The Second Decade (1958–67) *Manifest Destiny—Expansion and Conflict*

As we have already remarked, the first decade after the publication of the Hempel-Oppenheim paper saw little published criticism—or acknowledgment—of it. Quite possibly this portion of the received view—the box in the upper left corner of Table 1—was accepted with considerable satisfaction for the most part by the philosophy of science community. The situation changed rather dramatically around 1958. This was the year in which the second volume of *Minnesota Studies in the Philosophy of Science* (Feigl et al. 1958) was published, containing Scriven's first article attacking the D-N model. Hanson's *Patterns of Discovery* (1958) appeared during the same year.

In the next few years following 1958 a great many papers on scientific explanation appeared, devoted mainly to issues we have already mentioned. These included, for example, debates on the covering law conception and on the explanation/prediction symmetry thesis. The critiques of the Hempel-Oppenheim account fall into three main categories. First, as we have seen, the Eberle-Kaplan-Montague (1961) critique and replies to it come under the heading of sympathetic efforts to find and eliminate any technical flaws in that explication. Unlike the following two types, these were critiques of the formal explication given in Part III of the Hempel-Oppenheim paper, rather than objections to the preliminary conditions of adequacy advanced in Part I. Second, as we shall see in §2.1, the attacks by Hanson, Scriven, and others were motivated by deep philosophical disagreements with anything resembling the logical empiricist point of view. Third, there were constructive efforts by philosophers such as Bromberger (1962, 1963, 1966) and Scheffler (1957, 1963) who generally accepted something like the received view, and sought ways to improve and perfect it. As we shall see in §2.3, many of the problems raised under the latter two headings were formulated with the aid of putative counterexamples that have since become standard in the literature.

During this time there were also attempts to further elaborate or defend the received view. One major effort in that direction can be found in May Brodbeck's (1962) defense of deductive explanation in her contribution to the third volume in the *Minnesota Studies* series. More significant still is Ernest Nagel's magnum opus, *The Structure of Science* (1961) which contains a great wealth of factual material on explanation in many different branches of science—physical, biological, and social—as well as in history. Nagel begins with a series of examples that include explanations in mathematics, explanations of laws (universal and statistical), deductive explanations of particular facts, probabilistic explanations, functional explanations, and genetic explanations. He also provides searching discussions of such issues as the nature of laws and the status of scientific theories. Although he does call attention to the importance of probabilistic explanation (1961, 22–23), and does discuss examples of probabilistic explanation in history (1961, 550–63), he does not provide an analysis of it. That task was not undertaken in a serious way until Hempel did so in 1962.

This period was a time of intense activity in philosophy of science in America. The Minnesota Center for the Philosophy of Science, under the direction of Herbert Feigl, had been founded in 1953, but had devoted most of its attention during the first few years to problems in the foundations of psychology (see Feigl and Scriven 1956). In the years just before 1958 the topics of discussion became more general (see Feigl et al. 1958). In 1959 Feigl and Grover Maxwell organized an ambitious program for Section L (History and Philosophy of Science) of the American Association for the Advancement of Science, the proceedings of which were published in (Feigl and Maxwell 1961). The Center for the Philosophy of Science at the University of Pittsburgh, was founded in 1960, under the direction of Adolf Grünbaum, and in the same year the Department of History & Philosophy of Science at Indiana University was created with Hanson as Chair. In addition, Robert S. Cohen and Marx Wartofsky of Boston University initiated the Boston Colloquium for the Philosophy of Science in 1960. These entities are still functioning actively. During the two academic years 1961–63, the University of Delaware conducted its Philosophy of Science Seminar (see Baumrin 1963). A great deal of important work in philosophy of science around this time was sponsored by these institutions and was published in preceedings of one kind or another. Scientific explanation was by no means the only topic—nor even the main topic, for the most part—but it was the focus of considerable attention.

Perhaps the most momentous development of the second decade was the clear articulation of theories of statistical explanation. Although a number of authors had already called attention to the need for a model of explanation in which the explanandum followed from the explanans with something less than complete deductive certainty, no real theory of that sort of explanation existed before 1962.[1] Hempel made the first attempt in that year (Hempel 1962), but followed it up with a greatly improved account in (Hempel 1965a, §3, 376–411). Much of the work of the following two decades leans heavily upon this achievement.

2.1 A Major Source of Conflict

Although Hanson was an American by birth and Scriven an Australian, both received their advanced training in England. Ludwig Wittgenstein's *Philosophical Investigations* had been published in 1953, and his influence at Cambridge and Oxford was formidable. During that era a strong opposition developed between the Wittgensteinians, who practiced ordinary language analysis, and the logical empiricists, who might be characterized as artificial language analysts. The Hempel-Oppenheim article is an outstanding example of the use of an artificial formal language for purposes of explicating a fundamental scientific concept. After Hanson and Scriven moved to America, Stephen Toulmin, who was English by birth, was the best known philosopher of science of the ordinary language school remaining in England.[2] Hanson, Scriven, and Toulmin were the most conspicuous Wittgensteinian opponents of the received view of scientific explanation during the second decade.

The basic reason for the opposition between these two schools can be seen quite readily. There is a widely accepted tripartite classification of domains in the study of languages, natural or artificial. *Syntactics* is the area in which we study merely the relationships among symbols, without regard for their relationships either to the users of the symbols or to the objects to which the symbols refer. In ordinary English it is a syntactic rule, I believe, that every sentence must begin with a capital letter and have a dot at the end (recalling that both the exclamation mark and the question mark contain dots). In a formal language, the rules for forming well-formed-formulas (wffs) are part of syntactics. *Semantics* is concerned with the relationships between symbols and the objects to which they refer, but without taking language users into account. Among its important concepts are designation, meaning, and truth. Deductive validity can be construed either syntactically or semantically; for purposes of our discussion it does not matter much which interpretation is chosen. *Pragmatics* takes account of the users of symbols, as well as the interrelations among the symbols and the relationships between the symbols and the entities they stand for. It emphasizes, among many other things, the context in which a statement is made, the purpose of the person who makes it, and the presuppositions that are shared in that context.

The explications offered by the logical empiricists were usually constructed entirely in syntactical and/or semantical terms. The point is well illustrated by the Hempel-Oppenheim treatment of scientific explanation. They start with a standard first order logic, which can be characterized in purely syntactic terms, and proceed to offer semantic rules for its interpretation. All of the key notions—e.g., lawlike sentence, law, theory, explanans—are defined semantically. Pragmatics plays hardly any role at all. Writing in the third decade, Nicholas Rescher (1970, 6–8) points to the many pragmatic features of scientific explanation, but maintains that, for purposes of logical analysis, it is best to abstract from them.

For anyone who focuses primarily on the ordinary uses of language, the pragmatic aspects will be most conspicuous. Some explanations, for example, may be requested by formulating why-questions. The answers are to be judged in terms of the interests and background knowledge of the questioner. An explanation of a particular phenomenon that is entirely satisfactory for one person may be totally inappropriate for another.

It is easy to see why deep philosophical conflict arose in that situation. The ordinary language philosopher finds the logical empiricist insensitive to human needs and interests. The logical empiricist finds the ordinary language philosopher unappreciative of the objective features that determine whether a proffered explanation is a bona fide scientific explanation. The logical empiricists employed formal techniques; the ordinary language philosophers tended to deprecate and avoid them. In those days formal pragmatics did not exist in any highly developed form. As we shall see in the fourth decade, formal pragmatics now plays a crucial role in various approaches to explanation, especially that of Bas van Fraassen.

Hempel was aware from the beginning that when scientists and others offer explanations they often omit parts that are obvious. Often the omitted part is the law. Such explanations are unobjectionable to Hempel if the law is obvious to both the questioner and the respondent; it would be needlessly pedantic to insist on mention of the obvious. Partial explanations and explanation sketches are frequently acceptable. To this degree, at least, Hempel acknowledges the pragmatic aspects of explanation (see 1965a, §4–5, regarding these and other pragmatic considerations). Nevertheless, Hempel insists, when you spell out the correct explanation in complete detail, it will always contain a law.

Scriven takes an opposite attitude. He notices that in many—if not most—cases, an explanation of one particular fact consists in citing another particular fact. Why did the automobile radiator rupture? Because the car was left outside overnight without antifreeze, and the temperature fell to 10° F during the night. If this explanation is questioned, the law concerning the expansion of water upon freezing may be cited, but, according to Scriven, not as a suppressed part of an incomplete explanation. The explanation, as given, was satisfactory. If the law is invoked it serves to justify the explanation, not to complete it.

This view is in strict accord with a thesis about laws that had been shared by some early logical positivists and the ordinary language philosophers, namely, that laws are not statements of fact, but rather, rules of inference. On that view, the sentence "Silver is an electrical conductor" is not a true generalization; it is an 'inference ticket' that entitles one to conclude from "This object is made of silver" that "This object is capable of conducting electricity." Hempel and Oppenheim insisted that a correct D-N explanation be a valid deductive argument, but they did not suggest that the argument include a statement of the rules of deduction to which it conforms. If anyone questions the validity of a given explanation, we can trot out the rules to demonstrate its validity. Philosophers who, like Toulmin, regarded laws as 'inference tickets,' likewise objected to the idea that these rules of inference be included as parts of the explanation itself. They stand on the side, so to speak, to be called up if the correctness of a given explanation is challenged. In making this point about explanation, Scriven refers to the *role-justifying function* of laws (1962, 200, 207, 211).

2.2 Deeper Linguistic Challenges

Not long after Hanson and Scriven had fired their opening salvos against the Hempel-Oppenheim approach, a rather different sort of linguistic approach was initiated by Sylvain Bromberger. Informed by empirical linguistics, which at the time had had virtually no impact on ordinary language philosophers, Bromberger advanced a much more detailed and precise account of explanation than was offered by any of them.

At the beginning of his first paper, "An Approach to Explanation," Bromberger (1962, 72–73) invites consideration of three statements that involve explanation:

(1) The kinetic theory of gases explains why the state equation of vapours near condensation differs markedly from that of an ideal gas.
(2) Someone explained to somebody how World War II might have been avoided.
(3) Newton explained a long time ago the variations of the tides.

He selects statements like (2) for primary attention as a matter of convenience, he says, and he readily extends his considerations to statements like (3). It should be noted, however, that when he gets to the end of this essay he has little to say about statements like (1), and he admits to having no theory that adequately handles them. At the close of the essay he remarks, "The account of the nature of explanation just given falls short of what is eventually wanted: it fails to provide the sort of insight that can be translated into explicit standards and into a patttern of analysis applicable to all explanations and capable of deciding their correctness; it fails to make explicit the criteria that make correct explanations *correct* explanations" (1962, 104–5). So I think the choice of statements like (2)—at least as opposed to those like (1)—is based on more than convenience. What Bromberger admittedly failed to achieve is just the sort of thing Hempel and Oppenheim were trying to accomplish.

From a linguistic standpoint, a statement like (2) is especially tractable because it implies the occurrence of a particular sort of linguistic performance involving two particular individuals or groups of people at some particular time. In addition to the people and the time, such statements incorporate some form of the verb "to explain," and 'something' that is explained, "where the 'something' can be specified by means of an indirect question" (1962, 73). It is close to what Bas van

Fraassen, in the fourth decade, refers to as *the topic* of a question. More precisely, Bromberger is concerned with statements of the form "A E to B W," where "A" and "B" indicate places that can be occupied by terms designating individuals or groups of people, "E" indicates a place where a tensed form of "to explain" occurs, and "W" indicates a place where an indirect question occurs. The heavily linguistic character of this approach is shown by the fact that this characterization could not even purport to apply to explanations in any language other than English. The Hempel-Oppenheim model, by contrast, could apply to explanations offered in myriad different languages.

Bromberger's linguistic analysis proceeds by classifying the verb "to explain," according to a taxonomy provided by Zeno Vendler, as an *accomplishment* term. Other verbs fall into the categories of *activity* terms, *state* terms, or *achievement* terms. Activity terms and accomplishment terms apply to doing something that occupies a span of time, but an accomplishment term refers to an activity that can result in a completion. Explaining and reading are activities that take place in that way, but when an activity term is used in the simple past, it does not imply any completion. If I say I read last night, that does not mean I finished a story, an article, or a book. It is an activity in which I could be engaged and which I could have stopped doing without finishing anything. It means merely that I spent some time engaged in that activity. If I say that I explained in class yesterday, that claim would not be correct unless I had completed an explanation. If no explanation was completed, I could correctly say that I was trying to explain, or that I got part way through an explanation, but not that I explained anything.

> The difference between activity terms and accomplishment terms is readily seen when we compare their simple past tense. Both types have a simple past tense which implies that the continuous present was applicable at some moments in the past. The simple past tense of an activity term is applicable as soon as such moments have passed, and implies only the existence of such moments in the past. Aristotle walked. This implies that during some moments in the past Aristotle was walking. It does not tell whether or not Aristotle is through walking. The simple past tense of accomplishment terms implies more. It implies that relevant activities took place in the past, but furthermore that they have come to an end. And not to a mere stop, but to a conclusion. In other words, the simple past tense of accomplishment verbs entails that something has been finished, completed, that might, in principle, have been left unfinished, incomplete, that might have stopped before coming to its logical end. (1962, 75)

State terms, such as "to know" and "to love" (in their non-episodic senses), truly apply in the simple present tense to individuals at different times even though the individual is in that state just once—e.g., he loves her, said at any time throughout their 35 years of marriage. Achievement terms can be truly applied in the simple present at more than one time only if the individual achieves more than once. She wins at tennis at time t_1 and she wins at tennis at t_2 (where t_1 is different from t_2) only if she wins two different games (or sets, or matches).

Since "to explain" is an accomplishment term, "A good analysis should therefore make explicit the nature of the completion implied by statements in 'A E to B W' form in which 'to explain' occurs in the *simple past tense*; it should bring out what must be the case for a statement in 'A explain*ed* to B W' form to be true' " (1962, 76).

Another item of important concern about statements of the form 'A E to B W' is the nature of the indirect question at the place indicated by "W." Clearly, Bromberger emphasizes, certain sorts of questions are appropriate, while other kinds are inappropriate.

> Many kinds of indirect question can occupy the position indicated by 'W', and they may open on a variety of interrogatives[3]—'Why', 'How', 'Whence', 'Whither', 'What'—but not *every* indirect question is at home there; some would be out of place, awkward, reminiscent of Eisenhower prose, *e.g.* 'what the distance in miles between London and Paris is' or 'whether it will rain tomorrow' or 'what time it is' or 'which sister Sam married'. A good analysis should show why some indirect questions do not sit well in these contexts. (1962, 74)

Bromberger invites us to consider two questions:

(A) What is the height of Mt. Kilimanjaro?

(B) Why do tea kettles emit a humming noise just before the water begins to boil? (1962, 80)

Each of these is a sound question—each has a correct answer and neither has any false presuppositions.

Although he does not know a correct answer for either, he knows quite a bit about what would constitute a correct answer. A correct answer to (A) might be a positive integer followed by an expression, such as "feet," designating a unit of length. If the answer is given in feet, he knows that the number will be greater than 100 and less than 30,000; if some other unit is used, it will be possible to convert the answer into feet, and the same numerical limits will apply. He knows enough about the answer to be able to exclude many expressions—e.g., "12 feet," "Morton White," "19,321 pounds." Although he does not know a correct answer, there is a straightforward sense in which he has thought of a correct answer, for he could write down and understand any integral numeral between "100" and "30,000."

A correct answer for (B) would be statable as a sentence (which may be a conjunction of sentences) following the word "because." This sentence would include mention of something that happens whenever water is just about to boil, and it would include mention of something that creates vibrations of air of suitable frequency and amplitude. Other requirements on an answer might be offered, and they would serve to exclude many expressions as possible answers, but altogether they do not add up to a correct answer. In the case of (B), however, unlike (A), he has not thought of any answer that could possibly be a correct answer.

This is a key feature of explanation. Someone could tell Bromberger the height of Kilimanjaro, but it would be incorrect usage to say anyone had explained it to him. Perhaps there is a geophysical explanation of the height of Kilimanjaro, but question (A) was not a request for it. Question (B) was a request for an explanation. He recapitulates:

(i) I take both (A) and (B) to be sound questions, to admit of a right answer.
(ii) I know, or believe I know, enough about each answer to be able to eliminate a number of possible utterances, *i.e.* of expressions, as not being formulations of it.
(iii) In the case of (A) I can think of some possible utterances that I cannot eliminate in this way.
(iv) In the case of (B) I can think[4] of no expression that I cannot eliminate in this way. (1962, 82)

On the basis of these considerations, Bromberger defines the notion of a *p-predicament* as follows:

> *S is in a p-predicament with regard to question Q if and only if, on S's views, Q admits of a right answer, but S can think of no answer to which, on S's views, there are no decisive objections.* (1962, 82)

He points out that one could not be in a p-predicament with regard to an indirect question beginning with "whether" that requires a "yes" or "no" answer—e.g., whether it is raining here just now. Even if one does not know the correct answer, it surely has been thought of. Similarly, for reasons that have already been mentioned, one would not be in a p-predicament regarding indirect questions like (A), beginning with "what is," that call for quantitative answers. Likewise, one cannot be in a p-predicament with respect to indirect questions, beginning with "which," that call for a selection from a well-defined set of alternatives. In my present state of knowledge I may not know which planet in the solar system has the highest surface temperature, but I believe I have thought of the possible candidates, and I suppose it is either Mercury or Venus.

To arrive at a characterization of explanation, Bromberger also defines the notion of a b-predicament:

> *S is in a b-predicament with regard to Q if and only if the question mentioned in it admits of a right answer, but that answer is beyond what the person men-*

> *tioned can conceive, can think of, can imagine, i.e. is something that that person cannot remember, cannot excogitate, cannot compose.* (1962, 90, italics not in original)

The main difference between a p-predicament and a b-predicament is that the latter refers to a right answer, whereas the former refers to possible answers that cannot be eliminated.

Bromberger offers a series of four hypotheses concerning the nature of explanations, and he rejects the first three. In stating these hypotheses he refers to the person or persons doing the explaining as the *tutor* and to the recipient as the *tutee*. We shall look only at the fourth hypothesis:

> The essential characteristics of explaining episodes are the following:
>
> (a) the question is sound, i.e. admits of a right answer;
>
> (b) the tutor is rational and knows the right answer to the question at the time of the episode;
>
> (c) during the episode the tutor knows, or believes, or at least assumes that at the beginning of the episode the tutee was in a p-predicament with regard to the question,
>
> or that, at the beginning of the episode the tutee was in a b-predicament with regard to the question,
>
> or that, at the beginning of the episode the tutee was in either a p-predicament or a b-predicament with regard to the question;
>
> (d) in the course of the episode the tutor presents the facts that, in his opinion, the tutee must learn to know the right answer to the question;
>
> (e) in the course of the episode the tutor also provides the tutee with such instruction as he (the tutor) thinks necessary to remove the basis of whichever of the states mentioned in (c) he deems the tutee to be in;
>
> (f) at the end of the episode all the facts mentioned in (d) and (e) have been presented to the tutee by the tutor. (1962, 94–95)

Bromberger acknowledges that this lengthy characterization contains redundancies: (a) is entailed by (b); (c) and (f) are entailed by (d) and (e). Removing the redundancies, we have

> The essential characteristics of explaining episodes are the following:
>
> (b) the tutor is rational and knows the right answer to the question at the time of the episode;
>
> (d) in the course of the episode the tutor presents the facts that, in his opinion, the tutee must learn to know the right answer to the question;
>
> (e) in the course of the episode the tutor also provides the tutee with such instruction as he (the tutor) thinks necessary to remove the basis of whichever of the predicaments he deems the tutee to be in.

All the kinds of explanation we have discussed so far fit the schema "A E to B W," where "W" indicates a place where an indirect question occurs. After presenting this account, Bromberger goes on to discuss explanations that fit the schema "A E to B X," where "A," "B," and "E" have the same significance, but "X" is the location of something other than a question, for example, a noun phrase. He rather easily shows how these can be handled in terms of the preceding type, and remarks on the desirability of keeping clear on "the essential connection that links explaining to questions." (1962, 100)

Statement (3) of Bromberger's initial set, it will be recalled, is "Newton explained a long time ago the variations of the tides." This statement does not imply an explaining episode of the sort we have been discussing. Here "explained" is not an accomplishment term; it is, rather, an *achievement* term. The statement means that Newton solved the problem. His solution of the problem is such that, for anyone who understands it, that person can provide an explanation of the sort we previously discussed. "The connection, then, between the truth-conditions of 'A E W' and 'A E to B W' is that to have explained something in the sense now under consideration is to have become able to explain something in the [previous] sense . . . as a result of one's own endeavours and ingenuity." (1962, 101)[5]

Bromberger's first example of a statement involving "to explain" – the one about the ability of the kinetic theory to explain why certain vapors do not behave as ideal gases – has the form "T explains W," where "T" does not indicate a place for a term referring to a person or group of people. In this sentence, "to explain" functions as a state term; it refers to the continuing ability of a theory to function in explanations of the sort we have already discussed. As I remarked at the outset, Bromberger does not claim to have an adequate treatment of this kind of explanation.

I have discussed Bromberger's approach in considerable detail and I have presented lengthy direct quotations to give a clear flavor of what he is attempting, and to contrast it with the sort of thing Hempel and Oppenheim were trying to do. Clearly, Bromberger has great sensitivity to the nuances of language. He goes to considerable length to understand the usages of "to explain" – for instance, to discover the differences between explanation-seeking questions and other kinds of questions. In his principal examples the explanation-seeking question was a why-question, but he denies that all and only why-questions are explanation-seeking. His identification of p-predicaments and b-predicaments involves deep insight into the nature of questions. It also exhibits clear appreciation of many of the pragmatic aspects of explanation.

In the end, however, as Bromberger is clearly aware, his characterization of explanation employs the unanalyzed notion of a right answer to a why-question or any other kind of explanation-seeking question. As I said above, what Hempel and Oppenheim were trying to do is to analyze the notion of a correct answer to an explanation-seeking question. Although they undoubtedly made significant progress on their enterprise, they offered no criteria at all to enable us to distinguish explanation-seeking from non-explanation-seeking questions. Bromberger's paper, which he aptly titled "An Approach to Explanation" (rather than, say, "A Theory of Explanation") provides a valuable prolegomenon to the Hempel-Oppenheim project, but, as he recognized, not a substitute for it.

Toward the end of the second decade, Bromberger (1966) returned to this issue and attempted to give a partial answer. Although he still insists that not every request for an explanation can be given in the form of a why-question, he does try to characterize correct answers to explanation-seeking why-questions. He offers several counterexamples – similar in import to the flagpole (CE-2, to be discussed in the next section) – to show that not every argument that satisfies the criteria for D-N explanation, as set out by Hempel and Oppenheim, qualifies as an acceptable answer to a particular why-question. However, he does not dispute the thesis that these conditions are necessary for satisfactory answers – for satisfactory *deductive* answers, we should say, for he does not deny that inductive or statistical explanations are possible. The theory of why-questions can thus be seen as a friendly amendment to the Hempel-Oppenheim account.[6] He does not, however, discuss any of the issues surrounding the theories of statistical explanation that were published by Hempel shortly before the appearance of this article by Bromberger.

As his point of departure, Bromberger rejects the covering law conception that identifies explanation, or correct answers to why-questions, with subsumption under true lawful generalizations. Consider the question, "Why does this live oak keep its leaves during the winter?" The appropriate response, he suggests, is "All live oaks do!" "Because all live oaks do" is, he claims, not correct. Since he maintains that answers to why-questions are sentences that follow "because," the correct response is not an answer, but a rejection of the question (1966, 102). The question suggests that there is something atypical about this particular tree; the response denies that supposition.

Bromberger's basic idea is that why-questions arise "when one believes that the presupposition is true, views it as a departure from a general rule, and thinks that the conditions under which departures from the general rule occur can be generalized" (1966, 100). To develop this thesis we need several definitions.

(1) A *general rule* is simply a *lawlike* generalization of the form

$$(x)[(F_1x \,.\, F_2x \;.\;.\;.\; F_jx) \supset (S_1x \,.\, S_2x \;.\;.\;.\; S_kx)] \qquad (j, k \geq 1)$$

Examples are: All gold is malleable, no rubber bands are brittle, and the velocity of an object never changes. Obviously, a general rule need not be true or even plausible (1966, 97).

(2) Bromberger introduces the concept of a *general abnormic law*, but the definition is quite complex, and we do not need all of the details for our discussion. He remarks that every general abnormic law is equivalent to a conjunction of spe-

cial abnormic laws. It should, however, be emphatically noted that general abnormic laws are true (1966, 97–99).

(3) A *special abnormic law* is a true lawlike generalization that contains an unless-clause. Examples are: The velocity of an object does not change unless the net force on it is not equal to zero; no rubber band is brittle unless it is very old or very cold. Special abnormic laws have the following form:

$$(x)\{(F_1x \,.\, F_2x \,.\,.\,.\, F_jx) \supset [-Ex \equiv (A_1x\ A_2x \,.\,.\,.\, A_nx)]\} \quad (j, n \geq 1)$$

which can obviously be reformulated as

$$(x)\{(F_1x \,.\, F_2x \,.\,.\,.\, F_jx) \supset [Ex \equiv -(A_1x\ A_2x \,.\,.\,.\, A_nx)]\} \quad (j, n \geq 1)$$

They must also fulfill further restrictions that need not be spelled out for our purposes.[7]

(4) Bromberger defines the *antonymic predicates of an abnormic law,* but we need only the concept of *antonymic predicates of a special abnormic law.* These are the predicates that occur in the place of "E" and its negation in the foregoing forms. For example, "brittle" and "nonbrittle" would be the antonymic predicates in "No rubber bands are brittle unless they are old or cold" (1966, 99).

(5) Finally, we need the concept of the *completion of a general rule by an abnormic law.* An abnormic law is the completion of a general rule if and only if the general rule is false and is obtainable from the abnormic law by dropping the "unless"-clause (1966, 99).

With this terminology in place we can now characterize the concept of a correct answer to a why-question:

> Here then is the relation: *b* is the *correct answer* to the question whose presupposition is *a* if and only if (1) there is an abnormic law *L* (general or special) and *a* is an instantiation of one of *L* 's antonymic predicates; (2) *b* is a member of a set of premises that together with *L* constitute a deductive nomological explanation whose conclusion is *a* ; (3) the remaining premises together with the general rule completed by *L* constitute a deduction in every respect like a deductive nomological explanation except for a false lawlike premise and false conclusion, whose conclusion is a contrary [or contradictory] of *a*. (1966, 100)[8]

Consider one of Bromberger's examples. Suppose we have a simple pendulum that is oscillating with a particular period p. It consists of a bob suspended by a string of a given length. From the law of the simple pendulum, the acceleration of gravity at the earth's surface g, and the length l of the string, one can deduce the period p of our pendulum. This argument would qualify as a deductive nomological explanation according to the Hempel-Oppenheim account, and it would be regarded as a correct explanation of the period. However, from the same law, the same value for the acceleration of gravity, and the period of the pendulum we can deduce the length of the string. This would not seem to constitute a good answer to the question of why the string has its particular length l.

Consider the general rule, "No simple pendulum oscillates with period p." It is, of course, false. It can, however, be completed by the abnormic law, "No simple pendulum has the period p unless[9] it has a suspension of length l." From the abnormic law and the fact that the suspension has length l, we can deduce that it has period p. From the general rule and the fact that we have a simple pendulum we can deduce that it is not oscillating with period p. Thus, it is correct, according to Bromberger's account, to answer the why-question about the period of oscillation with facts about the length of the suspension and the local acceleration of gravity, and the law of the simple pendulum. However, to answer the question, "Why does this pendulum have a suspension of length l?" it would be manifestly incorrect to answer, "Because it is oscillating with period p." For consider the putative abnormic law, "No pendulum has a suspension of length l unless it is oscillating with period p." This is patently false, for a pendulum that is not oscillating may have just such a suspension. Bromberger thus attempts to solve a nagging problem about asymmetries of explanation.

Bromberger closes this essay with the following remark:

> It may seem odd that abnormic laws should be associated with a special interrogative. But they are, after all, the form in which many common-sense generalizations that have been qualified through the ages are put. They are also a form of law appropriate to stages of exploratory theoretical developments when general rules are tried, then amended, until finally completely replaced. We are always at such a stage. (1966, 107)

What Bromberger has noticed, I believe, is a deep fact about scientific explanations. To provide an adequate explanation of any given fact, we need to provide information that is relevant to the occurrence of that fact – information that *makes a difference* to its occurrence. It is not sufficient simply to subsume an occurrence under a general law; it is necessary to show that it has some special characteristics that account for the features we seek to explain. As we shall see, this notion of explanatory relevance plays a key role in the development of theories of statistical explanation.

As I have already remarked, and as we shall see in greater detail, the formulation of theories of inductive or statistical explanation was one of the most significant – perhaps the most significant – development in the second decade. Being confined to deductive types of explanation, Bromberger's treatment did not encompass this broader range of issues. Moreover, as an incisive critique by Paul Teller (1974) showed, it does not even achieve its aim within its intended range of application. Nevertheless, it provided a fertile basis for subsequent work on the pragmatics of explanation, especially that of Achinstein and van Fraassen during the fourth decade. In addition, it has served as a major stimulus to Philip Kitcher in developing the novel unification approach to explanation embodied in "Explanatory Unification and the Causal Structure of the World" in this volume (Kitcher & Salmon 1989) especially (Bromberger 1963).

2.3 Famous Counterexamples to the Deductive-Nomological Model

At the conclusion of §1.1 we raised a number of general issues arising out of the Hempel-Oppenheim treatment of scientific explanation of particular occurrences. During the second decade many of them were vividly posed in terms of counterexamples that have since become quite standard. The existence of such a standard set is, in itself, a tribute to the solidity of the received view. Let us take a look at some of the best known.

One rather obvious problem has to do with the temporal relations between the explanatory facts (as expressed by the singular sentences in the explanans) and the fact-to-be-explained (as expressed by the explanandum-sentence). In the schema reproduced above (p. 13; H-O 1948, 249) the Cs are labeled as "antecedent conditions," but in the formal explication no temporal constraints are given. Indeed, no such temporal constraints are mentioned even in the informal conditions of adequacy. This issue has been posed in terms of the explanation of an eclipse.

(CE-1) The eclipse. Going along with the D-N model, we might, for example, regard a total lunar eclipse as satisfactorily explained by deducing its occurrence from the relative positions of the earth, sun, and moon at some prior time in conjunction with the laws of celestial mechanics that govern their motions. It is equally possible, however, to deduce the occurrence of the eclipse from the relative positions of the earth, sun, and moon at some time after the eclipse in conjunction with the very same laws. Yet, hardly anyone would admit that the latter deduction qualifies as an explanation.[10] One might suppose that the failure to impose temporal restrictions was merely an oversight that could be corrected later, but Hempel (1965a, 353) raises this question explicitly and declines to add any temporal constraint.[11]

Another issue, closely related to the matter of temporal priority, has to do with the role of causality in scientific explanation. Our commonsense notion of explanation seems to take it for granted that to explain some particular event is to identify its cause and, possibly, point out the causal connection. Hempel and Oppenheim seem to share this intuition, for they remark, "The type of explanation which has been considered here so far is often referred to as causal explanation" (H-O 1948, 250). In "Aspects of Scientific Explanation," while admitting that some D-N explanations are causal, Hempel explicitly denies that all are (1965a, 352–54).

The problems that arise in this connection can readily be seen by considering several additional well-known examples.

(CE-2). Bromberger's flagpole example. A vertical flagpole of a certain height stands on a flat level piece ground.[12] The sun is at a certain elevation and is shining brightly. The flagpole casts a shadow of a certain length. Given the foregoing facts about the height of the flagpole and the position of the sun, along with the law of rectilinear propagation of light, we can deduce the length of the shadow. This deduction may be accepted as a legitimate D-N explanation of the length of the shadow. Similarly, given the foregoing facts about the position of the sun and the length of the shadow, we can invoke the same law to deduce the height of the flagpole. Nevertheless, few people would be willing to concede that the height of the flagpole is explained by the length of its shadow.[13] The reason for this asymmetry seems to lie in the fact that a flagpole of a certain height causes a shadow of a given length, and thereby explains the length of the shadow, whereas the shadow does not cause the flagpole, and consequently cannot explain its height.

(CE-3) The barometer. If a sharp drop in the reading on a properly functioning barometer occurs, we can infer that there will be a storm – for the sake of argument, let us assume that there is a law that whenever the barometric pressure drops sharply a storm will occur. Nevertheless, we do not want to say that the barometric reading explains the storm, since both the drop in barometric reading and the occurrence of the storm are caused by atmospheric conditions in that region. When two different occurrences are effects of a common cause, we do not allow that either one of the effects explains the other. However, the explanation of the storm on the basis of the barometric reading fits the D-N model.

(CE-4) The moon and the tides. Long before the time of Newton, mariners were fully aware of the correlation between the position and phase of the moon and the rising and falling of the tides. They had no knowledge of the causal connection between the moon and the tides, so they had no explanation for the rising and falling of the tides, and they made no claim to any scientific explanation. To whatever extent they thought they had an explanation, it was probably that God in his goodness put the moon in the sky as a sign for the benefit of mariners. Nevertheless, given the strict law correlating the position and phase of the moon with the ebb and flow of the tides,[14] it was obviously within their power to construct D-N explanations of the behavior of the tides. It was not until Newton furnished the causal connection, however, that the tides could actually be explained.

One of the most controversial theses propounded by Hempel and Oppenheim is *the symmetry of explanation and prediction*. According to this view, the very same logical schema fits scientific explanation and scientific prediction; the sole difference between them is pragmatic. If the event described by E has already occurred, we may ask why. A D-N explanation consisting of a derivation of E from laws and antecedent conditions provides a suitable response. If, however, we are in possession of the same laws and antecedent conditions before the occurrence of E, then that same argument provides a prediction of E. Any D-N explanation is an argument that, were we in possession of it early enough, would enable us to anticipate, on a sound scientific basis, the occurrence of E. Since every D-N explanation involves laws, a hallmark of explanations of this type is that they provide *nomic expectability.* [15]

In discussing the symmetry of explanation and prediction in the preceding paragraph, I was tacitly assuming that the so-called antecedent conditions in the explanans are, in fact, earlier than the explanandum event. However, in view of Hempel's rejection of any requirement of temporal priority, the symmetry thesis must be construed a bit more broadly. Suppose, for example, that the explanandum-event E occurs before the conditions C in the explanans. Then, as I construe the symmetry thesis, we would be committed to the view that the D-N explanation is an argument that could be used subsequent to the occurrence of the explanatory conditions C to retrodict E. It is quite possible, of course, that E has occurred, but that we are ignorant of that fact. With knowledge of the appropriate laws, our subsequent knowledge of conditions C would enable us to learn that E did, in fact, obtain. Parallel remarks could be made about the case in which C and E are simultaneous. Thus, in its full generality, the symmetry thesis should be interpreted in such a way that "prediction" is construed as "inference from the known to the unknown."[16]

As Hempel later pointed out in "Aspects of Scientific Explanation," the symmetry thesis can be separated into two parts: (i) Every D-N explanation is a prediction—in the sense explained in the preceding paragraph—and (ii) every (nonstatistical) scientific prediction is a D-N explanation. It is worthwhile, I think, to distinguish a *narrower symmetry thesis*, which applies only to D-N explanations of particular facts, and a *broader symmetry thesis*, which applies to both D-N and I-S explanations of particular facts. According to the narrower thesis, every *nonstatistical* prediction is a D-N explanation; according to the broader thesis, every prediction is an explanation of either the D-N or I-S variety. Given the fact that statistical explanation is not explicated in the Hempel-Oppenheim article, only the narrower symmetry thesis is asserted there. The broader thesis, as we shall see, was advocated (with certain limitations) in "Aspects."

Nevertheless, various critics of the Hempel-Oppenheim article failed to take sufficient notice of the explicit assertion that not all legitimate scientific explanations are D-N—that some are statistical. Scriven (1959) strongly attacked subthesis (i)—that all explanations could serve as predictions under suitable pragmatic circumstances—by citing evolutionary biology and asserting that it furnishes explanations (of what has evolved) but not predictions (of what will evolve). If, as I believe, evolutionary biology is a statistical theory, then Scriven's argument applies at best to the broader, not the narrower symmetry thesis. Although this argument was published in 1959,[17] it does, I believe, pose a serious problem for the theory of statistical explanation Hempel published three years later. In the same article Scriven set forth a widely cited counterexample:

(CE-5) Syphilis and paresis. Paresis is one form of tertiary syphilis, and it can occur only in individuals who go through the primary, secondary, and latent stages of the disease without treatment with penicillin. If a subject falls victim to paresis, the explanation is that it was due to latent untreated syphilis. However, only a relatively small percentage—about 25%—of victims of latent untreated syphilis develop paresis. Hence, if a person has latent untreated syphilis, the correct prediction is that he or she will not develop paresis. This counterexample, like the argument from evolutionary biology, applies only to the broader symmetry thesis.

When the narrower symmetry thesis is spelled out carefully, it seems impossible to provide a counterexample for subthesis (i)—that every explanation is a prediction (given the right pragmatic situation). That subthesis amounts only to the assertion that the conclusion of a D-N argument follows from its premises. Against subthesis (i) of the broader symmetry thesis the syphilis/paresis counterexample is, I think, quite telling.

When we turn to subthesis (ii) of the narrower symmetry thesis—i.e., that every (nonstatistical) prediction is an explanation—the situation is quite different. Here (CE-3) and (CE-4) provide important counterexamples. From the barometric reading, the storm can be predicted, but the barometric reading does not explain the storm. From the position and phase of the moon, pre-Newtonians could predict the behavior of the tides, but they had no explanation of them. Various kinds of correlations exist that provide excellent bases for prediction, but because no suitable causal relations exist (or are known), these correlations do not furnish explanations.

There is another basis for doubting that every scientific prediction can serve, in appropriate pragmatic circumstances, as an explanation. Hempel and Oppenheim insist strongly upon the covering law character of explanations. However, it seems plausible to suppose that some respectable scientific predictions can be made without benefit of laws—i.e., some predictions are inferences from particular instances to particular instances. Suppose, for instance, that I have tried quite a number of figs from a particular tree, and have found each of them tasteless. A friend picks a fig from this tree and is about to eat it. I warn the friend, "Don't eat it; it will be tasteless." This is, to be sure, low-level science, but I do not consider it an unscientific prediction. Moreover, I do not think any genuine laws are involved in the prediction. In (1965, 376) Hempel considers the acceptability of subthesis (ii) of the symmetry thesis an open question.

There is another fundamental difficulty with Hempel and Oppenheim's explication of D-N explanation; this one has to do with explanatory relevance. It can be illustrated by a few well-known counterexamples.

(CE-6) The hexed salt. A sample of table salt has been placed in water and it has dissolved. Why? Because a person wearing a funny hat mumbled some nonsense syllables and waved a wand over it—i.e., cast a dissolving spell upon it. The explanation offered for the fact that it dissolved is that it was hexed, and all hexed samples of table salt dissolve when placed in water. In this example it is *not* being supposed that any actual magic occurs. All hexed table salt is water-soluble because all table salt is water-soluble. This example fulfills the requirements for D-N explanation, but it manifestly fails to be a bona fide explanation.[18]

(CE-7) Birth-control pills. John Jones (a male) has not become pregnant during the past year because he has faithfully consumed his wife's birth-control pills, and any male who regularly takes oral contraceptives will avoid becoming pregnant. Like (CE-6), this example conforms to the requirements for D-N explanation.

The problem of relevance illustrated by (CE-6) and (CE-7) is actually more acute in the realm of statistical explanation than it is in connection with D-N explanation. Insofar as D-N explanation is concerned, it is possible to block examples of the sort just considered by any of several technical devices.[19] We will return to this issue when we discuss statistical explanation.

2.4 Statistical Explanation

In an article entitled "The Stochastic Revolution and the Nature of Scientific Explanation," Nicholas Rescher (1962) made an eloquent plea for an extension of the concept of scientific explanation beyond the limits of deductive explanation. The "stochastic revolution" yields "forcible considerations . . . that militate towards a view of explanation prepared to recognize as an 'explanation' of some fact an argument which provides a rationalization of this fact from premises which render it *not necessary but merely probable*" (ibid., 200). He adds, "To refuse to accord to such explanatory reasonings the title of 'explanation' is to set up so narrow a concept of explanation that many of the reasonings ordinarily so-called in modern scientific discussions are put outside the pale of *explanations proper* by what is in the final analysis, a fiat of definition buttressed solely by fond memories of what explanation used to be in nineteenth-century physics" (ibid. 204).

The most important development to occur in the second decade (1958–67) of our chronicle—the explicit treatment of statistical explanation—had its public inception in 1962. Although Rescher clearly recognized the inductive character of such explanations, neither he nor any of several other authors who recognized the legitimacy of statistical explanation offered an explicit model. They thought, I suspect, that nothing was needed beyond a trivial relaxation of the requirements imposed on D-N explanations. Hempel appears to have been the first to notice the profound difficulties involved in statistical explanation, in particular, the problem of *ambiguity*, over which he labored long and hard. The first explicit model was offered in his "Deductive-Nomological vs. Statistical Explanation" (1962) in volume III of *Minnesota Studies*. It constitutes the first serious attempt (of which I am aware) by any philosopher or scientist to offer a detailed and systematic account of any pattern of statistical explanation.[20] In this paper, Hempel deals with statistical explanations of particular facts—the lower left-hand box of Table 1—and characterizes what he later calls the *inductive-statistical (I-S)* model. In the same year (1962a) he offered a brief popular account of both deductive and inductive explanations of particular facts in history as well as in science. In section 3 of "Aspects," he offers a much improved account of the I-S model, and also introduces the *deductive-statistical (D-S)* model.

2.4.1 The Deductive-Statistical Model

In a D-S explanation, a statistical law[21] is explained by deriving it from other laws, at least one of which is statistical. There is no prohibition against the explanans containing universal laws as well. A time-honored example will illustrate how this sort of explanation goes.

In the 17th century the Chevalier de Méré posed the following question: Why is it that, when a player tosses a pair of standard dice in the standard manner 24 times, he has less than a 50–50 chance of getting double 6 at least once? The answer is as follows. On each toss of a fair die, the chance of getting 6 is 1/6. This probability is independent of the outcome of the toss of the other die. Consequently, the probability of double 6 on any throw is 1/36, and the probability of not getting double 6 is 35/36. The outcome of any toss of the pair of dice is independent of the outcomes of previous tosses; hence, the probability of not getting double 6 on n throws is $(35/36)^n$. When we calculate the value of $(35/36)^{24}$ we find it is greater than 1/2; therefore, the probability of getting double 6 in 24 tosses is less than 1/2. Assuming that there is a sharp distinction between mathematics and empirical science, we can say that the empirical statistical laws in this derivation are the generalizations about the behavior of standard dice: the probability of 6 is 1/6 and the tosses are independent of one another. The rest is simply arithmetic. The "laws of probability"—such as the multiplication rule—are not empirical laws at all; they are laws of mathematics which have no factual content.

Another example of a D-S explanation would be the derivation of the half-life of uranium-238 from the basic laws of quantum mechanics—which are statistical—and from the height of the potential barrier surrounding the nucleus and the kinetic energies of alpha particles within the nucleus. The answer is approximately 4.5 x 10^9 years. This example is highly theoretical, and consequently distinctly *not* empirical in the narrow sense of "empirical."

As we have seen, the Hempel-Oppenheim article attempted to characterize laws of nature. Because of the very simple formal language employed in that context, there is no possibility of formulating statistical laws within it. The reason, very simply, is that the language does not contain any numerical expressions. Whatever the philosophical merits of using such a language may be, clearly a much richer language is needed to express any important scientific content.[22] Adopting a language that would begin to be adequate for any real science would involve using one in which statistical laws could be formulated. So any language that could contain real universal scientific laws could contain statistical laws as well.

The statistical laws of empirical science are general in the same sense that universal laws are general. The simplest universal law would have the form, "All F are G." It would be formulated using a variable that ranges over all individuals in the universe. The force of the generalization is that, in the entire universe, nothing is an F and not a G or, equivalently, everything is either not an F or else it is a G. The corresponding negative generalization would mean that, in the entire universe, nothing is both F and G. The existential generalization, "Some F are G," would mean that, in the entire universe, at least one thing is an F and also a G. The simplest statistical law could be construed in either of two ways. First, it might be taken to mean, "Every F has a certain propensity to be G," in which case it is strictly analogous to a universal generalization. Second, it might be taken to mean, "A certain proportion of F are G." In that case we can construe it to mean that, among all of the individuals in the universe that are F, a certain proportion are also G. Either way it has the same sort of generality as universal or existential generalizations.[23]

In addition, the same problem about purely qualitative predicates obviously arises in connection with both universal and statistical laws. The same point about not restricting generality by making reference to particular objects, places, or times also applies to statistical as well as to universal laws. And the problem of distinguishing between lawful and accidental statistical generalizations is the same as for universal generalizations. Recalling that the official Hempel-Oppenheim explication of D-N explanation appealed to theories rather than laws, we can say that generalizations involving universal quantifiers, existential quantifiers, mixtures of the two kinds, or probabilities are on a par.

We should also remember that Hempel and Oppenheim confessed their inability to provide an explication for D-N explanation of laws because of the difficulty stated in "the notorious footnote 33." The problem was to make an appropriate distinction between really explaining a law by deducing it from a genuinely more general law and giving a pseudo-explanation by some such device as deducing it from a 'law' that consists of a conjunction of which it is one conjunct. This problem is clearly just as acute for statistical laws as for nonstatistical laws and theories. For reasons of these sorts, I suggested at the outset that we not regard D-S explanations as belonging to a type different from D-N, but treat them rather as a subtype of D-N. The right-hand column of Table 1 can be regarded as representing one kind of explanation, namely, D-N explanation of generalizations. Nowhere in "Aspects" does Hempel offer an answer to the problem stated in the notorious footnote.

2.4.2 The Inductive-Statistical Model

Hempel's main concern in section 3 of "Aspects" is clearly with I-S explanation. As he says, "Ultimately, however, statistical laws are meant to be applied to particular occurrences and to establish explanatory and predictive connections among them" (1965, 381).[24] So let us turn to his explication of that model.

There is a natural way to try to extend the treatment of D-N explanation of particular facts to cover statistical explanation of particular facts. Given the explanation/prediction symmetry thesis, we can say that a D-N explanation of a given fact is a deductive argument showing that the event in question was predictable had the explanatory facts been available early enough. We can then suppose that an I-S explanation is also an argument that would render the explanandum predictable had the explanatory facts been available early enough. In the case of I-S explanation, the explanans must include, essentially, at least one statistical law; as a result, it is impossible to *deduce* the explanandum statement from the explanans. Hempel therefore requires the I-S explanation to be an *inductive* argument that would render the explanandum predictable, not with deductive certainty but with high inductive probability, given the explanans. The simplest kind of example would fit the following schema:

$$\text{I-S} \quad \begin{array}{l} P(G|F) = r \\ Fb \\ \hline\hline Gb \end{array} \quad [r]$$

The first premise of this argument is a statistical law that asserts that the relative frequency of Gs among Fs is r,[25] where r is fairly close to 1. The double line separating the premises from the conclusion signifies that the argument is inductive rather than deductive. The expression "[r]" next to the double line represents the degree of inductive probability conferred on the conclusion by the premises. Note that I-S explanations are covering law explanations in exactly the same sense as D-N explanations are – namely, each such explanation must contain at least one law in its explanans.

Perhaps a few examples would be helpful. Why, for instance, did Yamamoto suffer severe physical injury in August of 1945? Because he was only a kilometer from the epicenter of the atomic blast in Hiroshima, and any person that distance from the epicenter of an atomic explosion of that magnitude will very probably suffer severe physical injury.

Why, for another example, is the ratio of carbon-14 to other isotopes of carbon in this particular piece of charcoal about half of the ratio of C^{14} to other isotopes in the atmosphere? Because the piece of wood from which this piece of charcoal came was cut about 5730 years ago, and the half-life of C^{14} is 5730 years. The proportion of C^{14} in the atmosphere remains fairly constant, because it is replenished by cosmic radiation at about the same rate as it decays radioactively. While a tree is living it continues absorbing C^{14} from the atmosphere, but when the tree dies or is cut it no longer does so, and the C^{14} content decreases as a result of spontaneous radioactive decay. This example qualifies as I-S rather than D-N for two reasons. First, the explanandum is a particular fact, namely the C^{14} content of one particular piece of charcoal. The fact that many atoms are involved does not make the explanandum a statistical law. Second, the law governing radioactive decay is a statistical law. From this law we can conclude that it is highly probable, but not absolutely certain, that about 1/2 of any reasonably numerous collection of atoms of a given unstable isotope will decay in the period of time constituting the half-life of that particular isotope.

Hempel's main example of I-S explanation is the case of John Jones who recovered quickly from a streptococcus infection. When we ask why we are told that penicillin was administered, and that most (but not all) strep infections clear up quickly when treated with penicillin. If we supply a definite number r for the probability of quick recovery from a strep infection, given that penicillin is administered, this example is easily seen to fit the I-S schema set out above, as follows:

(1) $P(G|F.H) = r$
$Fb.Hb$
$\overline{\overline{\qquad\qquad}}$ [r]
Gb

where F stands for having a strep infection, H for administration of penicillin, G for quick recovery, b is John Jones, and r is a number close to 1.

This example can be used to illustrate a basic difficulty with I-S explanation. It is known that certain strains of streptococcus bacilli are resistant to penicillin. If it turns out that John Jones is infected with a penicillin-resistant strain, then the probability of his quick recovery after treatment of penicillin is low. In that case, we could set up the following inductive argument:

(2) $P(G|F.H.J) = r_1$
$Fb.Hb.Jb$
$\overline{\overline{\qquad\qquad}}$ $[r_1]$
Gb

where J stands for the penicillin-resistant character of the strep infection and r_1 is a number close to zero.

This case exemplifies what Hempel calls *the ambiguity of I-S explanation*. We have two inductive arguments; the premises of these arguments are all logically compatible—all of them could be true. The conclusions of these two arguments are identical. Nevertheless, in one argument the conclusion is strongly supported by the premises, whereas in the other the premises strongly undermine the same conclusion—indeed, argument (2) can readily be transformed into an argument that strongly supports the negation of the conclusion of argument (1):

$$\text{(3)}\quad \begin{array}{l} P({\sim}G \mid F.H.J) = 1 - r_1 \\ Fb.Hb.Jb \\ \hline\hline {\sim}Gb \end{array} \quad [1 - r_1]$$

Given that r_1 is close to zero, $1-r_1$ must be close to 1. Hence, we have two strong inductive arguments with compatible premises whose conclusions contradict one another. This situation is inconceivable in deductive logic. If two valid deductive arguments have incompatible conclusions, the premises of one argument must contradict the premises of the other.

At this point we are confronting one of the most fundamental differences between deductive and inductive logic. Deductive logic has a *weakening principle*, according to which $p \supset q$ entails $p.t \supset q$, for any arbitrary t whatever. From this it follows that, given a valid deductive argument, it will remain valid if additional premises are inserted, provided none of the original premises is deleted. Probability theory does not have any such weakening principle. Let $P(G \mid F)$ be as close to 1 as you like, $P(G \mid F.H)$ may be arbitrarily close to 0.[26] From this it follows that an inductive argument that strongly supports its conclusion may be transformed, by the addition of a new premise consistent with the original premises, into an argument that strongly undermines that conclusion. Before the discovery of Australia, for example, Europeans had strong inductive evidence, based on many observations of swans, for the conclusion "All swans are white." Nevertheless, the addition of one premise reporting the observation of a black swan in Australia not only undermines the inductive conclusion, it deductively refutes it.

Inductive logicians have long recognized this feature of inductive arguments, and have come to terms with it by means of *the requirement of total evidence*. According to this principle, an inductive argument strongly supports its conclusion only if (i) it has true premises, (ii) it has correct inductive form, and (iii) no additional evidence that would change the degree of support is available at the time. Argument (1) above clearly fails to satisfy the requirement of total evidence, for its set of premises can be supplemented as follows:

(4) $P(G|F.H) = r$
$P(G|F.H.J) = r_1$
$Fb.Hb$
Jb
═══════════ $[r_1]$
Gb

where r_1 is very different from r.

The first temptation, with regard to the ambiguity of I-S explanation, might be to impose the requirement of total evidence, and to say that any inductive argument that is to qualify as an explanation must satisfy that requirement.[27] Such a move would be disastrous, for normally when we try to explain some fact we already know that it is a fact. Hence, our body of knowledge includes the conclusion of the argument. If the conclusion is not included among the premises, the requirement of total evidence is violated. If the conclusion is included among the premises, the argument is not inductive; rather, it is a trivially valid deduction—one, incidentally, that cannot even qualify as a D-N explanation, because, if the explanandum is included in the explanans, no law statement can occur *essentially* in the explanans. Consequently, in order to deal with the ambiguity of I-S explanation, Hempel sought a weaker counterpart for the requirement of total evidence that would not rule out altogether the possibility of I-S explanations.

The requirement Hempel saw fit to impose he called *the requirement of maximal specificity (RMS)*. Suppose, referring back to the schema (I-S) given above, that s is the conjunction of all of the premises and k is the body of knowledge at the time in question. "Then," Hempel says, "to be rationally acceptable in [that] knowledge situation . . . the proposed explanation . . . must meet the following condition:

> 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 also imply a statement specifying the statistical probability of G in F_1, say
>
> $P(G|F_1) = r_1$
>
> Here, r_1 must equal r unless the probability statement just cited is simply a theorem of mathematical probability theory. (1965, 400)[28]

The unless-clause in the final sentence is intended to guard against RMS being so strong that it would rule out the possibility of I-S explanation altogether. If we want to explain Gb, then presumably we know that b belongs to the class G, and if we want to use $P(G|F) = r$ as a statistical law, we know that b belongs to F; consequently, k includes the statement that b is a member of $F_1 = F.G$, which is a subclass of F. But, trivially, $P(G|F.G) = 1$; indeed, trivially, all F.Gs are Gs. But this is not an appropriate basis for condemning the original explanation.

Since, Hempel claims, all bona fide I-S explanations must satisfy RMS, and since RMS makes specific reference to a particular knowledge situation, "*the concept of statistical explanation for particular events is essentially relative to a given knowledge situation as represented by a class K of accepted statements* " (1965, 402). Hempel refers to this feature as the *epistemic relativity of statistical explanation*. This relativity has no counterpart in D-N explanation. The reason is that the requirement of maximal specificity is automatically fulfilled in the case of D-N explanation, for, given that all F are G, it follows immediately that all F_1 are G if F_1 is a subclass of F. This is just an application of the weakening principle that was cited above.

We must guard against one easy misunderstanding. Someone might claim that D-N explanations are relativized to knowledge situations because what we take to be a law depends upon what we know at any given time. It is true, of course, that what is considered a law at one time may be rejected at another, and that we can never know for certain whether a given general statement is true. At best, we can hope to have general statements that are highly confirmed and that we are justified in accepting. These considerations apply equally to universal laws, general theories, and statistical laws, and consequently they apply equally to D-N and I-S explanations. The epistemic relativity of I-S explanation refers, however, to something entirely different.

Suppose that we have two putative explanations of two different particular facts, one a D-N explanation, the other I-S. Suppose that each of them has correct logical form—deductive and inductive, respectively. Suppose further that we are prepared, on the basis of our knowledge at the time, to accept the premises of each argument as true. Then, according to Hempel, we are entitled to accept the D-N explanation as correct (recognizing at the same time that we may be mistaken). We are not, however, entitled to accept the I-S explanation as correct, on the grounds just mentioned; in addition, we have to determine whether the statistical law to which we appeal is maximally specific. Whether it is or not depends upon the content of our body of knowledge. A statistical law can be true without being maximally specific. That is why, according to Hempel, we need the requirement of maximal specificity.

With RMS in place, Hempel has provided us with two models of scientific explanation of particular facts, one deductive and one inductive. A comprehensive characterization can be given. Any explanation of a particular occurrence is an argument to the effect that the event-to-be-explained was to be expected by virtue of certain explanatory facts. The explanatory facts must include at least one general law. The essence of scientific explanation can thus be described as *nomic expectability* —that is, expectability on the basis of lawful connections (1962a).

The general conditions of adequacy for scientific explanations set out in the first section of the Hempel-Oppenheim paper can be revised to encompass statistical explanation in the following way:

Logical conditions

1. An explanation is an argument having correct logical form (either deductive or inductive).
2. The explanans must contain, essentially, at least one general law (either universal or statistical).[29]
3. The general law must have empirical content.

Hempel and Oppenheim admit that requirement 3 is vacuous, for it will be automatically satisfied by any putative explanation that satisfies conditions 1 and 2. Any putative explanation that satisfies these conditions qualifies as a potential explanation. In order to qualify as an actual explanation (or, simply, an explanation), a potential explanation must fulfill two more conditions.

Empirical condition:

4. The statements in the explanans must be true.

Relevance condition:

5. The requirement of maximal specificity.

As we have seen, this relevance requirement is automatically satisfied by D-N explanations.

At this juncture, the received view embraced two models of explanation of particular facts, and a large promissory note about D-N explanations of laws (including, of course, statistical laws). The first serious effort to pay off the debt was made by Michael Friedman in 1974 – well into the third decade[30] – and he did not attempt to deal with statistical laws. We shall examine his work in due course, noting for the moment that, even if he succeeds in his effort, the promise of the lower right corner of Table 1 remains unfulfilled.[31]

2.5 Early Objections to the Inductive-Statistical Model

My own particular break with the received view occurred shortly after the incident with Smart that I related at the beginning of this essay. In a paper (W. Salmon 1965) presented at the 1963 meeting of the American Association for the Advancement of Science, Section L, organized by Adolf Grünbaum, I argued that Hempel's I-S model (as formulated in his (1962)), with its high probability requirement and its demand for expectability, is fundamentally mistaken. Hempel's example of John Jones's rapid recovery from his strep infection immediately called to mind such issues as the alleged efficacy of vitamin C in preventing, shortening, or mitigating the severity of common colds,[32] and the alleged efficacy of various types of psychotherapy. I offered the following examples:

(CE-8) John Jones was almost certain to recover from his cold within a week because he took vitamin C, and almost all colds clear up within a week after administration of vitamin C.

(CE-9) John Jones experienced significant remission of his neurotic symptoms because he underwent psychotherapy, and a sizable percentage of people who undergo psychotherapy experience significant remission of neurotic symptoms.

Because almost all colds clear up within a week whether or not the patient takes vitamin C, I suggested, the first example is not a bona fide explanation. Because many sorts of psychological problems have fairly large spontaneous remission rates, I called into question the legitimacy of the explanation proffered in the second example. What is crucial for statistical explanation, I claimed, is not how probable the explanans renders the explanandum, but rather, whether the facts cited in the explanans *make a difference* to the probability of the explanandum.

To test the efficacy of any sort of therapy, physical or psychological, controlled experiments are required. By comparing the outcomes in an experimental group (the members of which receive the treatment in question) with those of a control group (the members of which do not receive that treatment), we procure evidence concerning the effectiveness of the treatment. This determines whether we are justified in claiming explanatory import for the treatment vis-à-vis the remission of the disease. If, for example, the rate of remission of a certain type of neurotic symptom during or shortly after psychotherapy is high, but no higher than the spontaneous remission rate, it would be illegitimate to cite the treatment as the explanation (or even part of the explanation) of the disappearance of that symptom. Moreover, if the rate of remission of a symptom in the presence of psychotherapy is not very high, but is nevertheless significantly higher than the spontaneous remission rate, the therapy can legitimately be offered as at least part of the explanation of the patient's recovery. It follows from these considerations that high probability is neither necessary nor sufficient for bona fide statistical explanation. Statistical relevance, not high probability, I argued, is the key desideratum in statistical explanation.

Henry E. Kyburg, Jr., who commented on my AAAS paper, noticed that a similar point could be made with regard to D-N explanations of particular facts. He illustrated his claim by offering CE-6, the 'explanation' of the dissolving of a sample of table salt on the basis of a 'dissolving spell' (Kyburg 1965). Once the point has been recognized, it is easy to come up with an unlimited supply of similar examples, including my favorite, CE-7, John Jones's 'explanation' of his failure to become pregnant on the basis of his consumption of oral contraceptives.[33]

Scriven's paresis example, CE-5, brought up much earlier in connection with the explanation/prediction symmetry thesis and the notion of expectability, shows that high probability is not required for probabilistic explanation. Scriven's discussion of explanation in evolutionary biology (1959) draws the same conclusion. The appeal to latent untreated syphilis to explain paresis is obviously an appeal to a statistically relevant factor, for the probability that someone with latent untreated syphilis will develop paresis, while not high, is considerably higher than the probability for a randomly selected member of the human population at large. Moreover, it can plausibly be argued that the evolutionary biologist, in explaining occurrences in that domain, also invokes statistically relevant facts. The issue of high probability vs. statistical relevance has thus been joined. It will prove to be a question of considerable importance in the further development of our story.