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Alternative Models

Wesley C. Salmon

STATISTICAL EXPLANATION AND ITS MODELS

THE PHILOSOPHICAL THEORY of scientific explanation first entered the twentieth century in 1962, for that was the year of publication of the earliest bona fide attempt to provide a systematic account of statistical explanation in science.[1] Although the need for some sort of inductive or statistical form of explanation had been acknowledged earlier, Hempel's essay "Deductive-Nomological vs. Statistical Explanation" (1962) contained the first sustained and detailed effort to provide a precise account of this mode of scientific explanation. Given the pervasiveness of statistics in virtually every branch of contemporary science, the late arrival of statistical explanation in philosophy of science is remarkable. Hempel's initial treatment of statistical explanation had various defects, some of which he attempted to rectify in his comprehensive essay "Aspects of Scientific Explanation" (1965a). Nevertheless, the earlier article did show unmistakably that the construction of an adequate model for statistical explanation involves many complications and subtleties that may have been largely unanticipated. Hempel never held the view—expressed by some of the more avid devotees of the D-N model—that *all* adequate scientific explanations must conform to the deductive-nomological pattern. The 1948 Hempel-Oppenheim paper explicitly notes the need for an inductive or statistical model of scientific explanation in order to account for some types of legitimate explanation that actually occur in the various sciences (Hempel, 1965, pp. 250–251). The task of carrying out the construction was, however, left for another occasion. Similar passing remarks about the need for inductive or statistical accounts were made by other authors as well, but the project was not undertaken in earnest until 1962—a striking delay of fourteen years after the 1948 essay.

One can easily form the impression that philosophers had genuine feelings of ambivalence about statistical explanation. A vivid example can be found in Carnap's *Philosophical Foundations of Physics* (1966), which was based upon a seminar he offered at UCLA in 1958.[2] Early in the first chapter, he says:

> The general schema involved in *all explanation* can be expressed symbolically as follows:
> 1. $(x)\ (Px \supset Qx)$
> 2. Pa
> 3. Qa
>
> The first statement is the universal law that applies to any object x. The second statement asserts that a particular object a has the property

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P. These two statements taken together enable us to derive logically the third statement: object *a* has the property *Q* (1966, pp. 7–8, italics added).

After a single intervening paragraph, he continues:

> At times, in giving an explanation, the only *known* laws that apply are statistical rather than universal. In such cases, we must be content with a statistical explanation. (1966, p. 8, italics added)

Farther down on the same page, he assures us that "these are genuine explanations," and on the next page he points out that "In quantum theory . . . we meet with statistical laws that may not be the result of ignorance; they may express the basic structure of the world." I must confess to a reaction of astonishment at being told that all explanations are deductive-nomological, but that some are not, because they are statistical. This lapse was removed from the subsequent paperback edition (Carnap, 1974), which appeared under a new title.

Why did it take philosophers so long to get around to providing a serious treatment of statistical explanation? It certainly was not due to any absence of statistical explanations in science. In antiquity, Lucretius (1951, pp. 66–68) had based his entire cosmology upon explanations involving spontaneous swerving of atoms, and some of his explanations of more restricted phenomena can readily be interpreted as statistical. He asks, for example, why it is that Roman housewives frequently become pregnant after sexual intercourse, while Roman prostitutes to a large extent avoid doing so. Conception occurs, he explains, as a result of a collision between a male seed and a female seed. During intercourse the prostitutes wiggle their hips a great deal, but wives tend to remain passive; as everyone knows, it is much harder to hit a moving target (1951, p. 170).[3] In the medieval period, St. Thomas Aquinas asserted:

> The majority of men follow their passions, which are movements of the sensitive appetite, in which movements of heavenly bodies can cooperate: but few are wise enough to resist these passions. Consequently astrologers are able to foretell the truth in the majority of cases, especially in a general way. But not in particular cases; for nothing prevents man resisting his passions by his free will. (1947, 1:Qu. 115, a. 4, *ad* Obj. 3)

Astrological explanations are, therefore, of the statistical variety. Leibniz, who like Lucretius and Aquinas was concerned about human free will, spoke of causes that incline but do not necessitate (1951, p. 515; 1965, p. 136).

When, in the latter half of the nineteenth century, the kinetic-molecular theory of gases emerged, giving rise to classical statistical mechanics, statistical explanations became firmly entrenched in physics. In this context, it turns out, many phenomena that *for all practical purposes* appear amenable to strict D-N explanation—such as the melting of an ice cube placed in tepid water—must be admitted *strictly speaking* to be explained statistically in terms of probabilities almost indistinguishable from unity. On a smaller scale, Brownian motion involves probabilities that are, both theoretically and practically, definitely less than one. Moreover, two areas of nineteenth-century biology, Darwinian evolution and Mendelian genetics, provide explanations that are basically statistical. In addition, nineteenth-century social scientists approached such topics as suicide, crime, and intelligence by means of "moral statistics" (Hilts, 1973).

In the present century, statistical techniques are used in virtually every branch of science, and we may well suppose that most of these disciplines, if not all, offer statistical explanations of some of the phenomena they treat. The most dramatic example is the statistical interpretation of the equations of quantum mechanics, provided by Max Born and Wolfgang Pauli in 1926–1927; with the aid of this interpretation, quantum theory explains an impressive range of physical facts.[4] What is even more important is that this interpretation brings in statistical considerations at the most basic level. In nineteenth-century science, the use of

probability reflected limitations of human knowledge; in quantum mechanics, it looks as if probability may be an ineluctable feature of the physical world. The Nobel laureate physicist Leon Cooper expresses the idea in graphic terms: "Like a mountain range that divides a continent, feeding water to one side or the other, the probability concept is the divide that separates quantum theory from all of physics that preceded it" (1968, p. 492). Yet it was not until 1962 that any philosopher published a serious attempt at characterizing a statistical pattern of scientific explanation.

INDUCTIVE-STATISTICAL EXPLANATION

When it became respectable for empirically minded philosophers to admit that science not only describes and predicts, but also explains, it was natural enough that primary attention should have been directed to classic and beautiful examples of deductive explanation. Once the D-N model had been elaborated, either of two opposing attitudes might have been taken toward inductive or statistical explanation by those who recognized the legitimacy of explanations of this general sort. It might have been felt, on the one hand, that the construction of such a model would be a simple exercise in setting out an analogue to the D-N model or in relaxing the stringent requirements for D-N explanation in some straightforward way. It might have been felt, on the other hand, that the problems in constructing an appropriate inductive or statistical model were so formidable that one simply did not want to undertake the task. Some philosophers may unreflectingly have adopted the former attitude; the latter, it turns out, is closer to the mark.

We should have suspected as much. If D-N explanations are deductive arguments, inductive or statistical explanations are, presumably, inductive arguments. This is precisely the tack Hempel took in constructing his inductive-statistical or I-S model. In providing a D-N explanation of the fact that this penny conducts electricity, one offers an explanans consisting of two premises: the particular premise that this penny is composed of copper, and the universal law-statement that all copper conducts electricity. The explanandum-statement follows deductively. To provide an I-S explanation of the fact that I was tired when I arrived in Melbourne for a visit in 1978, it could be pointed out that I had been traveling by air for more than twenty-four hours (including stopovers at airports), and almost everyone who travels by air for twenty-four hours or more becomes fatigued. The explanandum gets strong inductive support from those premises; the event-to-be-explained is thus subsumed under a statistical generalization.

It has long been known that there are deep and striking disanalogies between inductive and deductive logic.[5] Deductive entailment is transitive; strong inductive support is not. Contraposition is valid for deductive entailments; it does not hold for high probabilities. These are *not* relations that hold in some approximate way if the probabilities involved are high enough; once we abandon strict logical entailment, and turn to probability or inductive support, they break down entirely. But much more crucially, as Hempel brought out clearly in his 1962 essay, the deductive principle that permits the addition of an arbitrary term to the antecedent of an entailment does not carry over at all into inductive logic. If *A* entails *B*, then *A.C* entails *B*, whatever *C* may happen to stand for. However, no matter how high the probability of *B* given *A*, there is no constraint whatever upon the probability of *B* given both *A* and *C*. To take an extreme case, the probability of a prime number being odd is one, but the probability that a prime number smaller than 3 is odd has the value zero. For those who feel uneasy about applying probability to cases of this arithmetical sort, we can readily supply empirical examples. A thirty-year-old Australian with an advanced case of lung cancer has a low probability of surviving for five more years, even though the probability of surviving to age thirty-five for thirty-year-old Australians in general is quite high. It is *this* basic disanalogy between deductive and inductive (or probabilistic) relations that gives rise to what Hempel called *the ambiguity of inductive-statistical explanation*—a phenomenon that, as he emphasized, has no counterpart in D-N explanation. His *requirement of maximal specificity* was designed expressly to cope with the problem of this ambiguity.

Hempel illustrates the ambiguity of I-S explanation, and the need for the requirement of maximal specificity, by means of the following example (1965, pp. 394–396). John Jones recovers quickly from a streptococcus infection, and when we ask why, we are told that he was given penicillin, and that almost all strep infections clear up quickly after penicillin is administered. The recovery is thus rendered probable relative to these explanatory facts. There are, however, certain strains of streptococcus bacteria that are resistant to penicillin. If, in addition to the above facts, we were told that the infection is of the penicillin-resistant type, then we would have to say that the prompt recovery is rendered *improbable* relative to the available information. It would clearly be scientifically unacceptable to ignore such relevant evidence as the penicillin-resistant character of the infection; the requirement of maximal specificity is designed to block statistical explanations that thus omit relevant facts. It says, in effect, that when the class to which the individual case is referred for explanatory purposes—in this instance, the class of strep infections treated by penicillin—is chosen, we must not know how to divide it into subsets in which the probability of the fact to be explained differs from its probability in the entire class. If it has been ascertained that this particular case involved the penicillin-resistant strain, then the original explanation of the rapid recovery would violate the requirement of maximal specificity, and for that reason would be judged unsatisfactory.[6]

Hempel conceived of D-N explanations as valid deductive arguments satisfying certain additional conditions. Explanations that conform to his inductive-statistical or I-S model are correct inductive arguments also satisfying certain additional restrictions. Explanations of both sorts can be characterized in terms of the following four conditions:

1. The explanation is an argument with correct (deductive or inductive) logical form,
2. At least one of the premises must be a (universal or statistical) law,
3. The premises must be true, and
4. The explanation must satisfy the requirement of maximal specificity.

This fourth condition is automatically satisfied by D-N explanations by virtue of the fact that their explanatory laws are universal generalizations. If all A are B, then obviously there is no subset of A in which the probability of B is other than one. This condition has crucial importance with respect to explanations of the I-S variety. In general, according to Hempel (1962a, p. 10), an explanation is an argument (satisfying these four conditions) to the effect that the event-to-be-explained was to be expected by virtue of certain explanatory facts. In the case of I-S explanations, this means that the premise must lend high inductive probability to the conclusion—that is, the explanandum must be highly probable with respect to the explanans.

Explanations of the D-N and I-S varieties can therefore be schematized as follows (Hempel, 1965, pp. 336, 382):

$$\text{(D-N)}\quad \begin{array}{ll} C_1, C_2, \ldots, C_j & \text{(particular explanatory conditions)} \\ \underline{L_1, L_2, \ldots, L_k} & \text{(general laws)} \\ \quad E & \text{(fact-to-be-explained)} \end{array}$$

The single line separating the premises from the conclusion signifies that the argument is deductively valid.

$$\text{(I-S)}\quad \begin{array}{ll} C_1, C_2, \ldots, C_j & \text{(particular explanatory conditions)} \\ L_1, L_2, \ldots, L_k & \text{(general laws, at least one statistical)} \\ \overline{\overline{\qquad\qquad}}\ [r] & \\ \quad E & \text{(fact-to-be-explained)} \end{array}$$

The double lines separating the premises from the conclusion signifies that the argument is inductively correct, and the number r expresses the degree of inductive probability with which the premises support the conclusion. It is presumed that r is fairly close to one.[7]

The high-probability requirement, which seems such a natural analogue of the deductive entailment relation, leads to difficulties in two ways. First, there are arguments that fulfill all of the requirements imposed by the I-S model, but that patently do not constitute satisfactory scientific explanations. One can maintain, for example, that people who have colds will probably get over them within a fortnight if they take vitamin C, but the use of vitamin C may not explain the recovery, since almost all colds clear up within two weeks regardless. In arguing for the use of vitamin C in the prevention and treatment of colds, Linus Pauling (1970) does not base his claims upon the high probability of avoidance or quick recovery; instead, he urges that massive doses of vitamin C have a bearing upon the probability of avoidance or recovery—that is, the use of vitamin C is relevant to the occurrence, duration, and severity of colds. A *high* probability of recovery, given use of vitamin C, does not confer explanatory value upon the use of this drug with respect to recovery. An *enhanced* probability value does indicate that the use of vitamin C may have some explanatory force. This example, along with a host of others which, like it, fulfill all of Hempel's requirements for a correct I-S explanation, shows that fulfilling these requirements does not constitute a sufficient condition for an adequate statistical explanation.

At first blush, it might seem that the type of relevance problem illustrated by the foregoing example was peculiar to the I-S model, but Henry Kyburg (1965) showed that examples can be found which demonstrate that the D-N model is infected with precisely the same difficulty. Consider a sample of table salt that dissolves upon being placed in water. We ask why it dissolves. Suppose, Kyburg suggests, that someone has cast a dissolving spell upon it—that is, someone wearing a funny hat waves a wand over it and says, "I hereby cast a dissolving spell upon you." We can then 'explain' the phenomenon by mentioning the dissolving spell—without for a moment believing that any actual magic has been accomplished—and by invoking the true universal generalization that all samples of table salt that have been hexed in this manner dissolve when placed in water. Again, an argument that satisfies all of the requirements of Hempel's model patently fails to qualify as a satisfactory scientific explanation because of a failure of relevance. Given Hempel's characterizations of his D-N and I-S models of explanation, it is easy to construct any number of 'explanations' of either type that invoke some irrelevancy as a purported explanatory fact.[8] This result casts serious doubt upon the entire epistemic conception of scientific explanation, as outlined in the previous chapter, insofar as it takes all explanations to be arguments of one sort or another.

The diagnosis of the difficulty can be stated very simply. Hempel's requirement of maximal specificity (RMS) guarantees that *all* known relevant facts must be included in an adequate scientific explanation, but there is no requirement to insure that *only* relevant facts will be included. The foregoing examples bear witness to the need for some requirement of this latter sort. To the best of my knowledge, the advocates of the 'received view' have not, until recently, put forth any such additional condition, nor have they come to terms with counterexamples of these types in any other way.[9] James Fetzer's *requirement of strict maximal specificity*, which rules out the use in explanations of laws that mention nomically irrelevant properties (Fetzer, 1981, pp. 125–126), seems to do the job. In fact, in (Salmon, 1979a, pp. 691–694), I showed how Reichenbach's theory of nomological statements could be used to accomplish the same end.

The second problem that arises out of the high-probability requirement is illustrated by an example furnished by Michael Scriven (1959, p. 480). If someone contracts paresis, the straightforward explanation is that he was infected with syphilis, which had progressed through the primary, secondary, and latent stages without treatment with penicillin. Paresis is one form of tertiary syphilis, and it never occurs except in syphilitics. Yet far less than half of those victims of untreated latent syphilis ever develop paresis. Untreated latent syphilis is the explanation of paresis, but it does not provide any basis on which to say that the explanandum-event was to be expected by virtue of these explanatory facts. Given a victim of latent untreated syphilis, the odds are that he will *not* develop paresis. Many other examples can be found to illustrate the same point. As I understand it, mushroom poisoning may afflict only a small percentage of individuals who eat a particular type of mushroom (Smith, 1958, Introduction), but the eating of the mushroom would unhesitatingly be offered as the explanation in instances of the illness in question. The point is illustrated by remarks on certain species in a guide for mushroom hunters (Smith, 1958, pp. 34, 185):

Helvella infula, "Poisonous to some, but edible for most people. Not recommended."
Chlorophyllum molybdites, "Poisonous to some but not to others. Those who are not made ill by it consider it a fine mushroom. The others suffer acutely."

These examples show that high probability does not constitute a necessary condition for legitimate statistical explanations. Taking them together with the vitamin C example, we must conclude—provisionally, at least—that a high probability of the explanandum relative to the explanans is neither necessary nor sufficient for correct statistical explanations, even if all of Hempel's other conditions are fulfilled. Much more remains to be said about the high-probability requirement, for it raises a host of fundamental philosophical problems, but I shall postpone further discussion of it until chapter 4.

Given the problematic status of the high-probability requirement, it was natural to attempt to construct an alternative treatment of statistical explanation that rests upon different principles. As I argued in (Salmon, 1965), statistical relevance, rather than high probability, seems to be the key explanatory relationship. This starting point leads to a conception of scientific explanation that differs fundamentally and radically from Hempel's I-S account. In the first place, if we are to make use of statistical relevance relations, our explanations will have to make reference to at least two probabilities, for statistical relevance involves a difference between two probabilities. More precisely, a factor C is statistically relevant to the occurrence of B under circumstances A if and only if

$$P(B|A.C) \neq P(B|A) \qquad (1)$$

or

$$P(B|A.C) \neq P(B|A.C). \qquad (2)$$

Conditions (1) and (2) are equivalent to one another, provided that C occurs with a nonvanishing probability within A; since we shall not be concerned with the relevance of factors whose probabilities are zero, we may use either (1) or (2) as our definition of statistical relevance. We say that C is positively relevant to B if the probability of B is greater in the presence of C; it is negatively relevant if the probability of B is smaller in the presence of C. For instance, heavy cigarette smoking is positively relevant to the occurrence of lung cancer, at some later time, in a thirty-year-old Australian male; it is negatively relevant to survival to the age of seventy for such a person.

In order to construct a satisfactory statistical explanation, it seems to me, we need a *prior probability* of the occurrence to be explained, as well as one or more *posterior probabilities*. A crucial feature of the explanation will be the comparison between the prior and posterior probabilities. In Hempel's case of the streptococcus infection, for instance, we might begin with the probability, in the entire class of people with streptococcus infections, of a quick recovery. We realize, however, that the administration of penicillin is statistically relevant to quick recovery, so we compare the probability of quick recovery among those who have received penicillin with the probability of quick recovery among those who have not received penicillin. Hempel warns, however, that there is another relevant factor, namely, the existence of the penicillin-resistant strain of bacteria. We must, therefore, take that factor into account as well. Our original reference class has been divided into four parts: (1) infection by non-penicillin-resistant bacteria, penicillin given; (2) infection by non-penicillin-resistant bacteria, no penicillin given; (3) infection by penicillin-resistant bacteria, penicillin given; (4) infection by penicillin-resistant bacteria, no penicillin given. Since the administration of penicillin is irrelevant to quick recovery in case of penicillin-resistant infections, the subclasses (3) and (4) of the original reference class should be merged to yield (3′) infection by penicillin-resistant bacteria. If John Jones is a member of (1), we have an explanation of his quick recovery, according to the S-R approach, not because the probability is high, but, rather, because it differs significantly from the probability in the original reference class. We shall see later

what must be done if John Jones happens to fall into class (3′).

By way of contrast, Hempel's earlier high-probability requirement demands only that the posterior probability be sufficiently large—whatever that might mean—but makes no reference at all to any prior probability. According to Hempel's abstract model, we ask, "Why is individual x a member of B?" The answer consists of an inductive argument having the following form:

$$\frac{\begin{array}{l} P(B|A) = r \\ x \text{ is an } A \end{array}}{x \text{ is a } B} \; [r]$$

As we have seen, even if the first premise is a statistical law, r is high, the premises are true, and the requirement of maximal specificity has been fulfilled, our 'explanation' may be patently inadequate, due to failure of relevancy.

In (Salmon, 1970, pp. 220–221), I advocated what came to be called the statistical-relevance or S-R model of scientific explanation. At that time, I thought that anything that satisfied the conditions that define that model would qualify as a legitimate scientific explanation. I no longer hold that view. It now seems to me that the statistical relationships specified in the S-R model constitute the *statistical basis* for a bona fide scientific explanation, but that this basis must be supplemented by certain *causal factors* in order to constitute a satisfactory scientific explanation. In chapters 5–9 I shall discuss the causal aspects of explanation. In this chapter, however, I shall confine attention to the statistical basis, as articulated in terms of the S-R model. Indeed, from here on I shall speak, not of the S-R model, but, rather, of the *S-R basis*.[10]

Adopting the S-R approach, we begin with an explanatory question in a form somewhat different from that given by Hempel. Instead of asking, for instance, "Why did x get well within a fortnight?" we ask, "Why did this person with a cold get well within a fortnight?" Instead of asking, "Why is x a B?" we ask, "Why is x, which is an A, also a B?" The answer—at least for preliminary purposes—is that x is also a C, where C is *relevant* to B within A. Thus we have a prior probability $P(B|A)$—in this case, the probability that a person with a cold (A) gets well within a fortnight (B). Then we let C stand for the taking of vitamin C. We are interested in the posterior probability $P(B|A.C)$ that a person with a cold who takes vitamin C recovers within a fortnight. If the prior and posterior probabilities are equal to one another, the taking of vitamin C can play no role in explaining why this person recovered from the cold within the specified period of time. If the posterior probability is not equal to the prior probability, then C may, under certain circumstances, furnish part or all of the desired explanation. A large part of the purpose of the present book is to investigate the way in which considerations that are statistically relevant to a given occurrence have or lack explanatory import.

We cannot, of course, expect that every request for a scientific explanation will be phrased in canonical form. Someone might ask, for example, "Why did Mary Jones get well in no more than a fortnight's time?" It might be clear from the context that she was suffering from a cold, so that the question could be reformulated as, "Why did this person who was suffering from a cold get well within a fortnight?" In some cases, it might be necessary to seek additional clarification from the person requesting the explanation, but presumably it will be possible to discover what explanation is being called for. This point about the form of the explanation-seeking question has fundamental importance. We can easily imagine circumstances in which an altogether different explanation is sought by means of the same initial question. Perhaps Mary had exhibited symptoms strongly suggesting that she had mononucleosis; in this case, the fact that it was only an ordinary cold might constitute the explanation of her quick recovery. A given why-question, construed in one way, may elicit an explanation, while otherwise construed, it asks for an explanation that cannot be given. "Why did the Mayor contract paresis?" might mean, "Why did this adult human develop paresis?" or, "Why did this syphilitic develop paresis?" On the first construal, the question has a suitable answer, which we have already discussed. On the second construal, it has no answer—at any rate, we cannot give an answer—for we do not know of any fact in addition to syphilis that is relevant to the occurrence of paresis. Some philosophers have argued, because of these considerations, that scientific explanation has an unavoidably pragmatic aspect (e.g., van Fraassen, 1977, 1980). If this means simply that there are cases in which people ask for explanations in unclear or ambiguous terms, so that we cannot tell what explanation is being requested without further clarification, then so be it. No one would deny that we cannot be expected to supply explanations unless we know what it is we are being asked to explain. To this extent, scientific explanation surely has pragmatic or contextual components. Dealing with these considerations is, I believe, tantamount to choosing a suitable reference class with respect to which the prior probabilities are to be taken and specifying an appropriate sample space for purposes of a particular explanation. More will be said about these two items in the next section. In chapter 4—in an extended discussion of van Fraassen's theory—we shall return to this issue of pragmatic aspects of explanation, and we shall consider the question of whether there are any others.

THE STATISTICAL-RELEVANCE APPROACH

Let us now turn to the task of giving a detailed elaboration of the S-R basis. For purposes of initial presentation, let us construe the terms $A, B, C, \ldots$ (with or without subscripts) as referring to classes, and let us construe our probabilities in some sense as relative frequencies. This *does not mean* that

the statistical-relevance approach is tied in any crucial way to a frequency theory of probability. I am simply adopting the heuristic device of picking examples involving frequencies because they are easily grasped. Those who prefer propensities, for example, can easily make the appropriate terminological adjustments, by speaking of chance setups and outcomes of trials where I refer to reference classes and attributes. With this understanding in mind, let us consider the steps involved in constructing an S-R basis for a scientific explanation:

1. We begin by selecting an appropriate reference class A with respect to which the prior probabilities $P(B_i|A)$ of the B_is are to be taken.
2. We impose an *explanandum-partition* upon the initial reference class A in terms of an exclusive and exhaustive set of attributes $B_1, \ldots, B_m$; this defines a sample space for purposes of the explanation under consideration. (This partition was not required in earlier presentations of the S-R model.)
3. Invoking a set of statistically relevant factors $C_1, \ldots, C_s$, we partition the initial reference class A into a set of mutually exclusive and exhaustive cells $A.C_1, \ldots, A.C_s$. The properties $C_1, \ldots, C_s$ furnish the *explanans-partition.*
4. We ascertain the associated probability relations:
   prior probabilities
   $$P(B_i|A) = p_i \quad \text{for all } i\ (1 \leq i \leq m)$$
   posterior probabilities
   $$P(B_i|A.C_j) = p_{ij} \quad \text{for all } i \text{ and } j\ (1 \leq i \leq m) \text{ and } (1 \leq j \leq s)$$
5. We require that each of the cells $A.C_j$ be homogeneous with respect to the explanandum-partition $\{B_i\}$; that is, none of the cells in the partition can be further subdivided in any manner relevant to the occurrence of any B_i. (This requirement is somewhat analogous to Hempel's requirement of maximal specificity, but as we shall see, it is a much stronger condition.)
6. We ascertain the relative sizes of the cells in our explanans-partition in terms of the following marginal probabilities:
   $$P(C_j|A) = q_j$$
   (These probabilities were not included in earlier versions of the S-R model; the reasons for requiring them now will be discussed later in this chapter.)
7. We require that the explanans-partition be a maximal homogeneous partition, that is—with an important exception to be noted later—for $i \neq k$ we require that $p_{ji} \neq p_{jk}$. (This requirement assures us that our partition in terms of $C_1, \ldots, C_m$ does not introduce any irrelevant subdivision in the initial reference class A.)
8. We determine which cell $A.C_j$ contains the individual x whose possession of the attribute B_i was to be explained. The probability of B_i within the cell is given in the list under 4.

Consider in a rather rough and informal manner the way in which the foregoing pattern of explanation might be applied in a concrete situation; an example of this sort was offered by James Greeno (1971a, pp. 89–90). Suppose that Albert has committed a delinquent act—say, stealing a car, a major crime—and we ask for an explanation of that fact. We ascertain from the context that he is an American teen-ager, and so we ask, "Why did this American teen-ager commit a serious delinquent act?" The prior probabilities, which we take as our point of departure, so to speak, are simply the probabilities of the various degrees of juvenile delinquency (B_i) among American teen-agers (A)—that is, $P(B_i|A)$. We will need a suitable explanandum-partition; Greeno suggests B_1 = no criminal convictions, B_2 = conviction for minor infractions only, B_3 = conviction for a major offense. Our sociological theories tell us that such factors as sex, religious background, marital status of parents, type of residential community, socioeconomic status, and several others are relevant to delinquent behavior. We therefore take the initial reference class of American teen-agers and divide it into males and females; Jewish, Protestant, Roman Catholic, no religion; parents married, parents divorced, parents never married; urban, suburban, rural place of residence; upper, middle, lower class; and so forth. Taking such considerations into account, we arrive at a large number *s* of cells in our partition. We assign probabilities of the various degrees of delinquent behavior to each of the cells in accordance with 4, and we ascertain the probability of a randomly selected American teen-ager belonging to each of the cells in accordance with 6. We find the cell to which Albert belongs—for example, male, from a Protestant background, parents divorced, living in a suburban area, belonging to the middle class. If we have taken into account all of the relevant factors, and if we have correctly ascertained the probabilities associated with the various cells of our partitions, then we have an S-R basis for the explanation of Albert's delinquency that conforms to the foregoing schema. If it should turn out (contrary to what I believe actually to be the case) that the probabilities of the various types of delinquency are the same for males and for females, then we would not use sex in partitioning our original reference class. By condition 5 we must employ *every* relevant factor; by condition 7 we must employ *only* relevant factors. In many concrete situations, including the present examples, we know that we have not found all relevant considerations; however, as Noretta Koertge rightly emphasized (1975), that is an ideal for which we may aim. Our philosophical analysis is designed to capture the notion of a fully satisfactory explanation.

Nothing has been said, so far, concerning the rationale for conditions 2 and 6, which are here added to the S-R basis for the first time. We must see why these requirements are needed. Condition 2 is quite straightforward; it amounts to a requirement that the sample space for the problem at hand be specified. As we shall see when we discuss Greeno's information-theoretic approach in chapter 4, both the explanans-partition and the explanandum-partition are needed to measure the information transmitted in any explanatory scheme. This is a useful measure of the explanatory value of a theory. In addition, as we shall see when we discuss van Fraassen's treatment of why-questions in chapter 4, his contrast class, which is the same as our explanandum-partition, is needed in some cases to specify precisely what explanation is being sought. In dealing with the question "Why did Albert steal a car?" we used Greeno's suggested explanandum-partition. If, however, we had used different partitions (contrast classes), other explanations might have been called forth. Suppose that the contrast class included: Albert steals a car, Bill steals a car, Charlie steals a car, and so forth. Then the answer might have involved no sociology whatever; the explanation might have been that, among the members of his gang, Albert is most adept at hot-wiring. Suppose, instead, that the contrast class had included: Albert steals a car, Albert steals a diamond ring, Albert steals a bottle of whiskey, and so forth. In that case, the answer might have been that he wanted to go joyriding.

The need for the marginal probabilities mentioned in 6 arises in the following way. In many cases, such as the foregoing delinquency example, the terms C_j that furnish the explanans-partition of the initial reference class are conjunctive. A given cell is determined by several distinct factors: sex *and* religious background *and* marital status of parents *and* type of residential community *and* socioeconomic status *and* . . . which may be designated D_k, E_n, F_r, . . . These factors will be the probabilistic contributing causes and counteracting causes that tend, respectively, to produce or prevent delinquency. In attempting to understand the phenomenon in question, it is important to know how each factor is relevant—whether positively or negatively, and how strongly—both in the population at large and in various subgroups of the population. Consider, for example, the matter of sex. It may be that within the entire class of American teen-agers (A) the probability of serious delinquency (B_3) is greater among males than it is among females. If so, we would want to know by how much the probability among males exceeds the probability among females and by how much it exceeds the probability in the entire population. We also want to know whether being male is always positively relevant to serious delinquency, or whether in combination with certain other factors, it may be negatively relevant or irrelevant. Given two groups of teen-agers—one consisting entirely of boys and the other entirely of girls, but alike with respect to all of the other factors—we want to know how the probabilities associated with delinquency in each of the two groups are related to one another. It might be that in each case of two cells in the explanandum-partition that differ from one another only on the basis of gender, the probability of serious delinquency in the male group is greater than it is in the female group. It might turn out, however, that sometimes the two probabilities are equal, or that in some cases the probability is higher in the female group than it is in the corresponding male group. Relationships of all of these kinds are logically possible.

It is a rather obvious fact that each of two circumstances can individually be positively relevant to a given outcome, but their conjunction can be negatively relevant or irrelevant. Each of two drugs can be positively relevant to good health, but taken together, the combination may be detrimental—for example, certain antidepressive medications taken in conjunction with various remedies for the common cold can greatly increase the chance of dangerously high blood pressure (Goodwin and Guze, 1979). A factor that is a contributing cause in some circumstances can be a counteracting cause in other cases. Problems of this sort have been discussed, sometimes under the heading of "Simpson's paradox," by Nancy Cartwright (1983, essay 1) and Bas van Fraassen (1980, pp. 108, 148–151). In (Salmon, 1975c), I have spelled out in detail the complexities that arise in connection with statistical relevance relations. The moral is that we need to know not only how the various factors D_k, E_n, F_r, . . . , are relevant to the outcome, B_i, but how the relevance of each of them is affected by the presence or absence of the others. Thus, for instance, it is possible that being female might in general be negatively relevant to delinquency, but it might be positively relevant among the very poor.

Even if all of the prior probabilities $P(B_i|A)$ and all of the posterior probabilities $P(B_i|A.C_j)$ furnished under condition 4 are known, it is not possible to deduce the conditional probabilities of the B_i's with respect to the individual conjuncts that make up the C_j's or with respect to combinations of them. Without these conditional probabilities, we will not be in a position to ascertain all of the statistical relevance relations that are required. We therefore need to build in a way to extract that information. This is the function of the marginal probabilities $P(C_j|A)$ required by condition 6. If these are known, such conditional probabilities as $P(B_i|A.D_k)$, $P(B_i|A.E_n)$, and $P(B_i|A.D_k.E_n)$ can be derived.[11] When 2 and 6 are added to the earlier characterization of the S-R model (Salmon et al., 1971), then, I believe, we have gone as far as possible in characterizing scientific explanations at the level of statistical relevance relations.

Several features of the new version of the S-R basis deserve explicit mention. It should be noted, in the first place, that conditions 2 and 3 demand that the entire initial reference class A be partitioned, while conditions 4 and 6 require that *all* of the associated probability values be given. This is one of several respects in which it differs from Hempel's I-S model. Hempel requires only that the individual mentioned in the explanplaced within an appropriate class, satisfying his requirement of maximal specificity, but he does not ask for information about any class in either the explanandum-partition or the explanans-partition to which that individual does not belong. Thus he might go along in requiring that Bill Smith be referred to the class of American male teen-agers coming from a Protestant background, whose parents are divorced, and who is a middle-class suburban dweller, and in asking us to furnish the probability of his degree of delinquency within that class. But why, it may surely be asked, should we be concerned with the probability of delinquency in a lower-class, urban-American, female teen-ager from a Roman Catholic background whose parents are still married? What bearing do such facts have on Bill Smith's delinquency? The answer, I think, involves serious issues concerning scientific generality. If we ask why this American teen-ager becomes a delinquent, then, it seems to me, we are concerned with *all* of the factors that are relevant to the occurrence of delinquency, and with the ways in which these factors are relevant to that phenomenon (cf. Koertge, 1975). To have a satisfactory scientific answer to the question of why this A is a B_i—to achieve full scientific understanding—we need to know the factors that are relevant to the occurrence of the various B_is for *any* randomly chosen or otherwise unspecified member of A. It was mainly to make good on this desideratum that requirement 6 was added. Moreover, as Greeno and I argued in *Statistical Explanation and Statistical Relevance,* a good measure of the value of an S-R basis is the gain in information furnished by the complete partitions and the associated probabilities. This measure cannot be applied to the individual cells one at a time.

A fundamental philosophical difference between our S-R basis and Hempel's I-S model lies in the interpretation of the concept of homogeneity that appears in condition 5. Hempel's requirement of maximal specificity, which is designed to achieve a certain kind of homogeneity in the reference classes employed in I-S explanations, is *epistemically relativized.* This means, in effect, that we must not *know* of any way to make a relevant partition, but it certainly does not demand that no possibility of a relevant partition can exist unbeknown to us. As I view the S-R basis, in contrast, condition 5 demands that the cells of our explanans-partition be *objectively* homogeneous; for this model, homogeneity is not epistemically relativized. Since this issue of epistemic relativization versus objective homogeneity is discussed at length in chapter 3, it is sufficient for now merely to call attention to this complex problem.[12]

Condition 7 has been the source of considerable criticism. One such objection rests on the fact that the initial reference class A, to which the S-R basis is referred, may not be maximal. Regarding Kyburg's hexed salt example, mentioned previously, it has been pointed out that the class of samples of table salt is not a maximal homogeneous class with respect to solubility, for there are many other chemical substances that have the same probability—namely, unity—of dissolving when placed in water. Baking soda, potassium chloride, various sugars, and many other compounds have this property. Consequently, if we take the maximality condition seriously, it has been argued, we should not ask, "Why does this sample of table salt dissolve in water?" but, rather, "Why does this sample of matter in the solid state dissolve when placed in water?" And indeed, one can argue, as Koertge has done persuasively (1975), that to follow such a policy often leads to significant scientific progress. Without denying her important point, I would nevertheless suggest, for purposes of elaborating the formal schema, that we take the initial reference class A as given by the explanation-seeking why-question, and look for relevant partitions within it. A significantly different explanation, which often undeniably represents scientific progress, may result if a different why-question, embodying a broader initial reference class, is posed. If the original question is not presented in a form that unambiguously determines a reference class A, we can reasonably discuss the advantages of choosing a wider or a narrower class in the case at hand.

Another difficulty with condition 7 arises if 'accidentally'—so to speak—two different cells in the partition, $A.C_i$ and $A.C_j$, happen to have equal associated probabilities p_{ki} and p_{kj} for all cells B_k in the explanandum-partition. Such a circumstance might arise if the cells are determined conjunctively by a number of relevant factors, and if the differences between the two cells cancel one another out. It might happen, for example, that the probabilities of the various degrees of delinquency—major offense, minor offense, no offense—for an upper-class, urban, Jewish girl would be equal to those for a middle-class, rural, Protestant boy. In this case, we might want to relax condition 7, allowing $A.C_i$ and $A.C_j$ to stand as separate cells, provided they differ with respect to at least two of the terms in the conjunction, so that we are faced with a fortuitous canceling of relevant factors. If, however, $A.C_i$ and $A.C_j$ differed with respect to only one conjunct, they would have to be merged into a single cell. Such would be the case if, for example, among upper-class, urban-dwelling, American teen-agers whose religious background is atheistic and whose parents are divorced, the probability of delinquent behavior were the same for boys as for girls. Indeed, we have already encountered this situation in connection with Hempel's example of the streptococcus infection. If the infection is of the penicillin-resistant variety, the probability of recovery in a given period of time is the same whether penicillin is administered or not. In such cases, we want to say, there is no relevant difference between the two classes—not that relevant factors were canceling one another out. I bring this problem up for consideration at this point, but I shall not make a consequent modification in the formal characterization of the S-R basis, for I believe that problems of this sort are best handled in the light of causal relevance relations. Indeed, it seems advisable to postpone detailed consideration of the whole matter of regarding the cells $A.C_j$ as being determined conjunctively until causation has been explicitly introduced into the discussion. As we shall see in chapter 9, (Humphreys, 1981, 1983) and (Rogers, 1981) provide useful suggestions for handling just this issue.

Perhaps the most serious objection to the S-R model of scientific explanation—as it was originally presented—is based upon the principle that *mere* statistical correlations explain nothing. A rapidly falling barometric reading is a sign of an imminent storm, and it is *highly correlated* with the onset of storms, but it certainly does not *explain* the occurrence of a storm. The S-R approach does, however, have a way of dealing with examples of this sort. A factor C, which is relevant to the occurrence of B in the presence of A, may be screened off in the presence of some additional factor D; the screening-off relation is defined by equations (3) and (4), which follow. To illustrate, given a series of days (A) in some particular locale, the probability of a storm occurring (B) is in general quite different from the probability of a storm if there has been a recent sharp drop in the barometric reading (C). Thus C is statistically relevant to B within A. If, however, we take into consideration the further fact that there is an actual drop in atmospheric pressure (D) in the region, then it is irrelevant whether that drop is registered on a barometer. In the presence of D and A, C becomes irrelevant to B; we say that D screens off C from B—in symbols,

$$P(B|A.C.D) = P(B|A.D). \quad (3)$$

However, C does not screen off D from B, that is,

$$P(B|A.C.D) \neq P(B|A.C), \quad (4)$$

for barometers sometimes malfunction, and it is the atmospheric pressure, not the reading on the barometer per se, that is directly relevant to the occurrence of the storm. A factor that has been screened off is irrelevant, and according to the definition of the S-R basis (condition 7), it is not to be included in the explanation. The falling barometer does not explain the storm.

Screening off is frequent enough and important enough to deserve further illustration. A study, reported in the news media a few years ago, revealed a positive correlation between coffee drinking and heart disease, but further investigation showed that this correlation results from a correlation between coffee drinking and cigarette smoking. It turned out that cigarette smoking screened off coffee drinking from heart disease, thus rendering coffee drinking statistically (as well as causally and explanatorily) irrelevant to heart disease. Returning to a previous example for another illustration, one could reasonably suppose that there is some correlation between low socioeconomic status and paresis, for there may be a higher degree of sexual promiscuity, a higher incidence of venereal disease, and less likelihood of adequate medical attention if the disease occurs. But the contraction of syphilis screens off such factors as degree of promiscuity, and the fact of syphilis going untreated screens off any tendency to fail to get adequate medical care. Thus when an individual has latent untreated syphilis, all other such circumstances as low socioeconomic status are screened off from the development of paresis.

As the foregoing examples show, there are situations in which one circumstance or occurrence is correlated with another because of an indirect causal relationship. In such cases, it often happens that the more proximate causal factors screen off those that are more remote. Thus 'mere correlations' are replaced in explanatory contexts with correlations that are intuitively recognized to have explanatory force. In *Statistical Explanation and Statistical Relevance*, where the S-R model of statistical explanation was first explicitly named and articulated, I held out some hope (but did not try to defend the thesis) that all of the causal factors that play any role in scientific explanation could be explicated in terms of statistical relevance relations—with the screening-off relation playing a crucial role. As I shall explain in chapter 6, I no longer believe this is possible. A large part of the material in the present book is devoted to an attempt to analyze the nature of the causal relations that enter into scientific explanations, and the manner in which they function in explanatory contexts. After characterizing the S-R model, I wrote:

> One might ask on what grounds we can claim to have characterized explanation. The answer is this. When an explanation (as herein explicated) has been provided, we know exactly how to regard any *A* with respect to the property *B*. We know which ones to bet on, which to bet against, and at what odds. We know precisely what degree of expectation is rational. We know how to face uncertainty about an *A*'s being a *B* in the most reasonable, practical, and efficient way. We know every factor that is relevant to an *A* having the property *B*. We know exactly what weight should have been attached to the prediction that this *A* will be a *B*. We know all of the regularities (universal and statistical) that are relevant to our original question. What more could one ask of an explanation? (Salmon et al., 1971, p. 78)

The answer, of course, is that we need to know something about the causal relationships as well.

In acknowledging this egregious shortcoming of the S-R model of scientific explanation, I am not abandoning it completely. The attempt, rather, is to supplement it in suitable ways. While recognizing its incompleteness, I still think it constitutes a sound basis upon which to erect a more adequate account. And at a fundamental level, I still think it provides important insights into the nature of scientific explanation.

In the introduction to *Statistical Explanation and Statistical Relevance*, I offered the following succinct comparison between Hempel's I-S model and the S-R model:

> I-S model: an explanation is an *argument* that renders the explanandum *highly probable*.
> S-R model: an explanation is an *assembly of facts statistically relevant* to the explanandum, *regardless of the degree of probability* that results.

It was Richard Jeffrey (1969) who first explicitly formulated the thesis that (at least some) statistical explanations are not arguments; it is beautifully expressed in his brief paper, "Statistical Explanation vs. Statistical Inference," which was reprinted in *Statistical Explanation and Statistical Relevance*. In (Salmon, 1965, pp. 145–146), I had urged that positive relevance rather than high probability is the desideratum in statistical explanation. In (Salmon, 1970), I expressed the view, which many philosophers found weird and counter-intuitive (e.g., L. J. Cohen, 1975), that statistical explanations may even embody *negative* relevance—that is, an explanation of an event may, in some cases, show that the event to be explained is less probable than we had initially realized. I still do not regard that thesis as absurd. In an illuminating discussion of the explanatory force of positively and negatively relevant factors, Paul Humphreys (1981) has introduced some felicitous terminology for dealing with such cases, and he has pointed to an important constraint. Consider a simple example. Smith is stricken with a heart attack, and the doctor says, "*Despite* the fact that Smith exercised regularly and had given up smoking several years ago, he contracted heart disease *because* he was seriously overweight." The "because" clause mentions those factors that are positively relevant and the "despite" clause cites those that are negatively relevant. Humphreys refers to them as *contributing causes* and *counteracting causes*, respectively. When we discuss causal explanation in later chapters, we will want to say that a complete explanation of an event must make mention of the causal factors that tend to prevent its occurrence as well as those that tend to bring it about. Thus it is *not* inappropriate for the S-R basis to include factors that are negatively relevant to the explanandum-event. As Humphreys points out, however, we would hardly consider as appropriate a putative explanation that had only negative items in the "despite" clause and no positive items in the "because" category. "Despite the fact that Jones never smoked, exercised regularly, was not overweight, and did not have elevated levels of triglycerides and cholesterol, he died of a heart attack," would hardly be considered an acceptable *explanation* of his fatal illness.

Before concluding this chapter on models of statistical explanation, we should take a brief look at the deductive-homological-probabilistic (D-N-P) model of scientific explanation offered by Peter Railton (1978). By employing well-established statistical laws, such as that covering the spontaneous radioactive decay of unstable nuclei, it is possible to deduce the fact that a decay-event for a particular isotope has a certain probability of occurring within a given time interval. For an atom of carbon 14 (which is used in radiocarbon dating in archaeology, for example), the probability of a decay in 5,730 years is ½. The explanation of *the probability of the decay-event* conforms to Hempel's deductive-nomological pattern. Such an explanation does not, however, explain the actual occurrence of a decay, for, given the probability of such an event—however high or low—the event in question may not even occur. Thus the explanation does not qualify as an argument to the effect that the event-to-be-explained was to be expected with deductive certainty, given the explanans. Railton is, of course, clearly aware of the fact. He goes on to point out, nevertheless, that if we simply attach an "addendum" to the deductive argument stating that the event-to-be-explained did, in fact, occur in the case at hand, we can claim to have a probabilistic *account*—which is not a deductive or inductive argument—of the occurrence of the event. In this respect, Railton is in rather close agreement with Jeffrey (1969) that some explanations are not arguments. He also agrees with Jeffrey in emphasizing the importance of exhibiting the physical mechanisms that lead up to the probabilistic occurrence that is to be explained. Railton's theory—like that of Jeffrey—has some deep affinities to the S-R model. In including a reference to physical mechanisms as an essential part of his D-N-P model, however, Railton goes beyond the view that statistical relevance relations, in and of themselves, have explanatory import. His theory of scientific explanation can be more appropriately characterized as causal or mechanistic. It is closely related to the two-tiered causal-statistical account that I am attempting to elaborate as an improvement upon the S-R model.

Although, with Kyburg's help, I have offered what seem to be damaging counterexamples to the D-N model—for instance, the one about the man who explains his own avoidance of pregnancy on the basis of his regular consumption of his wife's birth control pills (Salmon et al., 1971, p. 34)—the major emphasis has been upon statistical explanation, and that continues to be the case in what follows. Aside from the fact that contemporary science obviously provides many statistical explanations of many types of phenomena, and that any philosophical theory of statistical explanation has only lately come forth, there is a further reason for focusing upon statistical explanation. As I maintained in chapter 1, we can identify three distinct approaches to scientific explanation that do not seem to differ from one another in any important way as long as we confine our attention to contexts in which all of the explanatory laws are universal generalizations. I shall argue in chapter 4, however, that these three general conceptions of scientific explanation can be seen to differ radically from one another when we move on to situations in which statistical explanations are in principle the best we can achieve. Close consideration of statistical explanations, with sufficient attention to their causal ingredients, provides important insight into the underlying philosophical questions relating to our scientific understanding of the world.

NOTES

1. Ilkka Niiniluoto (1981, p. 444) suggests that "Peirce should be regarded as the true founder of the theory of inductive-probabilistic explanation" on account of this statement, "The statistical syllogism may be conveniently termed the explanatory syllogism" (Peirce, 1932, 2:716). I am inclined to disagree, for one isolated and unelaborated statement of that sort can hardly be considered even the beginnings of any genuine theory.

2. As Carnap reports in the preface, the seminar proceedings were recorded and transcribed by his wife. Martin Gardner edited—it would probably be more accurate to say "wrote up"—the proceedings and submitted them to Carnap, who rewrote them extensively. There is little doubt that Carnap saw and approved the passages I have quoted.

3. Lucretius writes: "A woman makes conception more difficult by offering a mock resistance and accepting Venus with a wriggling body. She diverts the furrow from the straight course of the ploughshare and makes the seed fall wide of the plot. These tricks are employed by prostitutes for their own ends, so that they may not conceive *too frequently* and be laid up by pregnancy" (1951, p. 170, italics added).

4. See (Wessels, 1982), for an illuminating discussion of the history of the statistical interpretation of quantum mechanics.

5. These are spelled out in detail in (Salmon, 1965a). See (Salmon, 1967, pp. 109–111) for a discussion of the 'almost-deduction' conception of inductive inference.

6. We shall see in chapter 3 that the requirement of maximal specificity, as formulated by Hempel in his (1965) and revised in his (1968), does not actually do the job. Nevertheless, this was clearly its intent.

7. It should be mentioned in passing that Hempel (1965, pp. 380–381) offers still another model of scientific explanation that he characterizes as deductive-statistical (D-S). In an explanation of this type, a statistical regularity is explained by deducing it from other statistical laws. There is no real need, however, to treat such explanations as a distinct type, for they fall under the D-N schema, just given, provided we allow that at least one of the laws may be statistical. In the present context, we are concerned only with statistical explanations of nondeductive sorts.

8. Many examples are presented and analyzed in (Salmon et al., 1971, pp. 33–40). Nancy Cartwright (1983, pp. 26–27) errs when she attributes to Hempel the requirement that a statistical explanation increase the probability of the explanandum; this thesis, which I first advanced in (Salmon, 1965), was never advocated by Hempel. Shortly thereafter (1983, 28–29), she provides a correct characterization of the relationships among the views of Hempel, Suppes, and me.

9. In Hempel's most recent discussion of statistical explanation, he appears to maintain the astonishing view that although such examples have *psychologically* misleading features, they do qualify as *logically* satisfactory explanations (1977, pp. 107–111).

10. I am extremely sympathetic to the thesis, expounded in (Humphreys, 1983), that probabilities—including those appearing in the S-R basis—are important tools in the construction of scientific explanations, but that they do not constitute any part of a scientific explanation per se. This thesis allows him to relax considerably the kinds of maximal specificity or homogeneity requirements that must be satisfied by statistical or probabilistic explanations. A factor that is statistically relevant may be causally irrelevant because, for example, it does not convert any contributing causes to counteracting causes or vice versa. This kind of relaxation is attractive in a theory of scientific explanation, for factors having small statistical relevance often seem otiose. Humphreys' approach does not show, however, that such relevance relations can be omitted from the S-R basis; on the contrary, the S-R basis must include such factors in order that we may ascertain whether they can be omitted from the causal explanation or not. I shall return to Humphreys' concept of aleatory explanation in chapter 9.

11. Suppose, for example, that we wish to compute $P(B_i|A.D_k)$, where $D_k = C_{j1} \vee \ldots \vee C_{j4}$, the cells C_{jr} being mutually exclusive. This can be done as follows. We are given $P(C_j|A)$ and $P(B_i|A.C_j)$. By the multiplication theorem,

$$P(D_k.B_i|A) = P(D_k|A) \times P(B_i|A.D_k)$$

Assuming $P(D_k \mid A) \neq 0$, we have,

$$P(B_i \mid A.D_k) = P(D_k.B_i \mid A)/P(D_k|A) \qquad (*)$$

By the addition theorem

$$P(D_k \mid A) = \sum_{r=1}^{q} P(C_{j_r} \mid A)$$

$$P(D_k.B_i \mid A) = \sum_{r=1}^{q} P(C_{j_r}.B_i \mid A)$$

By the multiplication theorem

$$P(D_k.B_i \mid A) = \sum_{r=1}^{q} P(C_{j_r} \mid A) \times P(B_i \mid A.C_{j_r})$$

Substitution in (*) yields the desired relation:

$$P(B_i \mid A.D_k) = \frac{\sum_{r=1}^{q} P(C_{j_r} \mid A) \times P(B_i \mid A.C_{j_r})}{\sum_{r=1}^{q} P(C_{j_r} \mid A)}$$

12. Cartwright (1983, p. 27) asserts that on Hempel's account, "what counts as a good explanation is an objective, person-independent matter," and she applauds him for holding that view. I find it difficult to reconcile her characterization with Hempel's repeated emphatic assertion (prior to 1977) of the doctrine of essential epistemic relativization of inductive-statistical explanation. In addition, she complains that my way of dealing with problems concerning the proper formulation of the explanation-seeking why-question—that is, problems concerning the choice of an appropriate initial reference class—"makes explanation a subjective matter" (ibid., p. 29). "What explains what," she continues, "depends on the laws and facts true in our world, and cannot be adjusted by shifting our interest or our focus" (ibid.). This criticism seems to me to be mistaken. Clarification of the question is often required to determine what it is that is to be explained, and this may have pragmatic dimensions. However, once the explanandum has been unambiguously specified, on my view, the identification of the appropriate explanans is fully objective. I am in complete agreement with Cartwright concerning the desirability of such objectivity; moreover, my extensive concern with objective homogeneity is based directly upon the desire to eliminate from the theory of statistical explanation such subjective features as epistemic relativization.