Scientific Explanation and the Causal Structure of the World

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1 Scientific Explanation: Three General Conceptions

MODERN SCIENCE provides us with extensive knowledge about the world in which we live. We know that our universe originated in a 'big bang' several thousand million years ago, and that it has been expanding ever since. We know that there was a fairly severe drought in northeastern Arizona during the last few years of the fourteenth century A.D., and that large settlements were abandoned at that time. We know that Halley's comet moves in a roughly elliptical orbit, and that it will return to perihelion in 1986. We know that—barring nuclear holocaust—the human population of the earth will continue to increase for some time to come. We know that the planet Uranus has rings, that *E. coli* live in the normal human intestinal tract, that copper is an electrical conductor, and that the surface temperature of Venus is high. As these examples show, science provides knowledge of what has happened in the past, what will happen in the future, and what is happening now in regions that we are not observing at the present moment. It encompasses knowledge of both particular facts and general regularities. In none of these instances, of course, is our scientific knowledge to be regarded as certain or incorrigible; nevertheless, the physical, biological, and social sciences furnish impressive bodies of knowledge about what goes on in the world, and we have every reason to believe that these sciences will continue to grow at a prodigious rate.

Such knowledge—valuable as it may be for intellectual or practical purposes—is not fully satisfying. Not only do we desire to know *what* happens; we also want to understand *why*. Moreover, it is widely acknowledged today that science *can* provide explanations of natural phenomena; indeed, to many philosophers and scientists, this is the primary goal of scientific activity. Scientific explanations can be given for such particular occurrences as the appearance of Halley's comet in 1759 or the crash of a DC-10 jet airliner in Chicago in 1979, as well as such general features of the world as the nearly elliptical orbits of planets or the electrical conductivity of copper. The chief aim of this book is to try to discover just what scientific understanding of this sort consists in.

Before undertaking the task at hand, it may be useful to make a remark about the basic strategy. During the last thirty-six years (since 1948), a number of quasi-formal 'models' of scientific explanation have appeared in the philosophical literature, and a good deal of attention has been devoted to the investigation of their formal properties. Quite a few are such familiar friends that philosophers often refer to them merely by their initials. We shall meet many of them in the ensuing chapters: deductive-nomological (D-N), deductive-statistical (D-S), inductive-statistical (I-S), statistical-relevance (S-R), deductive-nomological-probabilistic (D-N-P), expected information (E-I), as well as several others for which such initialized designations have not been generally adopted.

Although I shall discuss in some detail various technical aspects of the models, my initial concern will be with what Carnap (1950, sec. 2) called "clarification of the explicandum." Many philosophical studies, including the one to which this book is devoted, aim at providing reasonably precise explications of fundamental concepts, but unless we take preliminary steps to give some understanding of the concept we are trying to explicate—the explicandum—any attempt to formulate an exact explication is apt to be wide of the mark. I am firmly convinced that such terms as "explanation," "scientific explanation," "scientific understanding," and a host of closely related terms are far from being well understood even in a preliminary and imprecise way. Consequently, in an effort to improve that situation, I shall devote the first chapter to a survey of three general conceptions of scientific explanation that have venerable histories and contemporary importance. In the fourth chapter, these three general conceptions will be reconsidered in detail in the light of certain pervasive features of explanations that make some essential appeal to statistical laws. As a result of these efforts at preliminary clarification of the explicandum, we shall find important guidelines for the evaluation of the various 'models' that have been proposed in recent years.

EXPLANATION VERSUS DESCRIPTION

The idea that there are two kinds of scientific knowledge—knowledge of *what* and knowledge of *why*—is not new. In the *Posterior Analytics* (71b18–25), Aristotle distinguishes syllogisms that provide scientific understanding from those that do not. In *The Art of Thinking* ("Port-Royal Logic," first published in 1662), Antoine Arnauld distinguishes demonstrations that merely *convince* the mind from those that also *enlighten* it. He continues, "Enlightenment ought to be the principal fruit of true knowledge. Our minds are unsatisfied unless they know not only *that* a thing is but *why* it is" (1964, p. 330).

The notion that explanation is a major goal of science—or any goal of science at all—has not been universally adopted. In the 1911 edition of *The Grammar of Science*, Karl Pearson spoke for a large group of physical scientists when he said, "Nobody believes that science explains anything; we all look upon it as a shorthand description, as an economy of thought" (1957, p. xi). Around the turn of the century, many physicists and chemists held this view about the nonexplanatory character of science because they were convinced that the existence of such micro-entities as electrons, atoms, and molecules could not be scientifically established; indeed, many believed that knowledge of the microstructure of matter is beyond the scope of science. Some argued that this is not really a limitation on the power of science, for the proper business of science is to enable us to make reliable predictions—or, more accurately, to make inferences from observed facts to unobserved facts—but not to try to explain anything. In its most extreme form—which is, incidentally, the version Pearson espoused—this thesis concerning the nature of science holds that the *sole* purpose of science is to enable us to predict our future experiences.

Several distinct issues are involved here. The first issue has to do with *phenomenalism*. This is the doctrine that the only reality consists of human sensations—that what we normally take to be ordinary physical objects are actually nothing more than complex combinations of sensations. One classic expression of this thesis can be found in Ernst Mach's *The Analysis of Sensations* (1914), and it was embraced by a number of early adherents of logical positivism. According to this view, the aim of science is to discover regular patterns among our sensations that will enable us to predict future sensations. Since, in my opinion, phenomenalism has been effectively discredited on a number of grounds, I do not intend to discuss it further in this book. I shall adopt the standpoint of *physicalism*, which holds that perception (of a fairly direct sort) provides us with reliable—though not incorrigible—knowledge of ordinary middle-sized physical objects and events. We have, I believe, sound reasons for taking such entities as flowers, rocks, and sneezes to be real.

The controversy regarding *instrumentalism* and *theoretical realism* is another issue that arises in this context. Instrumentalism is the doctrine that scientific theories that seem to make reference to unobservable entities are not to be considered literally true; instead, they should be regarded as useful instruments for dealing with the observable events, objects, and properties that we find in our environment. For example, some philosophers and scientists have claimed that although such perceptible entities as thermometers, pressure gauges, and containers of gas are real enough, atoms and molecules, which are too small to be observed, are unreal. In response to the successes of the molecular-kinetic theory of gases in the latter part of the nineteenth century, instrumentalists like Pearson and Mach could reply that atoms and molecules are useful fictions that enable us to make excellent predictions about the behavior of gases. At that time, however, many still insisted that there was no compelling evidence that such entities actually exist. As I shall explain in chapter 8, it seems to me that nineteenth-century physical scientists were justified in taking an agnostic view concerning the nature of microphysical entities, but that decisive evidence for the existence of atoms and molecules emerged early in the twentieth century. For the moment, however, I merely want to call attention to the thesis of instrumentalism.

If one maintains that atoms and molecules are real entities, then it may be plausible to claim that they enable us to explain the behavior of gases. Pressure, for example, is explained on the basis of collisions of small material particles (which obey Newton's laws of motion) with the walls of the container. If, however, atoms and molecules are mere fictions, it does not seem reasonable to suppose that we can *explain* the behavior of a gas by saying that it acts *as if* it were composed of small particles. Since instrumentalists do not have any alternative explanation of such phenomena, they have often taken refuge in the view that providing explanations is no part of the scientific enterprise anyhow.

The main idea of Mach, Pearson, and others concerning "shorthand descriptions" and "economy of thought" may be illustrated by Johann Jakob Balmer's formula for a particular series of lines in the spectrum of hydrogen. In 1885, Balmer published a formula for the wavelengths of the spectral lines, which can be written,

$$1/\lambda = R(1/2^2 - 1/n^2),$$

where n takes successive integral values beginning with 3, and R is now known as the Rydberg constant. When he worked out this relationship, Balmer was aware of the wavelengths of four lines that had been measured precisely:

H_α = 6562.10 Å (n = 3, red)
H_β = 4860.74 Å (n = 4, green)
H_γ = 4340.1 Å (n = 5, blue)
H_δ = 4101.2 Å (n = 6, violet)

Additional lines in the Balmer series have subsequently been discovered, and the measured values of their wavelengths agree well with the values that come from Balmer's formula. Obviously, it is more economical to write the simple formula than to keep a list of the numerical values for the wavelengths of all of the lines. Balmer also speculated that his formula could be generalized in the following way:

$$1/\lambda = R(1/m^2 - 1/n^2),$$

where m has been substituted for 2 in the previous formula. Additional series that fit this generalized formula have been found:

$m = 1 \rightarrow$ Lyman series
$m = 2 \rightarrow$ Balmer series
$m = 3 \rightarrow$ Paschen series
$m = 4 \rightarrow$ Brackett series
$m = 5 \rightarrow$ Pfund series

This formula yields an even more economical description of the lines in the hydrogen spectrum, but manifestly neither formula *explains* anything about the spectral lines. According to the instrumentalist, it seems, the ultimate aim of science is to find formulas of the foregoing sorts that enable us to describe concisely certain known facts and to predict accurately others that may be discovered in the future.

The instrumentalist view has also been highly influential in psychology, where behaviorists have maintained that their science is concerned only with the stimuli that impinge upon their subjects (humans or animals of other species) and the physical responses that are elicited under specifiable circumstances. These stimuli and responses can be objectively observed and measured. Internal psychological states—for example, feelings of anxiety, hunger, or love—are highly subjective; they are not amenable to objective measurement. They can, however, be regarded as fictions—sometimes called "intervening variables"—which do not literally describe any psychological reality, but which do have instrumental value in describing stimulus-response relationships and in predicting observable behavior. This kind of instrumentalism has remained influential in psychology well into the twentieth century. A classic account of the situation in psychology at mid-century can be found in (MacCorquodale and Meehl, 1948).

In his preface to the third edition of *The Grammar of Science*, just prior to the previously quoted statement about the nonexplanatory character of science, Pearson remarks, "Reading the book again after many years [the first edition was published in 1892, the second in 1900], it is surprising to find how the heterodoxy of the 'eighties had become the commonplace and accepted doctrine of to-day." For some years now, it seems to me, the "commonplace and accepted doctrine" of 1911 has been heterodox, and theoretical realism has become the orthodox view. However, theoretical realism has quite recently come under strong attack by Bas van Fraassen (1980) and Hilary Putnam (1982, 1982a), among others. One wonders to what extent such changes in philosophic doctrine are mere matters of changing fashion, rather than solid results based upon strong philosophical arguments. I shall return to this question in chapter 8.

Although the view that explanation is outside of the domain of science often goes hand in hand with the denial of theoretical realism, they do not always go together. For example, a letter to the editor of the *Scientific American* offered the following comment on an article that had claimed great explanatory merit for the quark hypothesis:

> The quark hypothesis describes the behavior of the subatomic particles in a successful and concise manner. No 'explanation' of these particles' behavior is, of course, ever possible through either pure or experimental science. "Why?" is a question left to philosophy and is not a proper part of any physical theory. (McCalla, 1976, p. 8)

The author of this letter does not appear to be denying the reality either of quarks or of other microphysical entities. It would be incorrect to give the impression that a denial of realism is the only ground on which people maintain that explanation is outside of the domain of science. In some cases, I suspect, such views rest upon the anthropomorphic notion that 'explanations' must appeal to human or superhuman purposes. I do not know what reasons motivated the author of the foregoing letter.

Conversely, it would also be a mistake to suppose that everyone who rejects theoretical realism denies that science has explanatory power. As we shall see in greater detail in chapter 4, van Fraassen, whose agnostic attitude regarding microphysical entities makes him an antirealist, offers a powerful account of scientific explanation (1980, chap. 5).

The question whether science can provide explanations as well as descriptive knowledge arises poignantly in contemporary quantum mechanics. In a popular article entitled "The Quantum Theory and Reality," Bernard d'Espagnat (1979) brings out this issue with admirable clarity. At the outset, he focuses explicit attention upon the problem of explanation:

> Any successful theory in the physical sciences is expected to make accurate predictions. . . . From this point of view quantum mechanics must be judged highly successful. As the fundamental modern theory of atoms, of molecules, of elementary particles, of electromagnetic radiation, and of the solid state it supplies methods for calculating the results of experiments in all these realms.
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> Apart from experimental confirmation, however, something more is generally demanded of a theory. It is expected not only to determine the results of an experiment but also to provide some understanding of the physical events that are presumed to underlie the observed results. In other words, the theory should not only give the position of a pointer on a dial but also explain why the pointer takes up that position. When one seeks information of this kind in the quantum theory, certain conceptual difficulties arise. (P. 158)

The bulk of the article is occupied with a discussion of some perplexing experiments that are closely related to the famous problem raised by Einstein, Podolsky, and Rosen (1935). Given the recalcitrant character of that problem, the last sentence in the d'Espagnat quotation may just be the understatement of the century. After presenting these problems in some detail, d'Espagnat then considers the possibility of simply denying that science provides explanations, and rejects it. Having noted that the results of the experiments in question agree with the predictions of quantum theory, he continues:

> One conceivable response to the . . . experiments is that their outcome is inconsequential. . . . the results are merely what was expected. They show that the theory is in agreement with experiment and so provide no new information. Such a reaction would be highly superficial. (P. 181)

In this judgment, I believe, d'Espagnat is altogether correct. I shall say more about these issues in chapter 9.

It is now fashionable to say that science aims not merely at describing the world; it also provides *understanding*, *comprehension*, and *enlightenment*. Science presumably accomplishes such high-sounding goals by supplying scientific explanations. The current attitude leaves us with a deep and perplexing question, namely, if explanation does involve something over and above mere description, just what sort of thing is it? The use of such honorific near-synonyms as "understanding," "comprehension," and "enlightenment" makes it sound important and desirable, but does not help at all in the philosophical analysis of explanation—scientific or other. What, over and above descriptive knowledge of the world, is required in order to achieve understanding? This seems to me to pose a serious philosophical problem, especially for those who hold what must be considered the most influential contemporary theory concerning the nature of scientific explanation (see Salmon, 1978).

The main purpose of the present book is to examine the nature of scientific explanation. It will become clear, as the discussion unfolds, that the issues to be considered apply not only to modern physics but rather to the whole range of scientific disciplines from archaeology to zoology, including a great many that occupy alphabetically intermediate positions. The approach will be both iconoclastic and constructive. I shall level what seem to me to be grave criticisms against what currently qualifies as 'the received view,' but I shall also try to elaborate a more satisfactory alternative account.

OTHER TYPES OF EXPLANATION

It is advisable, I believe, to begin with some brief remarks circumscribing the concept that is the object of our attention. The term "explanation" has a number of uses, many of which are beyond the scope of the present discussion. We are often asked, for example, to explain the meaning of a word; in such cases, something akin to a dictionary definition may constitute an adequate response. Someone might ask for an explanation of the meaning of a story or a metaphor; here a translation of figurative or metaphorical language into literal terms seems called for. A friend might ask us please to explain how to find the location of a party; in this instance, a set of detailed instructions or a map might do quite well. In none of these cases is a scientific explanation of a natural phenomenon being requested. It is crucially important to distinguish *scientific* explanations from such other types. Michael Scriven once complained that one of Carl G. Hempel's models could not accommodate the case in which one 'explains' by gestures to a Yugoslav garage mechanic what is wrong with a car. I am in complete agreement with Hempel's response: "This is like objecting to a metamathematical definition of proof on the ground that it does not fit the use of the word 'proof' in 'the proof of the pudding is in the eating,' nor in '86 proof Scotch' " (1965, p. 413). As Hempel remarks, a model of *scientific* explanation that did accommodate such cases would ipso facto be defective.

It is worth noting that none of the foregoing requests for explanations would normally be posed by asking a why-question. In some of the cases, we are asking *what* something means, or *what* is wrong with the car. In other cases, we are asking *how* a mathematical proof goes, or *how* to get to the party. A request for a scientific explanation, in contrast, can always be reasonably posed by means of a why-question. If the request is not originally formulated in such terms, it can, I believe, be recast as a why-question without distortion of meaning (see Bromberger, 1966). Indeed, van Fraassen's recent account of scientific explanations characterizes them *essentially* as answers to why-questions (1980, chap. 5).

It is crucial to recognize, however, that not all—or even most—why-questions are requests for scientific explanations. Why, we might ask, did one employee receive a larger raise than another? Because she had been paid less than a male colleague for doing the same kind of job. In this case, a *moral or legal justification* is the appropriate response. Why, someone might ask, did you go to the drugstore today? The answer, "To get some aspirin for my headache," constitutes a *practical justification*. When Job asked God why he had been singled out for such extraordinary misfortune and suffering, he seems to have been seeking *religious consolation*. It would, of course, spawn endless philosophical mischief to confuse justification or consolation with scientific explanation. Moreover, it would be a sadistic joke to offer a scientific explanation when consolation is sought. To tell a bereaved widow, who asks why her husband was taken from her, that (as she is fully aware) he was decapitated in an automobile accident and that decapitation is invariably fatal, would simply be wanton cruelty.

LAPLACIAN EXPLANATION

Although one particular philosophical account of scientific explanation has enjoyed considerable recent influence, there are others that have both historical and contemporary significance. As a point of departure for discussion of various general conceptions of scientific explanation, I should like to consider a historical example. Father Eusebio Francisco Kino was a Jesuit missionary who, in the latter part of the seventeenth century, founded a series of missions in what is now northern Mexico and southern Arizona. Kino was a highly educated man who was well trained in astronomy, cartography, and mathematics. While in Spain, just prior to his journey to North America, he observed the great comet of 1680. Writing to a colleague, he began by giving a scientifically accurate description of its appearance and motion; he then went on to comment upon its portent for humanity:

> It appears that this comet, which is so large that I do not know whether or not the world has ever seen one like it or so vast, promises, signifies, and threatens many fatalities . . . its influence will not be favorable. And therefore it indicates many calamaties for all Europe . . . and signifies many droughts, hunger, tempests, some earthquakes, great disorders for the human body, discords, wars, many epidemics, fevers, pests, and the deaths of a great many people, especially of some very prominent persons. May God our Lord look upon us with eyes filled with pity.
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> And because this comet is so large it signifies that its fatalities will be more universal and involve more peoples, persons, and countries. And since it is lasting so long a time . . . it indicates that its evil influence will afflict mortals for many years. (Bolton, 1960, pp. 62–63)

In those days, even the well-educated saw comets as divine omens of disaster. Kino was no ignorant, country parish priest.

Just a few years later, in 1687, Isaac Newton published the *Principia*. In this work, comets were explained as planetlike objects that move around the sun in highly eccentric orbits. The astronomer Edmund Halley—for whom the great comet of 1682 was named, and who was instrumental in securing the publication of the *Principia*—composed an "Ode to Newton," which was prefixed to that work. In it he wrote:

Now we know
The sharply veering ways of comets, once
A source of dread, nor longer do we quail
Beneath appearances of bearded stars.
(Newton, 1947, p. xiv)

The return of Halley's comet in 1759 marked a major triumph of classical mechanics.

Writing at the beginning of the nineteenth century, P. S. Laplace, the famous advocate of mechanistic determinism, drew an eloquent contrast between the scientific understanding of comets provided by Newtonian physics and the superstitious lack of understanding that led people, in pre-Newtonian times, to regard them as mysterious signs sent by inscrutable supernatural powers.

> But as these phenomena occurring and disappearing at long intervals, seemed to oppose the order of nature, it was supposed that Heaven, irritated by the crimes of earth, had created them to announce its vengeance. Thus, the long tail of the comet of 1456 spread terror throughout Europe. . . . This star after four revolutions has excited among us a very different interest. The knowledge of the laws of the system of the world acquired in the interval had dissipated the fears begotten by the ignorance of the true relationship of man to the universe; and Halley, having recognized the identity of this comet with those of 1531, 1607, and 1682, announced its next return for the end of the year 1758 or the beginning of the year 1759. The learned world awaited with impatience this return which was to confirm one of the greatest discoveries that have been made in the sciences. (Laplace, 1951, p. 5)

Although the elimination of superstitious fears is, undoubtedly, one of the major benefits of scientific explanations—and I think it is important to keep that fact in mind at times, such as the present, when irrationalism seems rampant[1]—it can hardly be regarded as their main purpose. For suppose that comets had been considered good omens—much to be hoped for, and sources of great joy when they appeared—but equally haphazard as far as anyone could tell. As objects of rejoicing rather than of fear, they would not have been any less mysterious.

There is some temptation, I believe, to view scientific explanation solely in psychological terms. We begin to wonder about some phenomenon—

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[1] As just one indication, consider the remarkable strength of the movement to mandate the teaching, in public schools, of the biblical account of the origin of the world, and of life within it, under the title "creation-science." See (Martin Gardner, 1981) for many additional examples, and (Singer and Benassi, 1981) for an interesting discussion.

be it familiar or unfamiliar, an object of joy or an object of fear—and we are intellectually (and perhaps emotionally) ill at ease until we can summon a set of facts that we take to explain it. Whether we have successfully explained the phenomenon, on this view, depends entirely upon whether we have overcome our psychological uneasiness and can feel comfortable with the phenomenon that originally set us to wondering. We need not object to this conception merely on the ground that people often invoke false beliefs and feel comfortable with the 'explanation' thus provided—as when the behavior of a member of a racial minority is 'explained' in terms of a racial stereotype. We can, quite consistently with this approach, insist that adequate explanations must rest upon *true* explanatory bases. Nor need we object on the ground that supernatural 'explanations' are often psychologically appealing. Again, we can insist that the explanation be grounded in *scientific* fact. Even with those restrictions, however, the view that scientific explanation consists in release from psychological uneasiness is unacceptable for two reasons. First, we must surely require that there be some sort of *objective* relationship between the explanatory facts and the fact-to-be-explained. Even if a person were perfectly content with an 'explanation' of the occurrence of storms in terms of falling barometric readings, we should still say that the behavior of the barometer fails objectively to explain such facts. We must, instead, appeal to meteorological conditions. Second, not only is there the danger that people will feel satisfied with scientifically defective explanations; there is also the risk that they will be unsatisfied with legitimate scientific explanations. A yearning for anthropomorphic explanations of all kinds of natural phenomena—for example, the demand that every explanation involve conscious purposes—sometimes leads people to conclude that physics doesn't *really* explain anything at all (recall the letter about the quark theory that was previously quoted in part). Some people have rejected explanations furnished by general relativity on the ground that they cannot visualize a curved four-dimensional space–time. The psychological interpretation of scientific explanation is patently inadequate.

Another conception, equally inadequate, may be suggested by Laplace's remarks. It is sometimes claimed that scientific explanation consists in reducing the unfamiliar to the familiar—as when Newton explained comets by showing that they are objects that behave in essentially the same way as planets, whose motions were by then quite well known. Olbers' paradox constitutes a clear refutation of that thesis. In 1826, the astronomer Heinrich Olbers showed that, on Newtonian principles, the entire night sky should be brilliantly luminous—every region shining at least as brightly as the midday sun.[2] Yet the fact is that for the most part, except for the moon

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[2] Edmund Halley anticipated Olbers when he wrote (1720), "If the number of the Fixt

and a few points of light coming from visible stars or planets, the night sky is dark. The explanation of this familiar fact, it turns out, takes us into some of the more esoteric reaches of modern cosmology, involving such concepts as the mean free path of photons and non-Euclidean geometry. In this case, the familiar is explained in terms of highly unfamiliar notions. Other examples, which exhibit a similar character, are easily found. It is a familiar fact that a metal table will support a coffee cup, but the explanation of this fact involves the details of atomic structure and the appeal to conceptions as unfamiliar to everyday experience as the Pauli exclusion principle. We need not linger longer over this conception, even though it has had prominent adherents.[3]

Still another characterization of scientific explanation is suggested by Laplace's discussion of comets, namely, that explanation consists in showing that what appears to be haphazard does actually exhibit some regularity. Although this appeal to regularities in nature is a step in the right direction, it certainly cannot be the whole story. The basic reason is that *although some regularities have explanatory power, others simply cry out to be explained.* Newton's theories explained comets by bringing them within the scope of the laws of universal gravitation and motion. In the context of classical physics, that sort of explanation is extremely satisfying, as Laplace made clear. At the same time, Newton's theories also explained the tides. The regular ebb and flow of the tides had been known to mariners for centuries prior to Newton; moreover, the correlation between the behavior of the tides and the position and phase of the moon was well known. These are the sorts of regularities that arouse, rather than satisfy, our intellectual curiosity. Without Newtonian physics, the tides were not really understood at all.

Consider another example. Suppose it has been noticed that on days when clothes hung out on the line tend to dry more slowly than usual, airplanes at nearby airports require greater than average distances to get off of the ground. Mr. Smith, whose business involves the use of a small airplane, complains to his wife one evening about the difficulty he had getting airborne that day. Even if both of them are fully aware of the previously mentioned regularity, it could hardly be considered an explanation of his difficulty for her to mention that the wash had taken an unusually long time to dry that day.[4]

Stars were more than finite, the whole superficies of their apparent Sphere [i.e., the sky] would be luminous." Quoted in (Misner et al., 1973, p. 756).

[3] For example, Holton and Brush (1973, p. 185) remark, "Perhaps it is not too frivolous to hold that 'to explain' means to reduce to the familiar, to establish a relationship between what is to be explained and the (correctly or incorrectly) unquestioned preconceptions." Bridgman (1928, p. 37) expresses a similar view, "I believe that examination will show that the essence of an explanation consists in reducing a situation to elements with which we are so familiar that we accept them as a matter of course, so that our curiosity rests."

I am dwelling upon Laplace's conception of scientific explanation not merely to exhibit and refute what I take to be patently mistaken notions, but primarily because he provided an account that was especially appropriate within the context of classical physics. Without making any claims about the historical influence of Laplace, I suspect that the conceptions he expressed have had a large—and not altogether beneficial—effect upon contemporary thought about scientific explanation. In the remainder of this chapter, I should like to support and amplify this general assessment. In so doing, I shall extract three distinct general conceptions of scientific explanation, all of which have historical as well as contemporary significance.

THREE BASIC CONCEPTIONS

Laplace attributed our ability to explain comets to our knowledge of the laws of nature. Twentieth-century philosophers have echoed that view by maintaining that, *with the aid of suitable initial conditions, an event is explained by subsuming it under one or more laws of nature.* If these laws are regarded as deterministic, this formulation becomes hardly more than a translation into more up-to-date and less colorful terminology of Laplace's famous statement:

> Given for one instant an intelligence which could comprehend all of the forces by which nature is animated and the respective situation of the beings who compose it—an intelligence sufficiently vast to submit all these data to analysis—it would embrace in the same formula the movements of the greatest bodies of the universe and those of the lightest atom; for it, nothing would be uncertain and the future, as the past, would be present to its eyes. (1951, p. 4)

Such an intelligence would exemplify the highest degree of scientific knowledge; it would, on Laplace's view, be able to provide a complete scientific explanation of any occurrence whatsoever.

There are, it seems to me, at least three distinct ways in which such Laplacian explanations can be construed. In order to relate them to the modern context, we will need to introduce a bit of technical terminology. It is customary, nowadays, to refer to the event-to-be-explained as the *explanandum-event*, and to the statement that such an event has occurred

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[4] Those readers who are unacquainted with this example can find an explanation of the regularity in chapter 9, pp. 268–69.

as the *explanandum-statement*. Those facts—both particular and general—that are invoked to provide the explanation are known as the *explanans*. If we want to refer specifically to statements that express such facts, we may speak of the *explanans-statements*. The explanans and the explanandum taken together constitute the explanation. Let us now look at the three conceptions.

(1) *Epistemic conception*. Suppose that we attempt to explain some occurrence, such as the appearance of a particular comet at a particular place and time. By citing certain laws, together with suitable initial conditions, we can deduce the occurrence of the event-to-be-explained. By employing observational data collected when his comet appeared in 1682, Halley predicted its return in 1759.[5] These data, along with the laws employed in the deduction, subsequently provided an explanation of that appearance of the comet. This explanation could be described as *an argument to the effect that the event-to-be-explained was to be expected by virtue of the explanatory facts*. The key to this sort of explanation is *nomic expectability*. An event that is quite unexpected in the absence of knowledge of the explanatory facts is rendered expectable on the basis of lawful connections with other facts. Nomic expectability as thus characterized is clearly an epistemological concept. On this view, we can say that there is a relation of *logical necessity* between the laws and the initial conditions on the one hand, and the event-to-be-explained on the other—though it would be more accurate to say that the relation of logical necessity holds between the explanans-statements and the explanandum-statement.

(2) *Modal conception*. Under the same circumstances we can say, alternatively, that because of the lawful relations between the antecedent conditions and the event-to-be-explained there is a relation of *nomological necessity* between them. In Laplace's *Essay*, the discussion of determinism is introduced by the following remarks:

> All events, even those which on account of their insignificance do not seem to follow the great laws of nature, are a result of it just as necessarily as the revolutions of the sun. In ignorance of the ties which unite such events to the entire system of the universe, they have been made to depend upon final causes or upon hazard . . . but these imaginary causes have gradually receded with the widening bounds of knowledge and disappear entirely before sound philosophy, which sees in them only the expression of our ignorance of the true causes. (1951, p. 3)

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[5] Actually Halley did not make a very precise prediction, for he did not take account of the perturbations in the orbit due to Jupiter and Saturn. This was done by Clairaut; see (Laplace, 1951, p. 6).

Nomological necessity, it might be said, derives from the laws of nature in much the same way as logical necessity rests upon the laws of logic. *In the absence of knowledge of the explanatory facts, the explanandum-event* (the appearance of the comet) was something that *might not have occurred for all we know; given the explanatory facts it had to occur*. The explanation exhibits the nomological necessity of the fact-to-be-explained, given the explanatory facts. Viewing the matter in this way, one need not maintain that an explanation is an argument showing that the explanandum-event had to occur, given the initial conditions. Although a deductive argument can be constructed (as in the epistemic account) within which a relation of logical entailment obtains, an explanation need not be regarded as such an argument, or as any kind of argument at all. In comparing the epistemic and modal conceptions, it is important to be clear on the roles of the two kinds of necessity. In the epistemic conception, the relation of *logical* necessity obtains between the entire explanans and the explanandum by virtue of the laws of deductive logic. In the modal conception, the relation of *physical* necessity holds between particular antecedent conditions and the explanandum-event by virtue of the general laws, which we are taking to be part of the explanans.[6]

(3) *Ontic conception*. There is still another way of looking at Laplacian explanations. If the universe is, in fact, deterministic, then nature is governed by strict laws that *constitute* natural regularities. Law-statements describe these regularities. Such regularities endow the world with patterns that can be discovered by scientific investigation, and that can be exploited for purposes of scientific explanation. To explain an event—to relate the event-to-be-explained to some antecedent conditions by means of laws—is to fit the explanandum-event into a discernible pattern. This view seems to be present in Laplace's thought, for he remarks that comets "seemed

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[6] The contrast being suggested is well illustrated by a controversy between Hempel and Scriven concerning the role of laws in scientific explanation. As we have seen, Hempel (1965) insists that general laws be present in the explanans. Scriven (though he is not a proponent of the modal conception) argues that a set of particular antecedent conditions may constitute an adequate explanation of a particular event; consequently, the explanans need not include reference to any general laws. A law that provides a connection between the explanans and the explanandum constitutes a "role-justifying ground" for the explanation by showing, roughly speaking, that the explanans is explanatorily relevant to the explanandum. For Hempel, the laws of logic—which provide the relation of relevance of the explanans to the explanandum—are not part of the explanation, but can be called upon to justify the claim that a given explanans has explanatory force with respect to some explanandum. Scriven invokes similar considerations to argue that general laws of nature should remain outside of scientific explanations to be called upon, if necessary, to support the claim that a given explanation is adequate. See (Scriven, 1959) for details; Hempel's reply is given in his (1965, pp. 359–364).

to oppose the order of nature" before we knew how to explain them, but that subsequent "knowledge of the laws of the system of the world" provided understanding of them (1951, p. 5). Moreover, as noted previously, he speaks of "the ties which unite such events to the entire system of the universe" (1951, p. 3). Because of the universal (nonstatistical) character of the laws involved in Laplacian explanations, we can also say that given certain portions of the pattern of events, and given the lawful relations to which the constituents of the patterns conform, other portions of the pattern of events must have certain characteristics. Looking at explanation in this way, we might say that *to explain an event is to exhibit it as occupying its* (nomologically necessary) *place in the discernible patterns of the world.*

These three general conceptions of scientific explanation all seem to go back at least to Aristotle. We have already remarked on his identification of certain sorts of syllogisms as explanations; this conforms to the epistemic conception that regards explanations as deductive arguments.[7] He seems to be expressing the modal conception when he remarks that "the proper object of unqualified scientific knowledge is something which cannot be other than it is" (*Posterior Analytics*, 1. 2. 71b14–16). And in the same context, discussing the nature of the syllogism that yields "scientific knowledge," he says, "The premises must be the causes of the conclusion, better known than it, and prior to it; its causes, since we possess scientific knowledge of a thing only when we know its cause; prior, in order to be causes; antecedently known, this antecedent knowledge being not our mere understanding of the meaning, but knowledge of the fact as well" (ibid., 71b29–33). These remarks suggest an ontic conception.

In the twentieth century, we still find the same three notions figuring prominently in philosophical discussions of scientific explanation. The *epistemic conception* represents the currently 'received view,' which has been advocated by such influential philosophers as Braithwaite, Hempel, Nagel, and Popper. It was succinctly formulated by Hempel in the following way:

> [An] explanatory account may be regarded as an argument to the effect that the event to be explained . . . was to be expected by reason of certain explanatory facts. These may be divided into two groups: (i) particular facts and (ii) uniformities expressed by general laws. (1962a, p. 10)

The *modal conception* has been clearly affirmed by D. H. Mellor: "The thesis is that we call for explanation only of what, although we know it is so, might have been otherwise for all else of some suitable sort we know" (1976, p. 234). In what does an explanation consist?

> We want to know why what might not have happened nonetheless did. Causal explanation closes the gap by deducing what happened from known earlier events and deterministic laws. So in this respect it satisfies the demand for explanation: what follows from what is true must also be true. Given the causal explanans, things *could not have happened otherwise* than the explanandum says. (1976, p. 235, italics added)

G. H. von Wright (1971) gives concise expression to this same conception: "What makes a deductive-nomological explanation 'explain,' is, one might say, that it tells us why *E had* to be (occur), why *E* was *necessary* once the basis [body of explanatory facts] is there and the laws are accepted" (p. 13, italics in original). This same view can be found explicitly in C. S. Peirce (1932, 2:776).

The *ontic conception* is the one for which I shall be arguing. In Salmon (1977a, p. 162), I offered the following characterization: "To give scientific explanations is to show how events . . . fit into the causal structure of the world." Hempel summarizes the import of his major monographic essay, "Aspects of Scientific Explanation" (1965a, p. 488), in rather similar terms: "The central theme of this essay has been, briefly, that all scientific explanation involves, explicitly or by implication, a subsumption of its subject matter under general regularities; that it seeks to provide a systematic understanding of empirical phenomena by showing that they fit into a nomic nexus." I find this statement by Hempel in almost complete accord with the viewpoint I shall be advocating; my suggestion for modification would be to substitute the words "*how* they fit into a *causal* nexus" for "*that* they fit into a *nomic* nexus." It seems to me that Hempel began the "Aspects" article with statements clearly indicating that he embraced the epistemic conception, but he ended with a summary that seems closer to the ontic conception. Because these three conceptions had not been explicitly formulated and distinguished at the time of his writing, he was, I think, unaware of any conflict. As we shall see in subsequent

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[7] In the *Posterior Analytics* (1928, 1. 2. 71b18–24), Aristotle writes: "By demonstration I mean a syllogism productive of scientific knowledge, a syllogism, that is, the grasp of which is *eo ipso* such knowledge. Assuming that my thesis as to the nature of scientific knowledge is correct, the premises of demonstrated knowledge must be true, primary, immediate, better known than and prior to the conclusion, which is further related to them as effect to cause. Unless these conditions are satisfied, the basic truths will not be 'appropriate' to the conclusion. Syllogism there may indeed be without these conditions, but such syllogism, not being productive of scientific knowledge, will not be demonstration." Richard Jeffrey (1969) offers an illuminating comparison between this Aristotelian view and Hempel's D-N account.

chapters, there are profound differences, especially in the context of statistical explanation.

Those philosophers who have adopted the ontic conception of scientific explanation have generally regarded the pattern into which events are to be fit as a causal pattern. This feature of the view is brought out explicitly in the quotation from Aristotle. It certainly was present in Laplace's mind when he wrote, "Present events are connected with preceding ones by a tie based upon the evident principle that a thing cannot occur without a cause which produces it" (1951, p. 3), and "We ought then to regard the present state of the universe as the effect of its anterior state and as the cause of the one which is to follow" (1951, p. 4). It was also explicit in my formulation quoted previously. Hempel, however, does not share this notion; for him the pattern is lawful (nomic), but the laws involved need not be causal laws (1965, pp. 352–354). In view of well-known Humean problems associated with causality, it might *seem* desirable to try to avoid reference to causal laws in dealing with scientific explanation. Nevertheless, I shall try to show that we need not purge the causal notions; indeed, I shall argue that they are required for an adequate theory of scientific explanation. In order to implement the causal version of the ontic conception, however, it will be necessary to examine the nature of causal relations with considerable care, and to show how they can be employed unobjectionably in a theory of scientific explanation. This problem will be postponed until chapters 5–7.

The foregoing three ways of thinking about scientific explanation may *seem* more or less equivalent—with somewhat distinct emphases perhaps—but hardly more than different verbal formulations. This is true as long as we are talking about the kind of explanation that involves appeal to universal laws only. A striking divergence will appear, however, when we consider explanations that invoke statistical (or probabilistic) laws. In the deterministic framework of Laplace's thought, all of the fundamental laws of nature are taken to be strictly universal; any appeal to probabilities is merely a reflection of human ignorance. In twentieth-century science, the situation is quite different. There is a strong presumption in contemporary physics that some of the basic laws of nature may be irreducibly statistical—that probability relations *may* constitute a fundamental feature of the physical world. There is, to be sure, some disagreement as to whether determinism is true or false—whether modern physics requires an indeterministic interpretation. I do not want to prejudge this issue. In the attempt to elaborate a philosophical theory of scientific explanation, it seems to me, we must try to construct one that will be viable in either case. Therefore, we must leave open the possibility that some scientific explanations will be unavoidably statistical. This means that we must pay careful attention to the nature of statistical explanation.

AN OUTLINE OF STRATEGY

Much of the contemporary literature on scientific explanation arises directly or indirectly in response to the classic 1948 Hempel-Oppenheim paper, "Studies in the Logic of Explanation."[8] In it the authors attempt to provide a precise explication of what has come to be known as the deductive-nomological or D-N model of scientific explanation. They did not invent this mode of scientific explanation, nor were they the first philosophers to attempt to characterize it. As mentioned previously, its roots go back at least to Aristotle, and it is strongly suggested in such works as Arnauld's *The Art of Thinking* ("Port-Royal Logic") and Laplace's *Philosophical Essay on Probabilities*. In none of the anticipations by these or other authors, however, do we find the precision and detail of the Hempel-Oppenheim account. One might almost say that 1948 marks the division between the prehistory and the history of the philosophical study of scientific explanation. When other such influential philosophers as R. B. Braithwaite (1953), Ernest Nagel (1961), and Karl R. Popper (1935, 1959) espoused a similar account of deductive explanation, it achieved virtually the status of a 'received view.'[9]

According to the 'received view,' particular facts are explained by subsuming them under general laws, while general regularities are explained by subsumption under still broader laws. If a particular fact is successfully subsumed under a lawful generalization, it is, on this view, completely explained. One can legitimately ask for an explanation of the general law that figures in the explanation, but an explanation of the general law would be a different and additional explanation, not an essential part of the original explanation of the particular fact. For example, to explain why this particular penny conducts electricity, it suffices to point out that it is composed of copper and that all copper objects conduct electricity. If we are asked to explain why copper conducts electricity, we may give a further *distinct* explanation in terms of the fact that copper is a metal with conduction electrons that are not tightly bound to individual atoms and are free to move when an electric potential is applied.

Most proponents of this subsumption theory maintain that some events can be explained statistically by subsumption under statistical laws in much the same way that other events—such as the fact that the penny just inserted

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[8] See (Rescher, 1970) for an extensive bibliography on scientific explanation up to the date of its publication.

[9] Although Popper's *Logik der Forschung* (1935) contains an important anticipation of the D-N model, it does not provide as precise an analysis as was embodied in (Hempel and Oppenheim, 1948). Moreover, Popper's views on scientific explanation were not widely influential until the English translation (Popper, 1959) of his 1935 book appeared. It is for these reasons that I chose 1948, rather than 1935, as the critical point of division between the history and the prehistory of the subject.

behind the fuse conducts electricity—are explained by appeal to universal laws. Thus we can explain the fact that a particular window was broken by pointing out that it was struck by a flying baseball, even though not all, but only most, windows so struck will shatter.

Although I disagreed from the beginning with the proponents of the *standard* subsumption view about the nature of the relation of subsumption of particular facts under universal or statistical generalizations, I did for some time accept the notion that *suitable* subsumption under generalizations is sufficient to explain particular facts. In *Statistical Explanation and Statistical Relevance* (Salmon et al., 1971), I tried to give a detailed account of what seemed to me the appropriate way to subsume facts under general laws for purposes of explanation. This effort led to the elaboration of the statistical-relevance (S-R) model of scientific explanation. As the name suggests, statistical relevance relations play a key role in this model of scientific explanation.

Subsequent reflection has convinced me that subsumption of the foregoing sort is only part—not all—of what is involved in the explanation of particular facts. It now seems to me that explanation is a two-tiered affair. At the most basic level, it is necessary, for purposes of explanation, to subsume the event-to-be-explained under an appropriate set of statistical relevance relations, much as was required under the S-R model. At the second level, it seems to me, the statistical relevance relations that are invoked at the first level must be explained in terms of *causal* relations. The explanation, on this view, is incomplete until the causal components of the second level have been provided. This constitutes a sharp divergence from the approach of Hempel, who explicitly rejects the demand for causal laws (1965, pp. 352–354).

It would be advisable, I believe, to adopt an approach similar to one suggested by Wolfgang Stegmüller (1973, p. 345), who characterized the kind of subsumption under statistical relevance relations provided by the S-R model as "statistical analysis" rather than "statistical explanation." The latter term is reserved for the entity that comprises both the statistical-relevance level and the causal level as well. As (Humphreys, 1981, 1983) and (Rogers, 1981) persuasively argue, statistical analyses have important uses, but they fall short of providing genuine scientific understanding. To emphasize this point, I shall use the term *S-R basis* to refer to the statistical component of an explanation.[10]

The remainder of the present book is divided into two main parts,

corresponding to the two tiers just mentioned. Chapters 2–3, which constitute the first part, deal essentially with the S-R basis; it is regarded as an indispensable portion of the present account of scientific explanation, even though it is no longer taken as a complete model. These two chapters contain a number of important revisions of the S-R model itself, vis-à-vis previous presentations, as well as a good deal of supplementary material. Chapter 3 deals with the difficult concept of physical randomness—that is, objective homogeneity of reference classes—which is required to implement an ontic treatment of statistical explanation adequate to the possibly indeterministic context of contemporary science. The result, I hope, is a substantially improved version of the S-R model—one that can provide a satisfactory basis for the second level. These two chapters contain, roughly, all that can be said, as far as I am aware, regarding scientific explanation of particular facts without invoking causal considerations.

Chapter 4 is a transitional discussion of the three general conceptions (introduced in this chapter) in the light of statistical considerations. It constitutes a serious attempt to provide a much-needed clarification of the explicandum. The conclusion drawn from the discussion is that the ontic conception is the only acceptable one.

Chapters 5–8, which make up the second main part, deal explicitly with causality in scientific explanation. In order to achieve the goal of explicating the role of causality in scientific explanation, it is necessary to develop a theory of causality that, though it borrows heavily from other authors, incorporates various novel elements. Among its conspicuous (though not necessarily novel) features are (1) it is a probabilistic or statistical concept, (2) it places great emphasis upon the distinction between causal *processes* and causal *interactions*, and (3) it takes processes to be more fundamental than events. The reader will have to judge for herself or himself concerning the adequacy of the account of causality offered in these chapters, and its fertility in providing an improved theory of scientific explanation. It is my hope that the result is a theory of scientific explanation that constitutes a significant advance beyond its predecessors.

Chapter 9, the concluding chapter, deals with some general features of my approach to scientific explanation, including some of its shortcomings and limitations. It points the way, I believe, to the kind of research that should extend and deepen our philosophical understanding of scientific explanation.

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[10] In relinquishing the thesis that the S-R model provides an adequate characterization of scientific explanation, I accept as valid most of the criticisms leveled against it by Achinstein (1983). These criticisms do not, however, undermine the utility of the S-R basis as a foundation for scientific explanations.

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