose truth is not yet known. But Hempel is careful to exclude postdictions from the scope of his symmetry thesis. The reason is obvious: even though we can use Newtonian mechanics to infer from the present positions of the sun, the moon, and the earth that a total solar eclipse occurred two thousand years ago, we cannot accept that the way things are now explains the way things were two thousand years ago; present events cannot explain past events. So the symmetry thesis is concerned solely with predictive arguments in Hempel's restricted sense, that is, with arguments in which all the initial conditions and particular facts hold at times prior to the event described in the conclusion.

With these preliminaries in place, we may now consider objections to the symmetry thesis. Counterexamples to the first subthesis must be adequate explanations that are not potentially predictions; counterexamples to the second subthesis must be adequate predictions that are not potentially explanations. We begin with three objections to the first subthesis—alleged examples of adequate explanations that could not have been used to predict the events that they explain. For convenience, we shall assign them titles.

THE SYPHILITIC MAYOR

Michael Scriven asks us to consider a certain medical patient—the mayor of his town, let us suppose—whose name is Jones. Consider the question, Why did the mayor, Jones, contract paresis? Paresis is a form of general paralysis that affects only those who have had untreated syphilis for many years. This being so, the answer to our question is, Because Jones had untreated syphilis, and the only cause of paresis is syphilis. This, allegedly, is an explanation of why Jones now has paresis. But since only 10 percent of untreated syphilitics go on to develop paresis, Jones's syphilis could not have been used to predict his paresis. Indeed, since 90 percent of syphilitics do not contract paresis, we would have predicted exactly the opposite.

Hempel's reply to this objection is short and sweet: no adequate explanation has been given. Merely to cite one condition that is necessary (but not sufficient) for the occurrence of an event—even if that condition is based on a law—is not to explain that event. Presumably, it is a law that all people who die have needed to breathe oxygen while they were alive. But we do not suppose that we have explained why someone has died merely by pointing out that the person was an oxygen breather. Similarly, to use one of Hempel's own examples, we do not think that we have explained why a particular person won the Irish sweepstakes simply by adducing the fact that the person had bought a ticket.

Many people have judged Hempel's reply to be unsatisfactory, in part because, in the syphilitic mayor example, we know of no other factors that can influence the chance of someone's being afflicted with paresis. There is thus an understandable tendency to regard this example as an event of low probability being explained by a statistical law. So what may in fact be at issue here is whether we should insist, as Hempel does, that any statistical explanation confers high probability on its explanandum. Hempel's high-probability requirement will be discussed later when we examine the I-S model and the criticisms of it.

EVOLUTIONARY THEORY

Several versions of the evolutionary theory objection have been raised by Hempel's critics. In its simplest form, the objection runs as follows:

> Darwin explained the origin of species using this theory of natural selection working on random variations. Scientists accept that Darwin's theory offers genuine explanations, yet no scientist has been able to use Darwin's theory to predict the coming-into-existence of any new species. Thus, evolutionary theory explains but it does not predict.

In reply, Hempel stresses the importance of distinguishing between what he calls the *story* of evolution and the *theory* of evolution. The story of evolution is a narrative describing the sequence of species that have arisen and become extinct since life first appeared on earth. Even if this narrative is completely true, it has no explanatory import whatever. It is merely a description of what has happened in the past. The theory of evolution, by contrast, employs generalizations about heredity, mutation, and selection plus a host of detailed assumptions about environmental conditions and ecological relations. At best, this theory can offer only partial, probabilistic explanations of general facts about species survival and extinction. What it cannot do, at least in the present incomplete state of our biological knowledge, is explain why any particular species came into existence when it did. In short, by arguing that evolutionary theory explains considerably less than one might have supposed, Hempel denies that evolutionary theory explains what it is unable to predict. Biologists may have reasonably good explanations of why, following the extinction of the dinosaurs, other species were able to flourish and evolve, but they cannot yet explain why, say, a particular species of rat or aardvark evolved when it did, with the characteristics it did. The random nature of the variations on which natural selection works precludes explaining or predicting anything very detailed about the coming-to-be of new species. Like the syphilitic-mayor objection, then, this one fails because it is based on a false assumption (about the supposed extent of explanation offered by evolutionary theory).

A closely related objection to Hempel's theory, also involving evolutionary theory, was raised by Michael Scriven.[8] Even after a new trait has appeared in an organism, it is difficult to know whether that trait is adaptive. In particular, it is hard to judge the magnitude of the advantage, if any, that a new trait (such as a larger shell) confers on its possessor. Moreover, there is an ineliminable element of chance involved in determining which individuals will actually survive. Even the fittest giraffe might be killed by lightning before having the opportunity to mate and produce offspring. But once natural selection has operated for some time on many thousands or millions of individual organisms, it is much easier to identify which traits are adaptive. Clearly, we must confront the threat of a circularity of definition here—namely, that of simply identifying the fittest organisms with those that actually survive.[9] But that aside, we must grant that sometimes the information needed to explain an event can be obtained only by making inferences from the fact that the event in question has actually occurred. Hempel refers to explanations having this feature as *self-evidencing* explanations. This notion plays a major role in Hempel's treatment of the next objection, also proposed by Scriven, of a collapsing bridge.

SCRIVEN'S BRIDGE

Sometimes the only ground we have for asserting that some statement in the explanans is true lies in our knowledge that the explanandum event did, in fact, occur. This feature is at work in Scriven's example of the collapsed bridge. The collapse tells us not only that metal fatigue occurred, but that it was serious enough to cause the failure of the entire structure. Similarly with the man who kills his wife out of jealousy or the patient who develops skin cancer after exposure to ultraviolet light: in all such cases, we could not have predicted the relevant events, but we can nevertheless explain them after they occur. Here again, contrary to the symmetry thesis, we seem to have explanations that are not predictions.

Hempel agrees that Scriven's bridge is a case in which we would not have had all the information necessary for predicting the collapse prior to its occurrence. But, Hempel insists, this does not mean that Scriven has given a counterexample to the first subthesis because, when interpreted correctly, that subthesis makes merely a *conditional* claim, namely:

> if all the information in the explanans had been known and taken into account before the occurrence of the explanandum event, then the event could have been predicted.

What Scriven has done is to show that, in some cases, the antecedent of this conditional is not satisfied. But this does not show that the conditional is false.

Hempel introduces the term *self-evidencing* to describe those explanations in which the information that the explanandum statement, E, is true provides a crucial evidential support for one of the particular statements in the explanans, C_1. Hempel insists that such explanations are not circular, since they are not being used to establish that E is true. As with any explanation, we already know (or presume that we know) that E is true, that the explanandum event happened as described. Thus, although we are using E as part (or even the whole) of the evidence for C_1, we are not then using C_1 as evidence for E. Hence, there is no epistemic circularity. Moreover, although we are using C_1 to explain E, we are not also using E to explain C_1. Hence, there is no explanatory circularity.

Hempel concedes that the second subthesis of the symmetry thesis is less secure than the first. That is, Hempel admits that there seem to be adequate predictions that are not potentially explanations. An example of his own invention, involving the association between Koplik spots and the measles, serves to illustrate the problem.

KOPLIK SPOTS

Koplik spots are tiny, whitish spots that appear on the inside of one's cheeks about a week before one succumbs to a full-blown case of measles. Supposing that the appearance of Koplik spots is *always* followed by the measles, the connection between them may be judged lawlike. Such a law can then be used to predict that a patient with Koplik spots will have measles a week later. Still, the Koplik spots do not explain why the patient will develop full-blown measles in a week's time.

Hempel suggests that our reluctance to regard the Koplik-spots argument as explanatory likely reflects our doubts about whether measles do in fact always follow the spots as a matter of universal law. Perhaps, he conjectures, we could produce Koplik spots by injecting a small quantity of the measles virus into someone's cheek without the spots then being followed by full-blown measles. But this response to the Koplik spots case is not entirely satisfactory. For even if the relation between Koplik spots and the measles is not one of universal law but merely one of high probability, it remains unclear why the resulting argument would not satisfy the conditions of Hempel's I-S model and hence qualify as a statistical explanation. (Hempel's I-S model is discussed in section 6.3 of this commentary.)

To some of his critics, Hempel's models of explanation seem vulnerable to the Koplik spots example because the models include no condition that mentions causation. Why do the Koplik spots fail to explain the later case of full-blown measles? Because the spots are not the cause of the measles. Rather, the spots and the full-blown measles are both joint effects of a common cause, namely, infection with the measles virus. In just the same way we can use the falling reading on a barometer to predict that a storm is approaching, but we do not take the barometer reading to be the cause of the storm's approach, nor do we take it to be an explanation of its approach. Again, we have prediction without explanation. The falling reading on the barometer and the approach of the storm are joint effects of a common cause, namely, a drop in atmospheric pressure. (In "Arguments, Laws, and Explanation," David-Hillel Ruben examines whether Hempel's models of explanation can be rescued by requiring that the explanans contain a statement describing the cause of the explanandum event.)

THE FLAGPOLE AND THE PENDULUM

Although Hempel does not consider these particular examples, they are often cited by critics of the symmetry thesis. A flagpole of height H, casts a shadow of length S. Given the law that light travels in straight lines, and the elevation of the sun, θ, we can deduce the length of the shadow from the height of the flagpole. Thus, we have both an adequate prediction of S and, let us assume, an explanation of why the shadow has that particular length by using the equation $H = S \tan \theta$. But given that same equation, we could have just as easily deduced the height of the flagpole from the length of its shadow. Although perfectly fine as a prediction, this would not be accepted as an explanation: the length of the shadow does not explain the flagpole's height. The pendulum example is similar. From the period of a simple pendulum (the time it takes to perform one complete oscillation) we can deduce (i.e., predict) the length of the pendulum, but we do not think that the pendulum's period explains its length. Thus, on the face of it, the flagpole and the pendulum seem to provide plausible counterexamples to the second subthesis: not every adequate prediction is potentially an explanation.

6.3 | Hempel's I-S Model

As we have seen, Hempel's D-N model construes explanations as deductive arguments. But Hempel was aware all along that some scientific explanations could not be reconstructed in this fashion. This is especially true of theories in physics and genetics that use probabilistic laws to explain particular events. In 1962, Hempel turned his attention to these other, nondeductive arguments and formulated his inductive-statistical (I-S) model of explanation.

HEMPEL'S CONDITIONS OF ADEQUACY FOR I-S EXPLANATION

Although by 1962 Hempel no longer required that all explanations deductively entail the events they explain, he continued to defend the covering law thesis: just like D-N explanations, I-S explanations must have at least one (in this case statistical) law among their premises. Hempel also retained a substantial part of the concept of explanation that motivated the D-N model. Like their D-N counterparts, I-S explanations are still arguments, and while the conclusions of I-S arguments no longer follow from their premises with logical necessity, Hempel insisted that the explanans must make the explanandum highly probable. The higher the probability, the stronger the argument and the better the explanation. The strength of an I-S explanation is measured by the inductive probability of the conclusion relative to its premises. Hempel assumed that this strength is equal to the numerical value of the probability given by the statistical law in the explanans. Thus, given the statistical law that *F*s are very likely to be (or be followed by) *G*s, and given some particular fact that *a* is *F*, it is very likely that *a* is *G*. An I-S explanation therefore has the following schematic form:

$$\begin{array}{ll} \left.\begin{array}{ll} P(G/F) = 0.95 & \text{Statistical law} \\ Fa & \text{Particular fact} \end{array}\right\} & \text{Explanans} \\ \hline\hline \qquad [0.95] & \\ Ga & \text{Explanandum} \end{array}$$

Note that the conclusion of this I-S argument is not "*a* is almost certain to be *G*" but the unqualified statement "*a* is *G*." It is the fact that *a* is *G* that this inductive argument purports to explain. Hempel insists that expressions such as "*a* is almost certain to be *G*" are incomplete and thus neither true nor false. All meaningful, empirical statements of probability must be qualified as being relative to some body of evidence. It is either true or false that *a* is or will be *G*: what the full I-S explanation expresses is that the truth of *Ga* is very probable relative to the statistical law and the particular facts.

From everything we have said thus far, D-N explanations would seem to be a limiting case of I-S explanations: when the argument is deductively valid, the inductive probability of the conclusion relative to the premises is 1. Hempel denies this. He sees I-S explanations as essentially different from D-N explanations because, he insists, I-S explanations must be relativized to a particular "knowledge situation" (712). This epistemic relativity of I-S explanations arises from the requirement of maximal specificity (RMS), which Hempel imposes on all statistical explanations of particular events as a way of solving a certain problem of ambiguity that infects such explanations.[10] Hempel's RMS, and the problem of ambiguity it is meant to solve, are discussed in the next section. For the present, we will summarize the conditions of adequacy for an I-S explanation of a particular fact or event.

■ LOGICAL CONDITIONS OF ADEQUACY FOR I-S EXPLANATION

S1 The explanandum must follow from the explanans with high inductive probability.

S2 The explanans must contain at least one statistical law, and this must be essential for the derivation of the explanandum.

S3 The explanans must have empirical content; that is, it must be capable, at least in principle, of test by experiment or observation.

■ EMPIRICAL CONDITIONS OF ADEQUACY FOR I-S EXPLANATION

S4 The sentences in the explanans must be true.

S5 The statistical law in the explanans must satisfy the requirement of maximal specificity.

THE PROBLEM OF AMBIGUITY IN STATISTICAL EXPLANATION

As explained in his article, "Inductive-Statistical Explanation," Hempel proposed his RMS in response to the problem of ambiguity in statistical explanation. The problem of ambiguity can be regarded as an instance of a more general difficulty arising whenever we wish to use statistical information about classes of cases or events to decide the probability of a single case or event. This is the so-called problem of the single case: for any event that we wish to explain, there will be many different reference classes to which the event could be assigned; each choice of a reference class will present us with a different statistical law, and often these laws will have different probabilities associated with them. Let us consider one of Hempel's own examples.

We wish to explain why a particular day, *n*, November 27 in Stanford, has the property, *W*, of being warm and sunny. Thus the explanandum is *Wn*. Among the many reference classes to which *n* belongs is (on the one hand) the class, *N*, of November days in Stanford, and the probability of warm weather on such a day, $P(W/N)$, is 0.95. So if we assign *n* to the reference class *N*, the high-probability condition is met and, apparently, we have an I-S explanation of *Wn*. That is, we have explained why November 27 in Stanford was warm and sunny by pointing out that *n* belongs to class *N* and citing the statistical law that says that the probability of warm and sunny days in that class is very high. But November 26 in Stanford was cold and rainy, and so *n* also belongs (on the other hand) to a different reference class, *S*, of immediate successors of cold and rainy days in Stanford. Assume, with Hempel, that $P(W/S) = 0.2$. Thus, $P({\sim}W/S) = 0.8$, which we may agree qualifies as high. Now if, contrary to fact, *n* had *not* been warm and sunny, then we could have used the law $P({\sim}W/S) = 0.8$ to explain why November 27 in Stanford was *not* warm and sunny. This, to Hempel, is intolerable. Of course, since *n* was warm and sunny, we could not use $P({\sim}W/S) = 0.2$ to explain *Wn*. But, nonetheless, without some further condition of adequacy for I-S explanations, we are in the position of being able to "explain" the weather on November 27 in Stanford either way, whether the day was warm and sunny or not. For Hempel, this possibility of "explaining" an event whether or not it occurred means that no genuine explanation has been given at all.

We can state the problem of ambiguity as follows: given an I-S explanation with true premises of some explanandum, *Ga*, there will often be another I-S explanation with true premises and conclusion, $\sim Ga$. For convenience, let us call these two arguments (1) and (2):

$$\begin{array}{ll} \text{Argument 1} & \text{Argument 2} \\ P(G/F) = 0.95 & P(\sim G/H) = 0.96 \\ \underline{Fa} & \underline{Ha} \\ \overline{\overline{\quad\quad\quad}}\ [0.95] & \overline{\overline{\quad\quad\quad}}\ [0.96] \\ Ga & \sim Ga \end{array}$$

Now, it might be thought that ambiguity is not really a problem at all. Since an explanation is sought only once the explanandum event has occurred, we would never actually accept both (1) and (2) as correct explanations. But Hempel judges this way of dismissing the problem to be unsatisfactory. Why? Because it is still the case that the premises of both (1) and (2) are true, and, let us assume, both are contained in our body of knowledge, *K*. (More will be said about Hempel's notion of a body of knowledge presently.) So if *a* had turned out not to be *G*, we could have given an equally strong explanation for ~ *Ga*. In what sense, then, have we explained the fact that *Ga* if by appeal to truths in *K* we could just as well have explained the fact that ~ *Ga*? To put the point in what may be a clearer form: if the essence of explanation is nomic expectability (that is, predictability based on laws), then we cannot accept that (2) would be just as good an explanation as (1) if *a* had failed to be *G*. Clearly, there is a connection between Hempel's concerns over explanatory ambiguity and his commitment to the thesis of structural identity (the symmetry thesis) between predictions and explanations. *K* contains the premises of (1) and the premises of (2). Thus, if we are justified in predicting *Ga*, we would also be justified in predicting ~ *Ga*. The fact that *Ga* turns out to be true and ~ *Ga* false cannot be given as a reason for judging one of these predictions justified and the other not. We cannot without further restrictions allow that both predictions are justified, for we would then be justified in accepting a contradiction. Although *K* remains consistent under logical implication, it would not remain consistent under unrestricted inductive inference. Arguments like (1) and (2) would inevitably generate what Hempel has elsewhere called "inductive inconsistencies."[11]

We might try to avoid the problem of ambiguity by adopting Carnap's requirement of total evidence on all applications of inductive logic. This requirement demands that we use all the evidence available in determining degrees of confirmation (inductive probability), allowing us to use a part of the total evidence only if the evidence we ignore is irrelevant to the conclusion (i.e., only if the conclusion has the same probability given the relevant part of the evidence as it has given the total evidence). Hempel believes that this suggestion is on the right track. The task is to refine Carnap's requirement so that it addresses the specific problem of ambiguity in statistical explanation. As Hempel notes, we should not interpret Carnap's requirement of total evidence as demanding that we use all the information available to us. For in that case all probabilistic explanations offered at a given time would have the same (very large) explanans. Moreover, when we offer an explanation, we already know that the explanandum event has occurred. Including this fact in the premises would make the argument trivially deductively valid (not inductive) and nonexplanatory (since no law in the premises would be essential to the deduction of the conclusion). So we must limit Carnap's requirement of total evidence to just that evidence that is of potential explanatory relevance to the explanandum-event. In short, we must express the desired condition in such a way that I-S arguments satisfying it will use the right reference class for the purposes of explaining the explanandum-event. Here is Hempel's proposal.

HEMPEL'S REQUIREMENT OF MAXIMAL SPECIFICITY (RMS)

Consider our standard statistical explanation schema:

$$\begin{array}{l} P(G/F) = r \\ \underline{Fb} \\ \overline{\overline{\quad\quad\quad}}\ [r] \\ Gb \end{array}$$

Let *S* be the conjunction of the premises, and let *K* be the total set of statements accepted at the time the explanation is proposed. Hempel's RMS stipulates that if (*S* & *K*) implies that *b* belongs to a class F_1, and that F_1 is a subclass of *F*, then (*S* & *K*) must imply a statement specifying the probability of *G* in subclass F_1, say $P(G/F_1) = r_1$. Here, r_1 must equal *r* unless the probability statement $P(G/F_1) = r_1$ is simply a theorem of mathematical probability theory.

As its name indicates, the RMS insists that we assign the explanandum event, *Gb*, to the most specific reference class (the maximally specific reference class) to which it is known to belong. To understand better how the requirement works, we may consider again the explanation of why the weather in Stanford on November 27 was warm and sunny and the problem of epistemic ambiguity arising from the rival arguments (1) and (2). (Recall that argument (1) begins with the probability that, relative to the class *N* of days in November, day *n* has the property *W* of being warm; argument (2) begins with the probability that, relative to the class *S* of days succeeding cold and rainy days in Stanford, day *n* has the property ~*W* of failing to be warm and sunny.) In this example, we have stipulated that our body of knowledge, *K*, includes the premises of both argument (1) and argument (2). Consider argument (1), offered as an explanation of why that November day was warm and sunny. Is argument (1) an admissible I-S explanation of its conclusion, once we add the RMS? The conjunction of the premises of (1) and *K* implies that *n*, November 27 in Stanford, belongs to the class (*N* & *S*), which is a subclass of *N*. Now, either the conjunction of the premises of (1) and *K* implies the probability of W (a warm and sunny day) in $(N \& S)$, or it does not. If the value of $P(W/N \& S)$ is not given, then the RMS is not satisfied. In that case, neither (1) nor (2) will qualify as an adequate I-S explanation. Alternatively, if the conjunction of the premises of (1) and K does imply that $P(W/N \& S) = r_1$, then we have to consider what the value of r_1 is. On the one hand, if $r_1 = r$, where r is the numerical value of $P(W/N)$, then S is statistically (and explanatorily) irrelevant to W, and argument (1) satisfies the RMS. If there is no other reference class more specific than N to which n is known to belong, then argument (1) stands as an acceptable I-S explanation of its explanandum, Wn. On the other hand, if $r_1 \neq r$, and if the statement that $P(W/N \& S) = r_1$ is not simply a theorem of mathematical probability theory, then the RMS is not satisfied, and argument (1) is disqualified as an acceptable I-S explanation. In this case, some other argument referring n to the more specific class $(N \& S)$ may constitute an acceptable I-S explanation of Wn only if it, in turn, satisfies the RMS. The reason for the "unless" clause in the RMS is thus easy to appreciate. The class $(N \& W)$ is clearly a subset of N, but $P(W/N \& W) = 1$ by the probability calculus alone, and so the probability of W in the subclass $(N \& W)$ must differ from the probability of W in the class N. Obviously, without the "unless" clause, no inductive argument could ever satisfy the RMS.

As we have seen, Hempel explicitly relativizes the RMS to a particular knowledge situation, K. What is K? It is the class of all the sentences that are accepted as true by empirical science at a given time. Thus, K could (and quite likely does) contain some false sentences, and the contents of K will change over time.[12] This feature of Hempel's RMS has profound consequences for his concept of an I-S explanation. It means that, for Hempel, there is no such thing as an objective, "correct" inductive explanation independent of the scientific context. By relativizing the RMS to the beliefs of scientists at a given time, Hempel is admitting that inductive explanations (unlike their D-N counterparts) are fundamentally relative and subjective: they depend on the beliefs of scientists for their very existence. Hempel calls this feature of inductive explanations the *epistemic relativity of statistical explanation*.

6.4 | Ruben on the Irrelevance Objection to Hempel's Models of Explanation

Many philosophers of science have judged Hempel's models of explanation unacceptable in one or another respect and have proposed alternative accounts to meet its difficulties. In the remainder of this commentary, we discuss two such accounts and the objections from which they arise. The first, from David-Hillel Ruben, emerges from an important criticism of Hempel's covering law model offered in various forms (by Peter Achinstein, Wesley Salmon, and others) and is known as the *irrelevance objection*. (The second account, by Peter Railton, is discussed in the final section of this commentary.) After evaluating the proposal that Hempel's account might be repaired by including a description of the explanandum's cause in the explanans—the *causal condition*—Ruben considers reasons for departing from Hempel's insistence that explanations are arguments involving laws.

In the first part of his article "Arguments, Laws, and Explanation," Ruben gives two sets of counterexamples to Hempel's covering law model of explanation. The first set (using Ardon Lyon and Baruch Brody's examples) concerns the explanation of laws; the second set (using Achinstein and Salmon's examples) deals with the explanation of particular facts. We shall focus on the second set.

THE ARSENIC EATER

Achinstein invites us to consider the ill-fated Jones, who eats at least a pound of arsenic and dies within twenty-four hours. Suppose that it is a law of nature that anyone who eats that much arsenic will be dead within a day. From this law and the initial condition that Jones ingests more than a pound of arsenic, we can deduce that Jones dies within twenty-four hours of his eating the arsenic. Thus, we have here an argument that satisfies all the conditions of Hempel's D-N model and that seems to be a good explanation of why Jones died. But then we learn that Jones did not die of arsenic poisoning but was run over by a bus shortly after his poisonous meal. Clearly, the D-N argument citing the lethal properties of arsenic now seems to fail as an explanation. Even though the premises of the D-N argument are true and make essential use of a (true) law to validly entail that Jones dies within twenty-four hours, those premises do not explain why Jones died. Why? Because it was the bus that killed Jones, not the arsenic. The premises of the D-N argument are explanatorily irrelevant to the explanandum. (On pp. 722–24, Ruben considers a number of ways in which the D-N model might, using resources already present in Hempel's theory, be defended against the arsenic-and-bus counterexample and argues that they are unsuccessful.)

THE BIRTH-CONTROL PILLS

It is poor Jones yet again who figures in a counterexample to Hempel's model, this time from Wesley Salmon. John Jones takes birth-control pills regularly and fails to become pregnant. This is hardly surprising. Yet it is, presumably, a law that any man who takes birth-control pills regularly will fail to become pregnant, and John Jones is such a man. So we have a D-N argument with Jones's failure to become pregnant as its conclusion. But clearly this argument does not explain why Jones failed to become pregnant. Jones failed to become pregnant because he is a man, not because he is a man who took birth-control pills regularly. The explanans of the D-N argument does assert that Jones is a man, but it also includes the irrelevant information about the oral contraceptives. As Ruben argues, it is the presence of this additional, irrelevant information that robs the D-N argument of its explanatory power.

THE HEXED SALT

Salmon and Achinstein's examples can also be adapted to provide counterexamples to Hempel's I-S model. Suppose, for example, that ordinary table salt has a high probability (say, 0.95) of dissolving when stirred into cold water for five minutes. I take some salt and place a "dissolving spell" on it. It is now a sample of hexed salt.[13] It is a law that all hexed salt dissolves in water with a probability of 0.95. But although this law can be used to predict that my sample of hexed salt will dissolve in water, it does not explain why it does so. As with Salmon's birth-control pills example, the I-S argument based on the hexed-salt law contains irrelevant information.

Notice that in the hexed-salt example, Hempel's RMS is satisfied. There is no more specific reference class to which the hexed salt could be assigned that would make any difference to its chances of dissolving. Indeed, what has gone wrong in this example is that the salt has already been assigned to a reference class (the class of things that are hexed salt) that is *too* specific. The solution to the problem might seem to lie in a simple modification of Hempel's RMS. Instead of requiring that the explanandum be referred to the most specific class that makes a difference to the probability, we should instead assign the explanandum to the widest, least specific class that satisfies the RMS. Salmon has named this new requirement, *the requirement of the maximal class of maximal specificity.* Unfortunately, this proposal (which Hempel advocated in 1968) will not work.[14]

To see why it fails, suppose, for the sake of argument, that sodium bicarbonate has a probability of 0.95 of dissolving when stirred into cold water for five minutes. This is exactly the same as the probability of salt (sodium chloride) dissolving under the same circumstances. Some white powder (that we know to be salt) is stirred into cold water and after five minutes all of it has dissolved. What explains the fact that the powder dissolved? According to the requirement of the maximal class of maximal specificity, we must seek the widest class that satisfies the RMS. The class of things that are salt satisfies the RMS; so does the class of things that are sodium bicarbonate; and so, too, does the disjunctive class of things that are either salt or sodium bicarbonate. On the assumption that we know of no other chemicals that dissolve in water with a probability of 0.95, it follows that the class of things that are either salt or sodium bicarbonate is the widest class that satisfies the RMS. Thus, according to the requirement of the maximal class of maximal specificity, what explains the dissolving of the powder is not the fact that it is salt but the fact that it is either salt or sodium bicarbonate. On this proposal, to say that the powder is salt is irrelevant—just as irrelevant as saying that it is hexed or that it was mined in Utah. And that just seems wrong. Surely, what explains the dissolving of the powder—at least at some level of explanation—is the fact that it is salt. Thus, while the RMS fails because it permits the explanandum to be referred to classes that are too specific, the new proposal fails because, in some cases, it requires that the explanandum be referred to classes that are too wide.

A PROPOSED CURE FOR THE IRRELEVANCE PROBLEM: THE CAUSAL CONDITION

Reflection on the irrelevance problem (and on the symmetry problem, discussed earlier) leads naturally to the idea that an adequate explanation for a particular fact must include a description of the cause of that fact. As Ruben quotes Salmon as saying, such a proposal would "put the *cause* back into *because*" (728). Thus, we need to consider whether Hempel's model can be repaired by adding an empirical causal condition—a condition requiring that the explanans contain a description of the cause of the explanandum and that this description play an essential role in the derivation of the conclusion of the explanatory argument.

As a remedy for the irrelevance and symmetry problems, the causal condition sounds promising. But as Timothy McCarthy has shown, and as Ruben explains, the causal condition is no guarantee that the resulting argument will be genuinely explanatory. Consider a version of the first of McCarthy's counterexamples, regarding an attempt to explain why a particular forest caught fire. Let us suppose that the actual cause of the forest fire was a lightning strike. Our D-N argument (substituting for the *A*s, *B*s, *C*s, and *D*s in McCarthy's formula) runs as follows:

1 All metals are conductors.
2 The forest was struck by lightning, and this screw is metallic.
3 Either this screw is not a conductor, or the forest was not struck by lightning, or the forest caught fire.
4 The forest caught fire.

Admittedly, this example is highly artificial. But the point is that, even though it satisfies Hempel's requirements and the causal condition, this argument surely fails to explain why the forest caught fire, because it is circular.

The circularity in the forest fire argument can be diagnosed in the following way. Premise (3) is a disjunctive statement containing three unrelated disjuncts. In order for premise (3) to be true, at least one of its disjuncts must be true. From premises (1) and (2) we can deduce that the first two disjuncts are false. So to know that the third premise is true, we must know that the third disjunct is true. But the third disjunct is simply a restatement of the conclusion. Thus, in a fairly obvious sense, the forest fire argument is viciously circular. It was this problem of vicious circularity that Jaegwon Kim attempted to avoid in the way described by Ruben. Kim imposed yet a further condition, namely, that the explanandum (the conclusion) not entail any of the conjuncts in the singular premises when those premises are written in conjunctive normal form. Kim's condition rules out the forest fire argument because the third premise of that argument is already in conjunctive normal form and is entailed by the explanandum. However, as Ruben explains, McCarthy was able to devise yet a further argument that satisfies Hempel's conditions, the causal condition, and Kim's condition but that still fails to explain its conclusion.

6.5 | Ruben's Single-Statement View of Explanation

The purpose of the causal condition, in meeting the irrelevance and symmetry objections, is to tighten the connection between explanans and explanandum so that their deductive relation exists by virtue of some actual causal connection. But notice that the proposed causal condition merely requires that the premises of an explanatory argument mention the event, *c*, which, as a matter of fact, is the cause of the explanandum event, *e*. The causal condition does not require that the premises contain a statement that says "*c* is the cause of *e*." McCarthy's counterexamples show that, in this form, the causal condition is too weak: a mere mention of the cause does not secure the explanatory relevance we seek. But a suitable strengthening of the causal condition is not easily accomplished. Suppose we strengthen the causal condition so that it now does require a statement in the premises that says "*c* is the cause of *e*." The consequences of this adjustment for the theory of explanation are radical. For it now emerges that all explanations of particular events would reduce to a very simple argument that has but a single premise, namely:

$$\frac{c \text{ is the cause of } e.}{e.}$$

As Ruben remarks, this argument is so trivial that the thesis of Hempel (Carnap, Mill, Aristotle, and many others) that explanations are arguments is called into question. For it seems that we could just as well say that the explanation of *e* is the single (true) statement that *c* is the cause of *e*.

Moreover, in such a construal of explanation no law is explicitly mentioned.

It is in this way, then—by considering important objections to Hempel's covering law model and the difficulty of avoiding them with a causal condition—that Ruben is led to a view of explanation that departs significantly from Hempel's model. In particular, this new account of explanation denies two of Hempel's key doctrines: that explanations are arguments and that they must explicitly involve laws.

RUBEN ON THE ROLE OF LAWS IN EXPLANATION

Ruben agrees with Hempel, Carnap, and others that if explanations are arguments, then explanations must include laws. (In a D-N argument explaining a single event, the lawlike generalization is essential if the premises are to entail the conclusion.) But, clearly, it does not follow from this that if explanations are not arguments, then they need not include laws; nor does it follow that laws are irrelevant to explanation. Ruben's own position is that every explanation is a single statement, not an argument. Some explanations (especially those in the physical sciences) do include laws, but the role laws play in such explanations is not that of a premise in an argument. Other explanations can be full and complete without containing laws, although relevant laws do still play an important role in such explanations.

In the final two sections of "Arguments, Laws, and Explanation," Ruben discusses how laws can be relevant to explanation on his single-statement view. On Ruben's view, *o*'s being *F* is the full explanation of why *o* is *G* only if it is a law that all *F*s are *G*, without exception or qualification. If it is false that all *F*s are *G*, then *o*'s being *F* does not fully explain why *o* is *G*. Perhaps the real connection between *F*-ness and *G*-ness is more complicated and the relevant law is: $(x)(Fx \,\&\, Kx \,\&\, Hx \,\&\, Jx \supset Gx)$. The point is not that the explanation of why *o* has property *G* must include the law $(x)(Fx \,\&\, Kx \,\&\, Hx \,\&\, Jx \supset Gx)$. Rather, it is that a full explanation of why *o* is *G* must mention not only that *o* is *F*, but also that *o* is *K*, *H*, and *J* as well. In this way, laws can be relevant to explanations without appearing in them explicitly.

Let us now suppose that it is a law that all *F*s are *G* and that someone claims that *o*'s being *F* is the full explanation of why *o* is *G*. Ruben argues that such a claim seems correct not because of the law (regarded as the generalization that all things that are *F* are also *G*), but because the law calls to our attention the properties that *o* possesses. From this point of view, it is strictly irrelevant that other *F*-things are also *G*-things; what matters, and the only thing that matters, for explaining why *o* is *G* are the properties of *F*-ness and *G*-ness that this particular thing, *o*, happens to have. The explanatory work is done by the properties *o* possesses, not by the law stating that all *F*s are *G*.

As Ruben remarks, it is hard to see why it should be thought relevant in explaining why this thing has property *G* to be told, via a law statement, about *other* things that also have properties *F* and *G*. No doubt some connection between *F*-ness and *G*-ness must hold, but this is precisely what the explanation claim under consideration already expresses. If *o*'s being *F* is the full explanation of why *o* is *G*, then, because of the implicit generality of the properties *F*-ness and *G*-ness, it follows logically that it is a law that all *F*s are *G*. But the law itself is not part of the explanation.

In the final section of his paper, "Generalizations Get Their Revenge," Ruben argues that laws (and theories) are often indirectly relevant to explanation, especially the deep kinds of explanation one finds in physics, by providing scientists with the vocabulary in which to phrase their explanations. What distinguishes a deep explanation from a shallow one is the explanatory power of the predicates that the explanation statement employs.[15] Almost inevitably in such cases, the explanation statement will explicitly include laws that connect the deep, explanatory properties with the more superficial properties used to describe the explanandum. Thus, on the surface at least, a full explanation in the sciences on Ruben's account may end up looking very similar to an ideal Hempelian D-N explanation, but the philosophical interpretation of what this and other explanations really are is fundamentally different.

6.6 | Railton on What Is Wrong with Hempel's Models of Explanation

In his article "A Deductive-Nomological Model of Probabilistic Explanation," Peter Railton begins by discussing what he takes to be the fundamental flaws in Hempel's two models of explanation. After diagnosing these flaws, Railton presents his own account, the D-N-P model of explanation.

NOMIC SUBSUMPTION

Throughout his writings on explanation, Hempel's guiding assumption is that to explain some event or phenomenon is essentially a matter of subsuming the explanandum under a law (whether universal or statistical). For Hempel, explanation just is nomic subsumption. But as we have seen from the earlier discussion of the thesis of structural identity and the symmetry objection criticizing that thesis, not every case of nomic subsumption is an explanation. We can, for example, use the laws of optics and geometry to predict the height of the flagpole from the length of its shadow and the elevation of the sun, but this prediction does not explain why the flagpole has the height it does. Nomic subsumption is not sufficient for explanation.

According to Railton, what is lacking in D-N arguments that fail to explain is an account of the underlying mechanisms responsible for causing the fact to be explained. Without providing some such account, many D-N arguments are too superficial to explain their conclusions. To explain why this *A* is also a *B*, we need to do more than cite the law that all *A*s are *B*s. For even if the law in question is causal, the explanation would be incomplete without an account of the mechanism (or mechanisms) at work. This condition, that an explanation provide a mechanism, plays a central role in Railton's effort to give an account of probabilistic explanation. Railton insists that explanations require mechanisms even when the phenomena in question are not deterministic but irreducibly probabilistic.

INDUCTIVE ARGUMENTS

Railton takes issue with two prominent features of Hempel's I-S model of probabilistic explanation: the requirements of high probability and maximal specificity. Railton thinks that both of these objectionable features stem from Hempel's insistence that the statistical explanation of particular facts be a type of *inductive* argument. Railton's proposal, in light of this diagnosis, is simple and radical: probabilistic explanations are not inductive arguments. Indeed, on Railton's D-N-P model, probabilistic explanations are not arguments at all; they include arguments, but none of the arguments they include is inductive, and the explanations are not themselves arguments.

To appreciate Railton's diagnosis of the source of those two features he finds worrisome in Hempel's I-S model, consider, first, the high-probability requirement. An inductive argument's strength depends on the degree of probability that its premises confer on its conclusion—the higher the probability, the stronger the argument. If statistical explanations are inductive arguments, then the high-probability requirement immediately follows: a statistical explanation will be strong only if it establishes that the explanandum occurs with suitably high probability. Now consider the RMS. Inductive arguments are notoriously sensitive to the addition of information to their premises. (In this respect, inductive arguments differ from deductively valid arguments. If an argument is deductively valid, then it remains valid, no matter what is added to its premises.) If Smith is a twenty-year-old woman, and 90 percent of such women survive to celebrate their fortieth birthday, then, relative to those premises, the probability of the conclusion that Smith will live at least another twenty years is quite high. But once we add the information that Smith has AIDS, the probability of her long-term survival plummets. Thus, we generate the problem of ambiguity that Hempel's RMS is intended to solve. Again, the root of the problem is the inductive nature of the arguments involved.

THE HIGH-PROBABILITY REQUIREMENT

Assuming that Railton has correctly traced the problems with Hempel's high-probability and maximal-specificity requirements to their inductive source, are these features as damaging to Hempel's account as Railton claims? The high-probability requirement has often been criticized (by Salmon, Jeffrey, and others) on the grounds that it rules out the possibility of explaining improbable events. Railton gives the example of a genuinely random wheel of fortune with 99 red stops and 1 black stop. The stipulation that the wheel is genuinely random means that no factor, even in principle, can affect the outcome once the wheel has been set spinning. Each stop thus has exactly the same probability of being chosen when the wheel comes to rest. In a setup like this, it seems absurd to insist that we can explain why the wheel halts at a red stop but not why the wheel halts at the black one. Surely, the explanation is equally good in either case, regardless of the outcome—there is an irreducibly indeterministic mechanism that generates red with probability 99/100 and black with probability 1/100. Hempel's high probability requirement seems to conflate *inductive strength* with *explanatory value*. It is the second that is of concern in seeking an adequate account of explanation. The predictive inference to the conclusion that the wheel will stop at black is much weaker, inductively, than the inference to the conclusion that the wheel will stop at red. But the explanation of why the wheel stops at black is just as good as the explanation of why the wheel stops at red, since, in either case, the explanatory statements provide us with as much of an understanding of the underlying mechanism as it is possible to have. Explanations should be judged by the completeness of the explanatory information they provide, not by the strength of the inferences they permit. To think otherwise is, in Railton's words, to confuse explanation with induction.

THE MAXIMAL SPECIFICITY REQUIREMENT

Railton rejects Hempel's requirement of maximal specificity because it relativizes the concept of probabilistic explanation. If Hempel were right, then there could be no correct or true probabilistic explanation of anything, for all such explanations would be relative to the state of scientific knowledge (and ignorance) at the time they were proposed. For Railton this is unacceptable because it would deprive probabilistic explanation of its objectivity.[16] It is important to note here that Railton is not denying that which explanations we believe to be correct depends on our beliefs. In just the same way, which propositions we believe to be true also depends on our beliefs. But, Railton insists, what it is for something to be a correct explanation cannot depend on our beliefs and neither can what it is for a proposition to be true. Indeed, it is only if being a correct explanation and being true are belief-independent that we can make sense of our beliefs that particular propositions are true or that particular explanations are correct.

Part of the reason for Hempel's error, Railton thinks, is that Hempel has failed to take seriously his own distinction between statistical descriptions (that just happen to be true) and genuine probabilistic laws. Suppose, on the one hand, that determinism were true. In this case, all laws would be universal; there would be no probabilistic laws and thus no correct or true probabilistic explanations. In such a purely deterministic world, statistical explanations would be merely a stopgap measure until we could discover the true, objective D-N explanations of the things that happen in that world. The statistical "laws" mentioned in such explanations would not be real laws at all but merely expressions of our ignorance at a given time. While it may seem as if, in a completely deterministic world, the conditions for Hempel's I-S model of explanation would be satisfied (capturing our best probabilistic explanations in varying states of ignorance), in fact they would not be satisfied, simply because in such a world there could be no genuine probabilistic laws at all.

On the other hand, suppose that the world is not deterministic but is governed by at least some true probabilistic laws. Once we realize that true probabilistic laws require genuine indeterminism, the epistemic relativity thesis collapses and with it goes the motivation for Hempel's RMS. Remember that the RMS says that, for the purposes of explaining why o has property G, we should assign o to the most specific reference class of which we have knowledge that would make a difference to the probability that o is G. (It is the reference in RMS to our state of knowledge, that is, to our beliefs at a given time, that makes probabilistic explanation epistemically relative on Hempel's account.) But if it is a genuine law that the probability that o is G, given that o is F, is 0.95, then there cannot be any more specific reference class than F to which o could be assigned that would make a lawlike difference to the probability that o is G. If it is a genuine probabilistic law that $P(G/F) = 0.95$, then, if o is F, the real, objective probability that o is G is 0.95, regardless of what anyone thinks or believes. Genuine probabilistic laws require indeterminism, and such indeterminism guarantees the objectivity of statistical explanation.

6.7 | Railton's Deductive-Nomological Model of Probabilistic Explanation

Railton's main concern is the explanation of what he calls *lawful* chance phenomena. Chance phenomena (such as radioactive decay) are lawful when they obey a statistical or probabilistic law. Like Railton, we shall focus on the explanation of particular events that are brought about by chance mechanisms in a lawful way.

Several key elements of Railton's D-N-P model of explanation have emerged already from the preceding discussion of Railton's critique of Hempel. Those elements can be summarized as follows:

- all explanations are objective; none of them is relative to a set of beliefs or to the state of scientific knowledge at a particular time;
- explanations are not arguments, nor should they be evaluated as if they were arguments; explanations are accounts that provide relevant information;
- explanations (whether probabilistic or not) require not only laws but also an account of the underlying mechanism(s);
- probabilistic explanations require genuine probabilistic laws; genuine probabilistic laws require indeterminism;
- there is no high-probability requirement for probabilistic explanation; improbable events can be explained just as well as highly probable events.

RAILTON'S CONDITIONS OF ADEQUACY FOR D-N-P EXPLANATION

Like Hempel's D-N model, Railton's D-N-P model can be set out in schematic form. (We have made minor changes to Railton's notation.)

Explanandum Ge,t_0 : *e*'s having property *G* at time t_0.

Explanans

a	A theoretical derivation of a probabilistic law of the form (*b*).		Theoretical derivation
b	$(t)(x)\ [Fx,t \rightarrow P(Gx,t) = r]$	Probabilistic law	Deductive argument
c	Fe,t_0	Initial condition	
d	$P(Ge,t_0) = r$	Statement of a single-case propensity	
e	(Ge,t_0)		Parenthetic addendum

The explanandum, the event to be explained, is *e*'s having property *G* at time t_0, which Railton writes as Ge,t_0. For example, the explanandum might be that a particular wheel of fortune, *e*, stopped on black at time t_0. The statement that this event occurred appears again at the end of the explanans, but only as a parenthetic addendum. Putting Ge,t_0 in parentheses in line (*e*) is Railton's way of indicating that the explanandum is not the conclusion of the explanans, nor is it inferred from the explanans. Rather, the parenthetic addendum is put there simply to remind us that the explanandum event did, in fact, occur. Remember, although the explanans contains arguments—the deduction of (*d*) from (*b*) and (*c*), plus whatever arguments are involved in the theoretical derivation of the probabilistic law in line (*a*)—the explanans as a whole is not itself an argument.

The probabilistic law written schematically in line (*b*) should be read as follows: for all times *t*, and all things *x*, if *x* has property *F* at time *t*, then *x* has probability *r* of having property *G* at that time. For example, if *F* is the property of being a genuinely random wheel of fortune with 99 red stops and 1 black stop, and *G* is the property of stopping on black after the wheel has been set spinning, then the value of *r* is 1/100. (In this example, the probability does not depend on time and so the variable *t* can be dropped from the statement of the law.)

Line (*d*) is derived from lines (*b*) and (*c*) in two steps: by universal instantiation followed by *modus ponens*. Again, it helps to consider our simple example of the wheel of fortune. The probabilistic law for the wheel of fortune is a universal generalization: it says (ignoring the time variable) that "for any object whatever, if that object is a genuinely random wheel with 99 red stops and 1 black stop, then the probability that the wheel will stop on black is 1/100." Universal instantiation allows us deduce that this generalization holds for a particular object, such as *e*. So we get the conditional statement: "if *e* is a genuinely random wheel with 99 red stops and 1 black stop, then the probability that *e* will stop on black is 1/100." In our wheel of fortune example, line (*c*) tells us that the antecedent of this conditional statement is true: *e* is indeed a genuinely random wheel with 99 red stops and 1 black stop. Thus, by *modus ponens* we can deduce further that the probability that *e* will stop on black is 1/100. As Railton remarks about an exactly similar deduction (argument (2) on page 754), it is vital for the first step of this derivation that (*b*) be a genuine universal law, for only then can conclusions be deduced from it by universal instantiation. If (*b*) were merely a statistical generalization saying, in our example, that in a very large sample of *N* wheels, on average, *N*/100 of them stop on black, nothing could be validly deduced about the probability that this particular wheel *e* will stop on black.

Line (*a*) of the explanans expresses Railton's requirement that any adequate explanation must specify an underlying mechanism that causally brings about the event to be explained. While the derivation of the probabilistic law of the form (*b*) is indeed meant to issue from scientific theory, Railton has in mind a quite liberal notion of what counts as a mechanism. For example, in the quantum-mechanical derivation of the law of radioactive decay, which Railton gives as his central illustration, the alpha particle escapes from the nucleus by what is called *tunneling* or *barrier penetration*. But tunneling is not a visualizable mechanism or any kind of process that is continuous in space and time. Like other distinctively quantum phenomena, it involves the discontinuous changes in energy and momentum of a system obeying the fundamental laws of quantum mechanics. The alpha particle, for example, only has a distinct existence as a particle, with a well-defined trajectory and momentum, after it has emerged from the nucleus. To refer to tunneling as a mechanism requires using the word *mechanism* in a very broad sense to mean, roughly, *a law-governed process (not necessarily spatially continuous or extended in time) that causally brings about the event we are interested in*. Newton's force of gravity, acting instantaneously at large distances through a vacuum, would qualify as a mechanism. The important thing, for Railton, is the theoretical derivation, not the intuitive idea of a mechanism as an assembly of pulleys, wheels, and strings.

PROPENSITIES AND PROBABILITIES

The deductively valid argument appearing explicitly in a D-N-P explanation is the inference from (*b*) and (*c*) to (*d*). Thus, just as in Hempel's D-N model, this argument contains a statement of law and a statement of initial conditions in its premises. The crucial difference between the two models lies in the nature of the law and the resulting difference in the conclusion. In a D-N-P explanation, the law is probabilistic and expresses the propensity, $P(Gx,t)$, for an object or system, x, to have the property G at time t. What is a propensity? It is a property of the single system x, a lawlike tendency or physical probability for that system to behave in a certain way. The conclusion that $P(Ge,t_0) = r$, which is deduced from (*b*) and (*c*), is a single-case probability; it is the probability that this particular e is G at the time in question. This notion of a propensity as a single-case probability is crucial to Railton's model. As Railton says in his paper, "the D-N-P model is viable only if sense can be made of propensities, or of objective, physical, lawful, single-case probabilities by any other name" (761).

We can try to grasp what propensities (or single-case probabilities) are supposed to be by considering the simple example of a perfectly symmetrical coin. Because of the perfectly symmetrical distribution of its mass and the laws of mechanics, the coin has a physical probability of exactly ½ of yielding a head on any toss. Even if the coin is never tossed, it has that propensity. If the coin is tossed and lands tails, its propensity to yield heads on the next toss remains ½. Propensities are not the same as frequencies; rather, they are properties that explain why we observe certain kinds of outcomes with the frequency that we do. Thus, on Railton's model, if (as we have supposed) the coin has a lawlike, indeterministic tendency of ½ to yield a head on any toss, and if we toss the coin eight times and get, much to our surprise, eight heads in a row, then the full, complete explanation of that outcome is that the coin had a propensity of $(\tfrac{1}{2})^8$ to behave in that way when tossed. The full explanation of the outcome has been given when the true chance of its occurrence, however low, has been given.

Like probabilities, propensities range in numerical value from 0 to 1. But propensities are not the same as probabilities. The difference between probabilities and propensities can be illustrated in the following example. Suppose that we have a 50-50 mixture of two radioactive elements, A and B, each with a different, known propensity to emit alpha rays. We know how likely it is that the A atoms will emit an alpha ray in a given time interval, and we know how likely it is that the *B* atoms will emit an alpha ray in the same time interval. Suppose that an alpha ray has been emitted from the mixture. From the information provided we can, using Bayes's theorem, calculate the probability that the alpha ray came from the A atoms, and we can also calculate the probability that the alpha ray came from the *B* atoms. But neither of these inverse probabilities can be a propensity, for an alpha ray existing at a given time cannot have a propensity to have been produced from a particular source at an earlier time.[17] Propensities are indeterministic causal tendencies. Like causes in general, propensities have a forward temporal direction, from the past and present into the future. Unlike probabilities, propensities can never run from the present to the past. Obviously much more work is needed before we can have a clear notion of what propensities are. Railton's own suggestions are the barest beginnings of this project.

RAILTON'S RESPONSE TO SOME OBJECTIONS

Toward the end of his article, Railton raises several objections to his D-N-P model of explanation and responds to them. One of these objections (the first) is particularly instructive. The criticism is that Railton's model of probabilistic explanation is at once too narrow and too broad. On the one hand, the D-N-P model may be judged too narrow because very few phenomena outside of quantum mechanics are genuinely indeterministic. Sciences such as evolutionary biology, genetics, epidemiology, economics, sociology, fluid mechanics, and meteorology all deal with phenomena that are deterministic but very complicated. Statistics and probability are used by these sciences, but largely because we are ignorant of the many thousands (often, millions) of initial conditions actually determining the behavior in which we are interested. Thus, on Railton's model, none of these uses of probability and statistics would be explanatory; the calculations, predictions, and arguments involved are not D-N-P explanations. On the other hand, Railton's model is accused of being too broad because, if any genuinely indeterministic propensities whatsoever are at work, however minuscule, then a proper D-N-P explanation must take account of them. For example, when an ice cube is placed in a glass of warm water, we expect it to melt. Normally, we would think that we have explained this melting by showing that it follows from the laws of classical thermodynamics (such as the second law stating that, in isolated systems, entropy always increases). But ice and water are composed of molecules, and the molecules of atoms; since atoms are subject to quantum-mechanical laws, there is a small probability that those molecules could move in such a way that the ice does not melt but instead becomes even colder while the water becomes hotter. The probability that this might happen is very, very small, but it is not 0. Thus, the propensity of the ice to melt when placed in water is not exactly 1. (So the second law of thermodynamics is not really a law at all because it is, strictly speaking, false that entropy must always increase.) Consequently, a proper explanation of the melting has to be a D-N-P explanation in which the exact numerical value of the propensity is calculated. (This, of course, is a very difficult task and not one we are normally able to perform.) Moreover, as Railton points out, just about everything that happens in the world, even at the macroscopic level is, to some very small degree, contingent on what happens at the atomic level. So even an explanation of human behavior or the motions of the planets would have to include a fiendishly complicated quantum-mechanical calculation of the relevant propensity before it could qualify as a proper, probabilistic explanation.

Railton's response to both arms of the objection is to concede the point it makes, but to deny that this reflects badly on his model. Thus, Railton willingly embraces the conclusion that if a system is genuinely deterministic, then however complicated it might be, no real explanation of its behavior can be probabilistic. Whatever else we are doing when we appeal to probabilities or statistics in contexts like this—whether it is predicting, approximating, or estimating—it is not explaining, and we should not pretend that it is. Likewise, if the system is indeterministic, to whatever small degree, then we cannot shirk our explanatory responsibility by ignoring that fact. In the final analysis, the aim of explanation is to achieve a true understanding of the ways things really are. Being conscientious about that aim makes the task of explanation—genuine explanation—extremely difficult. The fault, if fault there be, lies in the complexity of the world, not in the demands of the D-N-P model.

6.8 | Summary

In the second half of the twentieth century, most of the philosophical debate concerning the nature of scientific explanation has centered around Hempel's covering law thesis and his two models of explanation, the deductive-nomological (D-N) and the inductive-statistical (I-S). According to the covering law thesis, explanations are arguments (either deductively valid or inductively strong) that have among their premises at least one statement of an empirical law. As Wesley Salmon has pointed out, Hempel's covering law thesis is characteristic of an epistemic conception of scientific explanation, since it takes explanation to be essentially an inference showing that the explanandum event was to be expected. Given the information in the premises of the explanatory argument, the explanandum event could have been predicted, either with certainty (D-N) or with high probability (I-S). This capability of being used as a prediction is seen by many philosophers of science as the hallmark of a good explanation. For Hempel, Carnap, Nagel, and others, being able, at least in principle, to predict the event to be explained guarantees that the explanation has testable, empirical content and gets to the heart of what distinguishes genuine explanation from mere pseudoexplanation.

Hempel's commitment to the epistemic conception of explanation is made explicit in his advocacy of the thesis of structural identity, that all adequate explanations are potentially predictions and that all adequate predictions are potentially explanations. In other words, Hempel insists that there is no formal, objective difference between explanation and prediction. Whether an argument is an explanation or a prediction depends on pragmatic factors, such as when the argument is put forward and the intentions of the scientist who presents the argument. Predictive arguments are advanced prior to the events mentioned in their conclusions either to test the theory from which the laws are taken or, if the theory and its laws are well accepted, to provide a reliable basis upon which to plan for the future. Explanations are advanced after the events mentioned in their conclusions in order to achieve a theoretical understanding of why those events occurred. We make predictions in order to anticipate the future; we give explanations in order to understand the past. But, according to Hempel, in either case, the formal structure of our reasoning is the same and is captured by the D-N and I-S models.

Attacks on the thesis of structural identity (by Michael Scriven and others) go under the general heading of the symmetry objection. They consist of two kinds of alleged counterexamples: explanations that are not predictions and predictions that are not explanations. Hempel has strenuously opposed the first kind of alleged counterexample, often arguing that the presumed explanation is no such thing. But Hempel concedes that the second subthesis, that all adequate predictions are potentially explanations, is not as secure as its converse.

The irrelevance objection is another important class of criticisms of Hempel's two models of explanation. Various philosophers (including Wesley Salmon and Peter Achinstein) have given arguments that, while they satisfy all of Hempel's conditions for an adequate explanation, do not appear to explain their conclusions. The birth-control pills and hexed-salt examples offer typical cases in which the presence of irrelevant information in the premises robs the arguments of their explanatory power. Because of his commitment to the epistemic conception of explanation, Hempel has denied that these arguments lack explanatory power, but this response seems implausible. What seems to go wrong in these examples is that the lawlike premise in these arguments, while true, fails to identify the cause of the explanandum event. Consequently, Baruch Brody has proposed that we amend Hempel's D-N model by stipulating that the premises of an explanatory argument must contain a description of the event that is the cause of the explanandum. But, as David Hillel-Ruben explains, Brody's proposal has been roundly refuted by counterexamples devised by Timothy McCarthy. As Ruben points out, it is not enough that the premises contain a description of the event that is, as a matter of fact, the cause of the explanandum event; rather, the premises must explicitly identify that event as the cause. But in that case, the underlying structure of explanations is devastatingly simple—it consists simply of the inference from "*c* is the cause of *e*" to the conclusion "*e*"—and explanations need make no explicit mention of any laws. Thus, we are led to the view that Ruben believes is correct, namely, that explanations are not arguments but single statements and that laws, while vitally important for many types of explanation, are not an essential part of the sentence that explains.

Another important class of criticisms of Hempel concern his I-S model of probabilistic explanation. In order to avoid the problem of ambiguity for statistical explanation, Hempel introduced the requirement of maximal specificity (RMS). But the RMS is relativized to what Hempel calls "a given knowledge situation" (715), thus making statistical explanations depend for their very existence on the body of beliefs that scientists accept at a particular time. This epistemic relativity of statistical explanation in the I-S model stands in marked contrast to the objective, nonrelative character of explanations in the D-N model. Moreover, because Hempel construes all explanations as formally equivalent to predictions, I-S arguments are taken to explain their conclusions only when those conclusions follow with high inductive probability from their premises. Since the strength of the inductive relation between premises and conclusion depends directly on the value of the probability that appears in the statistical lawlike premise of such arguments, it follows that, on Hempel's model, it is impossible to explain improbable events. Both of these features—the epistemic relativity of I-S arguments and the high-probability requirement for explanation—have struck critics (such as Salmon and Railton) as serious flaws in Hempel's account.

Peter Railton has proposed his deductive-nomological model of probabilistic explanation (the D-N-P model) as an alternative to Hempel's account. Unlike Hempel's I-S model, Railton's D-N-P model regards probabilistic explanations as fully objective (not relative to any set of scientific beliefs) and permits the explanation of improbable events. Like Ruben, Railton denies that explanations are arguments, although unlike Ruben, Railton thinks that all explanations must contain a deductive argument based on a law. Central to Railton's D-N-P model is the requirement that explanations specify the causal mechanism that brings about the event (or the kind of event) referred to in the explanandum. Genuine probabilistic explanations can be given only when the mechanism at work is indeterministic, and when an indeterministic mechanism is involved, any genuine explanation must be probabilistic. Railton interprets probabilistic laws, not as generalizations about the frequency of certain kinds of event, but as statements of single-case propensities, such as the chance of getting heads when a particular coin is tossed. The propensity of an object or system is its causal tendency to behave in a particular way. Like probabilities, the strength of a propensity can vary on a scale from 0 to 1, but, unlike the empirical frequency interpretation of probability, propensities are just as much physical properties of individual things and systems as their mass and electric charge; in fact Railton sometimes refers to propensities as physical probabilities.

Apart from the difficulty of understanding exactly what propensities are and how they are related to frequencies, Railton's D-N-P model has a number of consequences that may strike one as counterintuitive. Many of these counterintuitive features stem from Railton's demand that explanation properly so-called must be based on the deepest theoretical understanding of nature that we have. Thus, for example, if a process (such as the melting of an ice cube in warm water) involves the slightest chance that the ice cube not melt but become colder, then Railton requires that any explanation of the melting involve a theoretical calculation of the exact propensity (not quite equal to 1) of the cube to melt. This sets a very high standard for probabilistic explanation. Also, as previously noted, on the D-N-P model probabilistic explanations are legitimate only when there is genuine, rock-bottom, physical indeterminacy involved. Thus, any attempt to explain the behavior of complex but ultimately deterministic systems by means of statistical generalizations is ruled out (thereby disqualifying many of the arguments currently accepted as explanatory in the physical, biological, and social sciences).

Because of their emphasis on causation and causal mechanisms (whether deterministic or indeterministic), the models of explanation advocated by Ruben and Railton are good examples of what Salmon has called the *ontic* conception of explanation.[18] Unlike the epistemic approach to explanation, the ontic approach does not regard explanations as arguments (even though they may include or involve arguments). What matters on the ontic account is not whether the explanandum can be predicted with high probability, but whether, regardless of the probability of the explanandum, we can give a correct description of the underlying causal mechanism that brought about the event we wish to explain. The ontic conception of explanation allows us to give complete explanations of events even when those events are highly improbable, as would be the case if nature is governed by indeterministic laws, as quantum mechanics supposes. Seen from the point of view of the ontic conception, the demand for high probability imposed by the epistemic conception ultimately stems from that conception's commitment to determinism as the final truth about the structure of the world. Seen from the point of view of the epistemic conception, Railton's insistence that we calculate objective, true propensities in all cases places an unreasonably high demand on what counts as an explanation. The epistemic approach (at least in Hempel's version of it) permits genuine statistical explanations even if the world is deterministic through and through, but it makes their status as explanations relative to human beliefs at a particular time. The ontic approach (in Railton's version) takes probabilistic explanations to be just as fully objective and nonrelative as those based on deterministic laws, but insists that such explanations can be given only when the mechanisms involved are fundamentally indeterministic.

■ | Notes

1. Carl G. Hempel and Paul Oppenheim, "Studies in the Logic of Explanation," *Philosophy of Science* 15 (1948): 567–79. Reprinted, with a postscript, in *Aspects of Scientific Explanation* (New York: Free Press, 1965), 245–95.

2. Carl G. Hempel, "Deductive-Nomological vs. Statistical Explanation," in *Minnesota Studies in the Philosophy of Science*, vol. 3, ed. H. Feigl and G. Maxwell (Minneapolis: University of Minnesota Press, 1962), 98–169.

3. "A physical theory is not an explanation. It is a system of mathematical propositions, deduced from a small number of principles, which aim to represent as simply, as completely, and as exactly as possible a set of experimental laws" (Pierre Duhem, *The Aim and Structure of Physical Theory*, trans. Philip P. Wiener [1914; Princeton, N.J.: Princeton University Press, 1954], 19).

4. *Aim and Structure*, p. 7.

5. Before judging too harshly Mach and Duhem's skepticism about atoms and the aether, we should remember that the atoms we now believe in are utterly different from those described in the classical "billiard-ball" models of the nineteenth century and that the aether has been discarded entirely. What remains of the kinetic theory of gases and the wave theory of light of the nineteenth century are testable equations and laws, and these alone, according to the positivists, have permanent scientific value.

6. Strictly speaking, then, (*R*2) should be phrased in terms of lawlike sentences, rather than laws. It is commonly assumed that we can judge whether a sentence is lawlike simply by examining its logical form and the predicates it uses. But to judge that a lawlike sentence is true—and hence expresses a law—requires empirical information.

7. Since the explanans can include laws of mathematics, some of the sentences it contains might not be empirical. But (*R*3) guarantees that every explanans must contain at least one empirical sentence.

8. Michael Scriven, "Explanation and Prediction in Evolutionary Theory," *Science* 130 (1959): 477–82

9. See "Popper and the Theory of Evolution" in the commentary on chapter 1.

10. There is no problem of ambiguity for D-N explanations. If a set of true premises deductively entails that *a* is *G*, then no other set of true premises can deductively entail that *a* is not *G*.

11. Carl G. Hempel, "Inductive Inconsistencies," *Synthese* 12 (1960): 439–69. Reprinted in *Aspects of Scientific Explanation*, 53–79.

12. Hempel is using the term *knowledge* in a common, nontechnical sense to mean, roughly, *justified belief*. It is in this sense that we talk of the current state of scientific knowledge while recognizing that some of the beliefs in that body of knowledge are quite likely to be false. When epistemologists use the term in its technical sense, they insist that truth is a necessary condition for knowledge.

13. The example and the phrase *hexed salt* were first proposed by Henry Kyburg. See Henry E. Kyburg, "Comments," *Philosophy of Science* 32 (1965): 147–51.

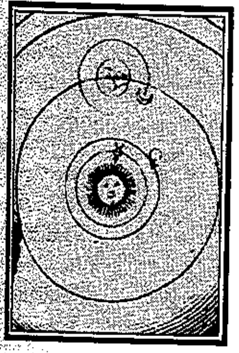
14. Carl G. Hempel, "Maximal Specificity and Lawlikeness in Probabilistic Explanation," *Philosophy of Science* 35 (1968): 116–33. The following criticism of the proposal is adapted from John Meixner, "Homogeneity and Explanatory Depth," *Philosophy of Science* 46 (1979): 366–81.

15. Ruben does not use the phrase "explanatory power." Instead, he talks about laws and theories providing a deeper vocabulary, a vocabulary that "gives a more profound insight into the phenomenon at hand" (741). One way of understanding this notion would be to associate the deeper vocabulary with those theories that unify many different phenomena within a single framework. But since Ruben insists that unification and explanation are two different things, he would reject this suggestion.

16. This line of criticism is also advanced in J. Alberto Coffa, "Hempel's Ambiguity," *Synthese* 28 (1974): 141–63.

17. This criticism originated with Paul Humphreys. See his "Why Propensities Cannot Be Probabilities," *Philosophical Review* 94 (1985): 557–70.

18. Wesley C. Salmon, *Scientific Explanation and the Causal Structure of the World* (Princeton, N.J.: Princeton University Press, 1984), chs. 1 and 4.



7

Laws of Nature

INTRODUCTION

Laws play a central role in scientific reasoning. As we saw in chapters 1 and 4, some philosophers of science think that using laws to explain things is an essential part of what it means to be genuinely scientific, and support for the view that scientific explanation must involve laws is widespread (though not unanimous). Many also believe that we are justified in trusting scientific inferences because these predictions rest, in part, on well-confirmed laws. Our expectations about the behavior of systems, instruments, and materials are reasonable to the extent that they are based on a correct understanding of the laws that govern them. Undoubtedly, much scientific activity is devoted to discovering laws, and one of the most cherished forms of scientific immortality is to join the ranks of Boyle, Newton, and Maxwell by having a law (equation or functional relation) linked with one's name. But despite the crucial importance of laws in science, it is difficult to find a general account of what sort of things laws are that can do justice to everything we take to be true of them.

In this chapter, two important and influential ways of understanding laws—the regularity approach and the necessitarian approach—will be discussed and criticized.[1] In terms roughly hewn, the regularity approach says that laws describe the way things actually behave, that they are nothing more than a special kind of descriptive summary of what has happened and what will happen. The necessitarian approach insists that laws are more than just summaries, that they tell us not merely how things actually behave, but, more importantly, how they must behave. For the necessitarians, both the universality and the necessity of laws are objective, real features of the world (although necessitarians disagree among themselves about the nature of that necessity).[2]

Modern adherents of the regularity approach trace their origins back