Notes

Introduction

1. See Rudolf Carnap (1966, reissued 1974, pp. 12–17) for interesting comments on the transition from the denial to the acceptance of the view that science can furnish explanations. This passage includes an interesting discussion of Hans Driesch's vitalism (including the appeal to entelechies), and of reactions to it by the logical positivists of the Vienna Circle.

2. It is possible, of course, to adopt a hypothetical or 'suspend the truth' attitude in which one asks how a particular event could be explained *if it were to occur*. This is *not* Velikovsky's attitude.

3. For a brief and accessible introduction see Gale (1981). The heading of this rather flamboyant article reads, "Certain conditions, such as temperature, were favorable to the emergence of life on earth. The anthropic principle argues the reverse: the presence of life may 'explain' the conditions." In the article Gale adds, "It is fair to say, however, that not all cosmologists and philosophers of science assent to the utility of the anthropic principle, or even to its legitimacy. Here I shall describe some of the ways in which the principle has been applied and let the reader judge its validity" (p. 154). For a more thorough and technical, as well as more recent, treatment see Barrow and Tipler (1986).

4. That feature, in itself, should give us pause, for it is an elementary logical fallacy to infer the truth of the premises from the truth of the conclusion of a valid deductive argument. As a matter of fact, neither Hempel nor I considers the traditional hypothetico-deductive schema an adequate characterization of scientific confirmation, but it seems to be so regarded by many people; see, for example, Braithwaite, *Scientific Explanation*, (1953 p. 9). Discussions of the shortcomings of the hypothetico-deductive method can be found in W. Salmon (1984, §30) or W. Salmon (1967, Chap. VII).

5. Samuel E. Gluck (1955) made a brief stab at the task, but it was insufficiently general and failed to take notice of such basic difficulties as the ambiguity of statistical explanation. One interesting feature of his article is the claim that, because of the inherently probabilistic character of physical measurement, even what we take to be D-N explanations in quantitative sciences are actually statistical explanations. This point has been a longstanding source of worry for deductivists.

6. There are, of course, precursors of the theory set forth in this article, including Aristotle and John Stuart Mill. An important twentieth-century precursor is Karl R. Popper, whose 1935 work sketches a version of D-N explanation, though not in the depth of detail of Hempel-Oppenheim. Moreover, Popper's book was not highly influential until the English translation (1959) was published.

The First Decade

1. For one explicit comment, see 1953, p. 347.

2. For a recent detailed and sophisticated discussion of the nature of laws see John Earman (1986, chap. 5).

3. For purposes of this discussion it is not necessary to draw a distinction between counterfactual and subjunctive conditionals. In the example, "If this table salt were placed in water it would dissolve," it does not matter whether or not the particular sample is at some future time placed in water.

4. I once made a very crude estimate of the amount of gold in the earth's seas, and it came out to be more than 1,000,000 kg; if sufficient resources were devoted to the project, somewhat more than 100,000 kg could be extracted from sea water and fashioned into a sphere.

5. It has been speculated that tachyons—particles that travel faster than light—exist, but on pain of contradiction with special relativity, they cannot be used to send messages.

6. In these remarks about the coextensiveness of laws and statements that support counterfactuals I am, of course, excluding counterfactuals based on logical truths and/or on definitions.

7. This may represent Braithwaite's fundamental view of the problem.

8. This point is closely related to the pragmatic view of John Stuart Mill, Charles Saunders Peirce, and David Lewis, according to which the laws are those generalizations that end up as fundamental principles in the ideal limit of scientific investigation. One crucial question with respect to this approach is whether ideal science must eventuate in a unique final form, or whether alternatives are possible. If alternatives are possible, this pragmatic resolution of the problem would not pick out a unique set of laws, for the future course of science is unpredictable. Consequently, the distinction between laws and nonlaws would be basically epistemic; the distinction would not be objective.

9. See the concept of resiliency in Brian Skyrms (1980).

10. A great deal of light will be shed on this issue by Bas van Fraassen's forthcoming book, *Laws and Symmetries*.

11. At this point in his discussion he is advancing this view for consideration, rather than asserting it, but this is precisely the conclusion he does draw at the end of this chapter, pp. 317–18.

12. Partly because of its somewhat opaque title and partly because of its formidable complexity this book received little attention. It was later reprinted under the more descriptive title, *Laws, Modalities, and Counterfactuals* (Berkeley & Los Angeles: University of California Press, 1976). In my foreword to this volume I attempted to survey the issues as they stood in 1976, and to provide a more accessible account of Reichenbach's major ideas.

13. Ernest Nagel, *The Structure of Science* (New York: Harcourt, Brace and World, 1961), Chap. 4.

14. This discussion was continued in (Goodman, 1947) and (Carnap, 1947a).

15. I am paraphrasing the Hempel–Openheim definitions, but I shall preserve their original numbering.

16. See W. Salmon, foreword, in Reichenbach (1947), pp. xxxii–xxxiii.

17. The expression "iff" is a standard abbreviation for "if and only if."

18. For the sake of a concrete interpretation of this counterexample, it will do no harm to restrict the range of our variables to humans.

19. In *no* sense is this remark intended as a historical comment; I do *not* mean to suggest that they were on the verge of sending it off for publication and spotted the problem just in the nick of time. I am merely drawing a parallel between this technical problem and that pointed out by Eberle, Kaplan, and Montague.

20. By the *conjunctive normal form* of a formula we mean an equivalent formula (containing just those sentential variables occurring essentially in the original) which is a conjunction each of whose terms is a disjunction of sentential variables or their negations.

21. It is a serious terminological error, I believe, to refer to the D-N model of scientific explanation as "*the* covering law model," for although it is one model that conforms to the general idea that explanation always involves subsumption under laws, it is by no means the only such model. Indeed, Hempel's I-S model is also a covering law model.

22. In conversation, when he visited the workshop at the Minnesota Center for Philosophy of Science.

23. It appears that Hempel published nothing on scientific explanation, beyond the Hempel-Oppenheim article, during this first decade of our chronicle.

24. For purposes of historical accuracy it should be noted that this chapter is based upon Braithwaite's 1946 presidential address to the Aristotelian Society. At about the same time Ernest Nagel (1956) produced an important study of functional explanation. This paper was prepared in 1953 as a reserach report at Columbia University. Neither Braithwaite nor Nagel makes any reference to the Hempel-Oppenheim paper in their discussions of teleological or functional explanation.

25. Like Braithwaite and Nagel, Scheffler also makes no mention of the Hempel-Oppenheim article. It might be remarked facetiously that if Scheffler had been sufficiently attuned to the historical situation he would have published this paper on teleology in 1957, and the 1957 critique of Hempel and Oppenheim in 1958.

26. Both Hempel and Nagel rely heavily on Merton's extensive discussion of *functional analysis*. Nagel's work (1956) contains a detailed analysis of Merton's treatment of this topic.

27. Although the first published version of the I-S model did not appear until three years later, it seems evident that Hempel had a pretty clear idea of what it would be like when he wrote the 1959 article.

28. Scriven (1959) would, I believe, be one example.

29. See, however, Scheffler (1957) for a discussion of the bearing of the referential opacity of intentional contexts in this connection.

30. Nagel (1961, chap. 1) lists four types of explanation: deductive, probabilistic, functional or teleological, and genetic, leaving open at that stage whether the latter two can be reduced to one or both of the former two.

31. John Canfield (1966) covers the discussion of functional explanation during a good deal of the first two decades of our story. It contains, in addition to classic papers by important authors, a clear and perceptive introduction by Canfield.

32. Wright (1973) contains important anticipations of the theory set out in his book.

The Second Decade

1. As we noted in §0.2, the most serious attempt before 1962 seems to be (Gluck, 1955), but it is much too sketchy to present a clearly articulated theory, and it fails to notice the serious difficulties involved in statistical explanation.

2. His book *The Philosophy of Science* (1953) contains many references to scientific explanation, but does not provide an explicit account of that concept. His *Foresight and Understanding* (1961) deos offer an explicit account.

3. In *Scientific Explanation* (1984, p. 130) I incorrectly attributed to Bromberger the view that all explanations can be appropriately requested by means of why-questions. I regret this error.

4. [Bromberger's footnote] 'Can think of no expression' and 'can think of no answer' as short for 'can imagine nothing, conjure up nothing, invent nothing, remember nothing, conceive nothing' . . . does not cover the familiar states of momentary amnesia during which one has the answer 'on the tip of one's tongue' but cannot utter it.

5. Broberger's footnote: "As an achievement term 'to explain' is also often used to credit people with certain scientific discoveries. 'Newton explained why the tides vary with the phases of the moon' may serve to mark the fact that Newton was the one who solved the riddle, who found the answer to a question with regard to which everybody had been in either a p-predicament or a b-predicament. To have explained something in this sense is to be one of the first to have explained it in the [previous] sense . . . to be one of the first to have been in a position to explain . . . it to a tutee."

6. See the charming dedication to Hempel under "NOTES" on p. 107.

7. These restrictions are contained in the definition of *general abnormic law* given in Bromberger (1966, p. 98).

8. For technical reasons an additional restriction—(4) the general rule completed by L has the property that if one of the conjuncts in the antecedent is dropped the new general rule cannot be completed by an abnormic law—is needed, but the main thrust if this account can be appreciated without it.

9. As Broberger notes explicitly, "unless" has to be contrued as the exclusive disjunction.

10. However, see Grünbaum (1963), Fetzer (1974), and Rescher (1963).

11. It should be recalled, from our discussion of functional or teleological explanation in §1.3, that Hempel did not base his qualms about explanations of these sorts on the problem of the temporal relation between the function and its goal.

12. I do not believe Bromberger ever published this precise example; his actual examples, which have the same import, are the height of a tower and the height of a utility pole to which a guy wire is attached.

13. Bas van Fraassen is an exception. In his 1980 work (pp. 132–34) he suggests that there are possible contexts in which such an explanation would be legitimate. We shall discuss his theory of explanation in §4.4 below.

14. This 'law' does, of course, make reference to particular entitites—earth, sun, and moon—but that, in itself, is not too damaging to the example. After all, in this respect it is just like Kepler's laws of planetary motion and Galileo's law of falling bodies. Like these, it qualifies as a derivative law, though not a fundamental law.

15. Scheffler (1957) subjected the symmetry thesis to searching criticism. To the best of my knowledge, this article is the first significant published critique of the Hempel-Oppenheim article.

16. See Grünbaum (1963) for a fuller discussion of this point.

17. As Philip Kitcher pointed out to me in a personal communication, Hempel could be defended against this example by arguing that "a *natural selection* explanation of the presence of a trait is really a deduction that the probability that the trait becomes fixed in a finite population is high."

18. This example is due to Henry Kyburg (1965). A variant of this example (due to Noretta Koertge) has unhexed table salt placed in holy water; and the 'explanation' of the dissolving is that the water was blessed and whenever table salt is placed in holy water it dissolves.

19. I offered one such method, based on Reichenbach's treatment of laws (Salmon, 1979).

20. In the introduction to this essay I noted that an earlier paper on statistical explanation by Samuel E. Gluck may qualify as a precursor.

21. I shall us the term "statistical law" to refer to factual generalizations, such as the chance of getting 6 if a standard die is tossed, or the probability that a carbon-24 nucleus will decay in 5730 years. One could say that statistical laws are empirical generalizations, provided it is clearly understood that this does *not* mean that only directly observable properties are involved. I shall use the term "law of probability" to refer to axioms and theorems of the mathematical calculus of probability. Such laws are not empirical and they do not have factual content.

22. I am not objecting to the use of idealizing simplifications in dealing with philosophical problems, but their limitations must be kept in mind.

23. Recalling the fact that, for the limiting frequency interpretation, the individuals have to be taken in some specific order, I must confess to a bit of queasiness here.

24. We shall find reasons below for doubting this assessment of the relative importance of D-S and I-S explanations.

25. Whether this statistical law asserts that r is the actual fraction of Fs within a finite class F, or the limiting frequency of Fs within an infinite class F, is an issue that need not concern us here.

26. If we are thinking of probabilities as limiting frequencies in infinite sequences, $P(G|F)$ may equal 1 and $P(G|F.H)$ may equal 0.

27. Hempel (1962) succumbed to this temptation, but he soon realized the essential difficulty, as he explains in (1965, p. 401, note 21).

28. I have slightly modified Hempel's notation, but not in any way that affects our discussion.

29. As we noted in discussing the precise Hempel-Oppenheim explication, the actual requirement for D-N explanation is that the explanans contain essentially at least one theory, where a theory may contain existential quantifiers and need not contain any universal quantifier. In the technical explication of a potential explanans, the explanatory theory must be true (since all theories, in the special sense of Hempel and Oppenheim, are, by definition, true). It would seem more reasonable to require only that the general premise be essentially generalized, for certainly we want to consider the explanatory power of theories (in the usual sense) that need not be true.

30. As we noted above, p. 16, although Braithwaite's *Scientific Explanation* contains a chapter devoted to explanations of laws, he does not come to grips with the fundamental problem noted by Hempel and Oppenheim.

31. Hempel published a revised version of RMS (1968), pp. 116–33, which he designated "RMS*." It embodies some technical revisions designed to overcome an objection by Richard Grandy, but these do not affect any of the philosophical issues we are discussing.

32. Linus Pauling's claims in this regard were receiving a good deal of publicity in the early 1960s (see Pauling, 1970).

33. I owe this example to the eminent geneticist Tracy Sonneborn. In a social conversation about hormones and sex, I asked him what would happen to a man who took oral contraceptives. Without a monent's hesitation he replied, "Well, he wouldn't get pregnant."

I am indebted to the eminent archaeologist William A. Longacre for furnishing further information regarding this question. He kindly sent me a copy of the 2 June 1987 issue of the tabloid *Sun* (vol. 5,

no. 22) which carried the front page headline "WIFE FEEDS HUBBY BIRTH CONTROL PILLS TO STOP HIM CHEATING." According to the story (p. 35), "Determined to stop her husband from fooling around, a betrayed housewife came up with the idea of feeding him birth control pills—and the dumbfounded hubby was frightened out of his wits when the pills made him impotent." When she confessed that she had been concealing them in his food he became furious and "stormed out of the house and has since filed for divorce . . . on the grounds of mental and physical cruelty."

The Third Decade

1. It is worth noting that Hempel introduces the term "statistically relevant" in this article, but he does not use it to refer to a statistical relevance relation (as that concept is generally understood).

2. As the issue was addressed here, it was buried rather deep in a paper whose title gave hardly any hint that it dealt with this topic.

3. This paper is a much expanded and highly revised version of one presented at a University of Pittsburgh workshop in 1965.

4. I discuss these three conceptions—the epistemic, the modal, and the ontic—rather briefly in (1982, 1986); and also in greater detail in (1984, chaps. 1 and 4). We shall return to them in this essay when we get to the fourth decade.

5. The answer is obvious in the sense of Christopher Columbus. The story is told of a dinner attended by Columbus sometime after his first voyage to the new world. Some of the guests were belittling his accomplishment by suggesting that it was not all that difficult. Columbus requested an egg from the kitchen, which he passed around asking each guest to try to stand it on end. When all had failed and it came back to him, he gave it a sharp tap on the table, breaking in the shell at one end, whereupon it remained upright. "You see," he said, "it is easy after you have been shown how."

6. I have in mind chiefly Peter Railton and myself.

7. Suppose, for example, that we have a class of tosses consisting of tosses of many different coins. Suppose further that many of the coins are biased, some toward heads, others toward tails. Some of the coins are fair. Now if each of the biased coins is biased in the same degree toward heads or tails (as the case may be), if as many are biased toward heads as toward tails, and all of the coins are tossed equally often, then the probability of heads in the entire class will be ½. This class may be relevantly partitioned into three cells—tosses with coins biased toward heads, tosses with coins biased toward tails, and tosses of fair coins. The probability of heads in the third cell is equal to the probability of heads in the original class, but that does not mean that the partition is not relevant.

8. I am modifying the example somewhat for purposes of the present discussion.

9. These arguments were also contained in his doctoral dissertation, *Foundations of Inductive Explanation* (1973).

10. The treatment of the topic in this article was seriously flawed, and it is completely superseded by my 1984 work, (chap. 3).

11. We might roughly define violet light as that having wave-lengths in the range of approximately 3600–3900 Å and red light as that having wave-lengths in the range of approximately 6500–7100 Å. It is a *fact* that the normal human range of vision extends from about 3600 Å to about 7100 Å.

12. The addition to Hempel's RMS is italicized.

13. In developing the statistical-relevance model of scientific explanation, I employ a non-epistemically-relativized counterpart of RMS. It is the requirement that reference classes used in explanations of this sort be *objectively homogeneous*. A number of the considerations that enter into the foregoing revision of Hempel's RMS were developed in the attempt to provide an adequate characterization of objective homogeneity. See W. Salmon (1984, chap. 3).

14. This attempt was seriously flawed in several ways. It is completely superseded by my 1984 work (chap. 3).

15. The members of F must be taken in some order. If F is finite the order makes no difference; if F is infinite, choose some natural order, such as the temporal order in which the events occur or are discovered.

16. This requirement is analogous to Richard von Mises's principle of insensitivity to place selections in his definition of the *collective*.

17. Philip Kitcher criticizes this proposal as follows, "I suspect that the omnipresence of correlations will make for inhomogeneity everywhere. This [proposal] will not get around the spurious correlation problem. There's bound to be some (possibly hokey) property that all correlates of the Bs have." If he is right, there is more work to be done on the concept of objective homogeneity. One suggestion, essentially adopted by Coffa, is to restrict consideration to nomically relevant (not just statistically relevant) factors.

18. This 'law' needs more careful formulation. Innumerable automotive and plumbing problems have been occasioned by the fact that water expands when it is cooled.

19. I have treated these issues in some detail (1979).

20. This is a highly contentious issue. In a personal communication Philip Kitcher comments, "Here I think you are describing a different chance setup. It depends how far you go back in the causal chain. If the setup involves the producing machine, then the Laplacian description seems right. If we start with some already selected coin of unknown constitution, then it seems that [the other] is right. We don't have an ambiguous description *of the same situation.*" My answer to this ploy is that we have two different descriptions of two different *types* of chance setups. The event in question actually belongs to both. If we want to characterize *this chance setup*, we have to decide which type to assign it to.

In another personal communication Paul Humphreys writes, "Your argument here applies only to long-run propensity interpretations, and not to single case propensities. Under the single case account, the continued operation of the machine is irrelevant to each specific trial, and under either experiment you describe, the propensity will be either less than or greater than ½, although in the first experiment it will remain fixed, whereas in the second experiment it will change depending on which kind of disk is picked on each trial." My answer to Humphreys is that he has done what any good limiting frequency or long-run propensity theorist would do. He has picked the appropriate type of chance setup to characterize *this trial*.

21. Because Coffa did not develop the idea or spell out any details, I had completely missed the point and forgotten all about it until I very recently reread his disseration. Every important philosophical discovery has to have a precursor; Coffa plays that role for Humphreys.

22. Hempel's response to Rescher can be found in (Hempel, 1965, pp. 403–6).

23. This book contains a comprehensive bibliography of works on scientific explanation up to its date of publication.

24. Indeed, any but the simplest putative Hempelian explanation of an individual event, because it involves subsumption under one or more laws, can be construed as an explanation of a regularity. The explanation of the bursting radiator, for example, is an explanation of why *any* radiator of a similar type, subjected to similar conditions, will burst.

25. Kicher responds, of course, that the Hempel-Oppenheim criteria are too weak. Where they required merely that the primitive predicates be purely qualitative, Kicher seems to want to require that they signifiy natural kinds.

26. Kitcher will argue, of course that typically not all of the Hs will represent predicates that are projectable from their instances.

27. Kitcher responds, "I'm not sure you could project either generalization without having a background theory about organisms that would give you both at once—or that would tell you that one is true if and only if the other is."

28. In a personal communication Kitcher also rejects this example: "I'm not sure we could accept any of these without accepting them all. The reason is that the projection from a finite sample seems to depend on believing that the relevant predicates pick out crucial classes in nature. There's no basis for thinking of the large-small cases, for example, as a privileged class for projection without thinking that the class of *all* two-body systems is projectable. Sometimes, I believe, you can't make a *restricted* projection. You either go all the way or nowhere."

29. Philip Kitcher, "Explanation, Conjunction, and Unification," *Journal of Philosophy* LXXIII (1976), p. 209.

30. In a personal communication Kitcher reponds: "Not necessarily, if the conditions on acceptability concern the projectability of predicates." Kitcher's remarks about projectability in response to my criticism of Friedman are interesting and important. If, however, this is the avenue to salvation for Friedman's program, he clearly needs to add a theory of projectability to the proposal he has offered.

31. A.A. Few, "Thunder," *Scientific American* (July, 1975), pp. 80–90. [Kitcher's reference.]

32. The first two dogmas were elaborated in W. V. Quine's classic essay "Two Dogmas of Empiricism," (1951).

33. This is the position Jeffrey took (1969); however, I do not agree with his way of making the distinction between those that are and those that are not.

34. If these temporal relations are not obvious, think of a given photon approaching the vicinity of the flagpole. If it passes by the flagpole without being absorbed, it reaches the ground a little later than it passed by the flagpole. A companion photon, traveling alongside of the above-mentioned one in a parallel path that intersects with the flagpole, will be absorbed a little before the other reaches the ground. As a very rough rule of thmb, the speed of light is a billion ft/sec; the photons travel about one foot per nanosecond.

35. I offered one suggestion on how this could be done in "Postscript: Laws in Deductive-Nomological Explanation—An Application of the Theory of Nomological Statements" (1979).

36. Moves of this sort were made in Fetzer (1981).

37. Carnap argues in careful detail for his denial of rules of acceptance (1950, §44b, 50–51). We need not go into his reasons for this approach here, for we are concerned only with its consequences in this context.

38. It has been noted that the special theory of relativity, while prohibiting the acceleration of ordinary material particles from speeds below that of light to superluminal speeds, may not preclude the existence of particles—called *tachyons*—that always travel faster than light. It is generally agreed, I believe, that there is, at present, no empirical evidence for their existence. Moreover, the presumption is that, if they should exist, they would be incapable of serving as signals or transmitting information. See my 1975 work (2nd ed., 1980, pp. 105, 122–24) for a discussion of the problems that would arise if tachyons actually existed.

39. Many philosophers would analyze processes as continuous series of events. While this can surely be done, there is, in my opinion, no particular reason for doing so,and there are some significant disadvantages. See my 1984 work (pp. 140–41, 156–57) for a discussion of this issue. See also John Venn's nice remark quoted on p. 183.

40. A material object that is at rest in one frame of reference will, of course, be in motion in other frames of reference.

41. In speaking of interventions that produce marks, I do *not* mean to suggest that such occurrences must be a result of human agency. For example, light from a distant star may be marked as a result of passage through a cloud of interstellar gas that selectively absorbs light of certain frequencies while letting others pass through unaffected.

42. The pseudo-process consisting of the moving spot of light can be changed from white to red from some point onward by putting a red lens on the beacon, but that does not qualify as an intervention *in the pseudo-process* because it is done elsewhere.

43. I often put so-called scare-quotes around the word "process" when it is being used to refer to pseudo-processes, for many people might be inclined to withhold that term when they realize that the process in question is pseudo. Reichenbach called them "unreal sequences."

44. The Lone Ranger's horse Silver was described as "A fiery horse with the speed of light." For the image of the horse on the screen that is possible in principle.

45. In the infinitesimal calculus we do, to be sure, define the concept of *instantaneous velocity*, but that definition requires consideration of the position of the object at neighboring instants of time. See my 1970 work, which contains Russell's article on Zeno's Paradoxes, and my 1975 work (chap. 2) for a fuller account.

46. Because of my own disciplinary limitations I have not participated in this field.

47. Perhaps I should add, or a bit of teleological non-action, such as not removing a tree that just happened to grow where it did, because it provides some wanted shade.

48. I do not find Nagel's criticisms of Wright particularly weighty.

49. These include Andrew Woodfield (1976) and Michael Ruse (1973).

50. It should be recalled, as previously mentioned, that Hempel addressed the problem of functional explanation in such areas as anthropology, sociology, and psychology, as well as biology. Nagel confines his attention, in these lectures, to biology. It is *much* harder to argue against functional equivalents in these other fields than it is in biology.

The Fourth Decade

1. I have argued this issue briefly (1967, pp. 729–32), and in greater detail (1969).

2. Coffa had previously provided searching critiques of both of these models in his doctoral disseration (1973).

3. This theory was also presented in his doctoral dissertation; see §3.3 above.

4. Before his 1977 publication at any rate. As I remarked above, it is not clear how the I-S model can survive abandonment of the high-probability requirement.

5. I discussed these general conceptions at length (1984, chaps. 1 and 4), and more briefly (1982, 1985).

6. I have no qualms about considering "micrometer" a term of the observational vocabulary, for it is defined in terms of "meter," which in view of the International Prototype and zillions of meter sticks in homes, stores, laboratories, and shops throughout the world, is surely an observational term.

7. Perrin actually studied the Brownian movements of particles suspended in liquids, but that fact does not affect the final results as applied to gases.

8. In offering this response on behalf of the modal conception I am, of course, assuming that knowledge of physical necessity and impossibility are not fully contained within our descriptive knowledge of the world. I shall return to this issue shortly.

9. I take it that predictive knowledge qualifies directly as part of descriptive knowledge, for knowledge of what is going to happen in the future is surely included in descriptive knowledge of the world.

10. See J. L. Mackie (1974) for detailed discussion of this approach. Mackie, himself, does not adopt it.

11. See, for example, Carnap (1966, 1974) for an exposition of this view of causality in the context of scientific explanation.

12. See David Lewis, *Counterfactuals* (1973). As I mentioned above, Lewis himself attemps to break out of the circle by adopting the view that laws are the basic principles in an ideally complete science. In my foregoing remarks on this topic, it will be recalled, I raised the crucial question of the uniqueness of characterization—a problem Lewis acknowledges.

13. Some physicists have proposed the "many-worlds" interpretation of quantum mechanics to resolve the problem of measurement (see De Witt and Graham, 1973). I am not favorably incluined toward this interpretation for much the same reasons.

14. It should be recalled that the term "theory," as it occures in the Hempel-Oppenheim formal explication, does not involve any appeal to unobservables. It simply refers to general statements that may contain existential as well as universal quantifiers.

15. I shall dicuss the legitimacy of this claim to empirical knowledge of unobservables in §4.5.

16. To those who suggested that such feelings are observable, at least by the subject, they responded that introspection is not a scientifically acceptable kind of observation.

17. I have argued this point most explicitly in my 1985 publication; it is also discussed in my 1984 work (pp. 229–38).

18. See my *Scientific Explanation* (pp. 13–15) for a striking historical example.

19. There are, of course, unsatisfactory answers to questions, but I would suppose they should be called unsatisfactory explanations. Not all explanations are good explanations.

20. See Frolich (1980), especially the photographs on p. 154.

21. Indeed, the only detailed critiques of van Fraassen's treatment of explanation so far published of which I am aware are in Salmon (1984) and Kitcher and Salmon (1987). The criticisms expressed in my work are entirely different from those given in Kitcher and Salmon. Achinstein (1983) gives brief attention to van Fraassen's theory of explanation.

22. For van Fraassen's response to this argument, as well as to mine, see Churchland and Hooker (1985, pp. 297–300).

23. A rather good sample of the range of opinion can be found in (Churchland and Hooker, 1985) and in (Leplin, 1984). Essays by Fine, Laudan, Boyd, and McMullin can be found in the Leplin volume.

24. In his useful discussion, "Structural Explanation" (1978, pp. 139–47), Ernan McMullin stresses the causal foundations of structural explanations, but he too steers clear of quantum phenomena.

25. It is ideal in the sense not of something we should necessarily strive to realize, but, rather, of (a Platonic?) something that may not exist in the physical world. It is the sort of thing Laplace's demon might be able to realize, if it concerned itself with explanations.

26. Ironically, the published article contains a misprint, which I have corrected, in the first two words of this passage. Instead of "Is it" the original reads "It is." Has Freud struck yet again?

27. I suspect Railton might agree (see Railton, 1981, pp. 243–44).

28. At the close of §3 of his paper in this volume (Kitcher & Salmon 1989), Kitcher offers his critique of Railton and me on the issue of the role of mechanisms in explanations.

29. This was reportedly the terse recommendation of this famous architect when, many years ago, he was brought to Pittsburgh by the city fathers to give advice on what could be done to improve the city.

30. This material is incorporated, to a large degree, in Fetzer (1981). Nute was Fetzer's collaborator on the formal aspects of probabilistic causality, but not on the general theory of scientific explanation.

31. A probability of one is not equated with a universal disposition and a probability of zero is not equated with a universal negative disposition.

32. One would expect the stronger result, that p and not-q are jointly possible, to be derivable.

33. As we saw in §3.2, however, Hempel's doctrine of essential epistemic relativization of inductive-statistical explanation brought him dangerously close to the brink of deductivism.

34. I discuss this example and others in some detail (1984, pp. 111–20).

35. Railton remarks, incidentally, that he is not strongly opposed to discarding the parenthetic addendum (1981, p. 236). He remians, however, strongly opposed to the view that explanations are always arguments.

36. Popper (1959, pp. 198–205) offers a criterion for deciding when the number of micro-events is large enough to justify this kind of replacement; Watkins employs the same criterion in his account.

37. I have discussed the status of Principle I, and other closely related principles (1984, especially pp. 111–20).

Conclusion

1. It should not be supposed that all mechanical explanations appeal to unobservable mechanisms. One might explain the workings of some gadget solely in terms of observables to someone who had not noticed the mechanical relationships among them—e.g., the way in which squeezing the handbrake on a bicycle brings the bicycle to a stop. However, deeper scientific explanations do seem usually to invoke unobservables.

2. Gregory Cooper, who was an active participant in the Minnesota Workshop, independently recognized the compatability of these two approaches earlier than I did. His thought on this matter is contained in his doctoral disseration. I am happy to acknowledge his priority with respect to this point.

3. This little story was previously published (W. Salmon, 1981 pp. 115–25). I did not offer an explanation of the phenomenon in that article.

4. Objects denser than air do not move toward the front of the cabin because the pressure difference is insufficient to overcome their inertia.

5. Albert Einstein et al., "Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?" *Physical Review* 47 (1935), pp. 777–80. In *The Book of Revelations* 13:18 it is said that the number of The Beast is 666; I believe it is 777.

6. N. David Mermin, "Is the Moon Really There When No One Is Looking? Quantum Theory and Reality," *Physics Today* (1985).

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