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The Spirit of Logical Empiricism: Carl G. Hempel's Role in Twentieth- Century Philosophy of Science*

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In this paper, I discuss the key role played by Carl G. Hempel's work on theoretical realism and scientific explanation in effecting a crucial philosophical transition between the beginning and the end of the twentieth century. At the beginning of the century, the dominant view was that science is incapable of furnishing explanations of natural phenomena; at the end, explanation is widely viewed as an important, if not the primary, goal of science. In addition to its intellectual benefits, this transition has important practical consequences with respect to dealing with the global problems humans everywhere will face in the twenty-first century.

To say that we live in a post-positivist age has been a cliché for decades, often uttered by those who have no understanding of the difference between the logical *positivism* of the Vienna Circle and logical *empiricism*, which originated in Berlin and completely superseded positivism in the second half of the twentieth century. Logical positivism is dead, but logical empiricism, I believe, is still a vital force in philosophy of science. By the middle of the century, logical empiricism had three great leaders, Rudolf Carnap, Carl G. Hempel, and Hans Reichenbach. This essay, which is dedicated to the memory of Hempel, emphasizes his influence on contemporary philosophy of science.

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1. From Positivism to Empiricism.¹ Carnap's *Der Logische Aufbau der Welt* (1928) can be considered the pinnacle of logical positivism. Taking as his point of departure Bertrand Russell's "supreme maxim of scientific philosophizing," namely, "Whenever possible, logical constructions should be substituted for inferred entities," Carnap mustered enormous ingenuity in his attempt to carry out the construction of the world in terms of private experiences. However, Nelson Goodman's critique in *The Structure of Appearance* (1951) showed convincingly that Carnap's endeavor was hopelessly flawed. Carnap's attempted construction was a magnificent failure. The philosophical labors of Carnap and Goodman did us the invaluable service of showing the futility of the phenomenalist approach in epistemology.

Long before Goodman's critique, Reichenbach focused his attention on Carnap's *Aufbau*. In an otherwise laudatory review, Reichenbach (1933) complained only that he saw no place for probability in Carnap's approach. Reichenbach's *Experience and Prediction* (1938), which sought to fill that lacuna, can be taken as the first major manifesto of logical empiricism. In this book, Reichenbach invoked probabilistic considerations to make three main points. First, he rejected phenomenism as an analysis of human knowledge, adopting instead a physicalistic approach in which our knowledge is based upon our admittedly corrigible observations of middle-sized material objects. Thus, Reichenbach explicitly abandoned the quest for certainty that motivated phenomenism. He denied C. I. Lewis's dictum that, "if anything is to be probable, something must be certain" (1946, 186). He maintained instead that corrigible observation and inductive reasoning yield probabilistic knowledge of the world. Second, like the positivists, Reichenbach advocated a criterion of empirical meaningfulness. Unlike the positivists, however, he required only the physical possibility of positive or negative empirical *probabilistic* evidence, not the possibility of *complete* confirmation or refutation. Third, Reichenbach supported scientific realism, arguing that we can have probabilistic knowledge of unobservable entities.

Reichenbach's *Experience and Prediction* was programmatic; he did not work out the details. To be sure, he had given a much deeper treatment of probability in his *Wahrscheinlichkeitslehre* (1935), but in neither of these books did he show how probabilities could employ uncertain evidence. Nor was this gap filled in *The Theory of Probability* (1949), the enlarged English edition of the 1935 treatise on probability. Moreover, when Carnap turned his attention to probability in the

1. Clearly the transition from logical positivism to logical empiricism is a complex historical matter, but a few central points are essential to our story.

1940s, the confirmation theory he developed also presupposed evidence statements that are ‘given’, i.e., no probability values could be attached to them. To the best of my knowledge, Richard Jeffrey (1965, Ch. 11) was the first philosopher to show precisely how this obstacle could be surmounted. Be that as it may, by 1950 it was clear that the concept of probability was both indispensable and highly problematic.

2. Reichenbach and Carnap on Scientific Realism. Reichenbach’s argument for scientific realism in *Experience and Prediction* consisted of an analogy—his ‘cubical world’ in which a new ‘Copernicus’ infers the existence of unobservable birds from bird shadows—along with a passing reference to Bayes’s theorem. If one turns to *The Theory of Probability* (which contains material on this topic not contained in *Wahrscheinlichkeitslehre*), one finds an altogether unilluminating explanation of how Bayes’s theorem is supposed to enable us to assign probabilities to theories. It seems to me that his argument becomes plausible only by invoking a common cause principle of the sort he later elaborated in *The Direction of Time* (1956), but many gaps need to be filled. His untimely death in 1953 prevented him from elaborating many important themes found in this posthumous publication.

Carnap expounded his views on realism in his justly famous 1950 paper, “Empiricism, Semantics, and Ontology.” Although this essay deals mainly with questions of existence arising in such fields as mathematics, set theory, and formal semantics, it contains brief references to the same sorts of questions in the empirical sciences. From these remarks we can discern his position on scientific realism.² His thesis is that there are two kinds of existence questions, internal and external. Consider some particular linguistic framework, such as Peano arithmetic. If, having adopted it, we ask whether an even prime number exists, this is an internal question to which we can readily give an unambiguous affirmative answer. If, in contrast, we ask whether natural numbers ‘really’ exist, and thereby question the legitimacy of adopting Peano arithmetic, we are asking an external metaphysical question to which no meaningful answer can be given. According to Carnap, the only legitimate external question that can be raised is whether the framework is a useful one for doing its job, namely, as a foundation for mathematics. This is a pragmatic question, and its answer has no ontological import. The same sorts of considerations apply to linguistic frameworks for empirical science. If we adopt the language of modern

2. Although he made some confusing statements on this issue in later works, he never actually changed his mind. See Parrini 1994 and my comment immediately following his paper.

atomic physics, then we can answer the question of whether electrons exist in the affirmative without difficulty.

Carnap seems to offer us a choice when it comes to the adoption of a framework for empirical science, for instance, phenomenalism, physicalism (somewhat similar to Bas van Fraassen's (1980) constructive empiricism), or theoretical realism. If we adopt a realistic framework, within which the affirmative answer to the question of the existence of electrons is available, then the only meaningful external question is whether this framework is well-adapted to the pursuit of theoretical physics. If, however, we adopt a linguistic framework in which only observables can be said to exist, the question of the existence of electrons receives a negative internal answer. The only meaningful external questions pertain to the utility of the framework itself, not to the question of whether electrons 'really' exist:

3. Hempel on Scientific Realism. Now, finally, the hero of this story enters the scene.³ In 1958, Carl G. Hempel published an epoch-making paper, "The Theoretician's Dilemma: A Study in the Logic of Theory Construction." To characterize the purpose of scientific theorizing, he introduces the term "systematization," which is construed broadly enough to cover at least prediction and explanation. He poses the following puzzle:

If the terms and principles of a theory serve their purpose they are unnecessary, as just pointed out; and if they do not serve their purpose they are surely unnecessary. But given any theory, its terms and principles either serve their purpose or they do not. Hence, the terms and principles of any theory are unnecessary. (1965, 186)

He establishes the first horn of this dilemma by means of an elementary logical argument showing that if, through the use of a theory, deductive connections between one set of observables and another can be established (as, for example, in making predictions of observable facts), then it is always possible to invoke a direct relationship among observables without making use of any theoretical terms—i.e., terms putatively referring to unobservables. This is not to deny for one moment the heuristic value of theories; it is a question of whether there is any logical necessity of appealing to a theoretical vocabulary. The second horn of the dilemma is trivial.

3. Obviously, I do not intend to imply that this essay is Hempel's earliest important contribution; it is simply the first piece in the story I am trying to tell.

In the end Hempel argues that there are two types of scientific systematization, deductive and inductive.⁴ For purposes of deductive systematization, he admits, theoretical terms are dispensable, but he maintains that they are indispensable for inductive systematization. Given the pervasive character of inductive (i.e., nondemonstrative) arguments in science, he concludes that theoretical language is essential. Although I may be carrying the argument a bit beyond Hempel's own view, it seems to me that he presents a pretty strong case for saying that, given the indispensability of the theoretical vocabulary, it is reasonable to conclude that theoretical terms denote unobservable entities.⁵

Hempel offered essentially the same argument in his contribution to the volume on Carnap in *The Library of Living Philosophers* (1963). In his response, Carnap accepts Hempel's point about the indispensability of the theoretical vocabulary for inductive systematization (1963, 960). Applying this consideration to Carnap's own position, we may say that the inadequacy of a phenomenalist language was shown by Goodman; Carnap did not try to rehabilitate it. Moreover, he agrees with Hempel that a physicalist language that refers only to observables is inadequate to the purposes of modern science. It would seem that powerful negative arguments have been given to the *external* questions about the adequacy of those languages. That leaves us with the theoretical language as the only adequate alternative. When we ask the *internal* questions about the existence of such unobservables as atoms and electrons, we obviously get the scientific realist's answer. In addition, correctly or incorrectly, Carnap classifies the result of Reichenbach's cubical world argument as an answer to an internal scientific question. Although there are many subtle issues I have not taken up here, there seems to be a high degree of convergence of opinion among the three great leaders of logical empiricism on the doctrine of scientific (theoretical) realism.⁶

4. Hempel's commitment to inductive systematization may have been an important motivating consideration for his attempt to deal with statistical explanation a few years later (Hempel 1962).

5. In drawing this conclusion, I am relying heavily upon three paragraphs in *Aspects*, beginning on the bottom of page 219 and continuing onto page 220. Here, I think, Hempel is expressing his own views. When he continues, in the last paragraph of page 220, to offer alternatives to those who are unwilling to accept his account of the status of sentences in partially interpreted theories, I take him to be suggesting lines of argument (which he ultimately finds unsatisfactory) to those who take a different view of theoretical significance. In these passages he refers to a basic vocabulary V_B , consisting of antecedently understood terms from other theories, as well as the observational vocabulary, but this move involves no difficulties for his fundamental thesis.

6. See Salmon 1994 for a more detailed examination of the views of Carnap, Hempel, and Reichenbach on this issue.

4. Hempel and Oppenheim on Scientific Explanation (1948). As already noted, when Hempel refers to scientific systematization, he explicitly includes both confirmation and explanation.⁷ Carnap and Reichenbach wrote major treatises on probability and confirmation. Although their viewpoints differed enormously, both of them made contributions of striking importance. Hempel also contributed significantly to this subject, but not to the degree of the other two.⁸ Carnap and Reichenbach, however, found little to say about scientific explanation. I have found fruitful suggestions on causality and explanation in Reichenbach's *Direction of Time*, but by no stretch of the imagination could Reichenbach be said to have offered any explicit account of explanation. To the best of my knowledge, Carnap did not contribute constructively to this subject.⁹ This is the area in which Hempel's work is preeminent.

To put things in perspective, let's look at the dominant attitude of scientifically oriented philosophers and philosophically inclined scientists at the beginning of the twentieth century. By and large, they held that there is no such thing as scientific explanation—explanation lies beyond the scope of science, in such realms as metaphysics and theology. Karl Pearson stated it concisely: "Nobody now believes that science *explains* anything; we all look upon it as a shorthand description, as an economy of thought" (1911, xi; emphasis in original). For a sharp contrast, consider the following statement, published in the final decade of this century, by the Nobel laureate physicist Steven Weinberg: "Whether or not the final laws of nature are discovered in our lifetime, it is a great thing for us to carry on the tradition of holding nature up to examination, asking again and again why it is the way it is" (1992, 275). His view on this matter is shared by large numbers of philosophers and scientists. In fact, I cannot think of any contemporary philosopher of science who denies the possibility of scientific explanation. While I cannot claim any comprehensive knowledge of the attitudes of contemporary scientists on this subject, I do feel confident that

7. It is plausible to suppose that Hempel's commitment to explanation as one form of systematization, along with his doctrine that theories explain and explanations must be true, provides a strong motive for his realistic interpretation of theories.

8. It is worth remarking that Clark Glymour's bootstrapping account of confirmation is heavily indebted to Hempel's satisfaction criterion. See Glymour 1980 and Hempel 1945.

9. Carnap (1966, 7) states explicitly that all scientific explanation conforms to the deductive-nomological model, and on the very next page he says that some explanations are statistical and therefore do not conform to the schema offered on the previous page. This error was corrected when the book was reissued under a different title in 1974.

Weinberg speaks for a substantial group.¹⁰ What happened to bring about this remarkable reversal of attitude?

Part of the answer, I think, hinges on an issue I have already touched upon. A large proportion of the philosophers and scientists who, at the beginning of the century, denied the possibility of scientific explanation, also denied the existence of such unobservables as molecules, atoms, and electrons. Pearson warns: “may there not be some danger that the physicist of to-day may treat his electron, as he treated his old unchangeable atom, as a reality of experience, and forget that it is only a construct of his imagination” (1911, xii).

When Hempel wrote the two essays on scientific realism already mentioned, I think that the issue had already been settled, but that philosophers were slow to perceive the import of certain scientific developments. The two crucial and profoundly related events occurred during the first decade of this century. The first is the theoretical explanation of Brownian motion by Albert Einstein and Maryan Smoluchowski; the second is the experimental determination of the value of Avogadro’s number by Jean Perrin. In the first place, Perrin’s work, as Einstein noted with pleasure, was the experimental verification of the Einstein-Smoluchowski theory. In the second place, Avogadro’s number is *the link between the macrocosm and the microcosm*. Given the values of the macroquantities, values of related microquantities can be computed, and vice versa. In the third place, the value of Avogadro’s number established by Perrin on the basis of Brownian motion agreed within experimental error with determinations based on a wide variety of completely distinct physical phenomena.¹¹ Perrin says it beautifully:

Our wonder is aroused at the very remarkable agreement found between values derived from the consideration of such widely different phenomena. Seeing that not only is the same magnitude obtained by each method when the conditions under which it is applied are varied as much as possible, but that the numbers thus established also agree among themselves without discrepancy, for all the methods employed, *the real existence of the molecule is given a probability bordering on certainty*. ([1913] 1923, 215–216; emphasis added)

10. I should emphasize that I am referring only to his views on the importance of scientific explanation, not to his views on the nature of explanation, on the existence of a “final theory,” or on the enormity of abandoning the Superconducting Supercollider (SSC) project. I happen to agree with him on this final matter, but that is beside the point.

11. Just above the following passage, Perrin furnishes a table of thirteen distinct ways of ascertaining Avogadro’s number. The table contains the results of each method.

The philosophical importance of Perrin's work was not widely appreciated until relatively late in the century. Philosophers interested in the issue of scientific realism should consult, in addition to Perrin's *Atoms*, Mary Jo Nye's philosophically sophisticated historical account in her *Molecular Reality* (1972). As I have explained elsewhere (Salmon 1984, 213–227), I find in this scientific development a stronger argument for scientific realism than any philosophical account of which I am aware.

Returning now to scientific explanation, it seems to me that the key development stems from the classic Hempel-Oppenheim essay of 1948, in which what came to be known as the deductive-nomological (D-N) explanation of particular facts was first presented with an unprecedented degree of precision. The authors of this article state explicitly that the account they offer is not novel; they cite a number of nineteenth and twentieth century authors, including John Stuart Mill and Karl R. Popper, as anticipators. However, even though the basic idea is not new, this 1948 article is the fountainhead from which practically all subsequent philosophical work on scientific explanation flowed, either directly or indirectly. My personal historical slant is that philosophers in roughly the first half of the twentieth century who wondered about scientific explanation may have had no clear idea of what it might be. "Explanation" (especially without the qualifier "scientific") seems so vague and ambiguous, with so many subjective overtones, that it is hard to see what sense can be made of it.

Hempel and Oppenheim gave a clear model. It had two stages. In Part I, the preliminary considerations are set forth, including four specific criteria of adequacy. These criteria provided a target. Arguments could be offered attacking or defending any or all. A strange temporal gap appears in the history. For about ten years this article attracted virtually no attention. Then, beginning in the late 1950s a huge literature began to emerge.¹² The crucial point is that virtually nobody argued that there can be no such thing as scientific explanation. They argued, for instance, whether every scientific explanation must include at least one law. They argued about the explanation/prediction symmetry thesis—i.e., whether every scientific explanation could, under suitable circumstances, have been a prediction, and whether every prediction, under suitable circumstances could have been an explanation. They argued whether the statements in the explanandum must be true, or whether high confirmation would be a more appropriate requirement.

The upshot is that, rightly or wrongly, Hempel and Oppenheim gave us a general idea of what a scientific explanation might consist in.

12. Details of the historical developments are given in Salmon 1990a, 11–50.

Maybe they got it wrong, but if so, the aim is to formulate a correct analysis, not to banish the very idea. For reasons I shall go into below, this is, I believe, one of the most significant philosophical achievements of the twentieth century.

We should note with care that Part III, not Part I, contains the precise analysis. Here we find an attempt to characterize lawlike statements and laws of nature. Philosophers the world over are still wrestling with that problem.¹³ Hempel and Oppenheim found that laws are not necessary for explanation; the formal requirement is for theories. It turns out, however, that theories are simply generalizations that may, but need not, contain existential quantifiers. Most of us would have called them laws. So the informal clarification of the explicandum mentions laws, but the formal explication makes matters precise by distinguishing laws from theories in this special sense. This terminological decision has absolutely no bearing on the problem of theoretical realism we have already discussed. Hempel and Oppenheim found (as they explain in footnote 33) that, although the informal clarification of the explicandum discusses explanations of laws as well as explanations of particular facts, the formal explication covers only explanations of particular facts. This resulted from a fundamental logical difficulty Hempel and Oppenheim were unable to overcome, and, to the best of my knowledge, Hempel never returned to that problem.¹⁴

To top this all off, the logicians Rolf Eberle, David Kaplan, and Richard Montague (1961) showed, to put it roughly, that according to the formal explication, almost any law could explain almost any fact. They did this essentially by exploiting the paradoxes of material implication. Then Kaplan (1961)¹⁵ and Jaegwon Kim (1963) showed two different ways in which this formal difficulty could be avoided by means of a little patchwork. To those of us (and there were many) who were enamored of formal logic, these exercises proved extremely exciting.

We can thus see that, for many reasons, the Hempel-Oppenheim 1948 article forced scientific explanation onto the attention of a wide class of logicians and philosophers of science. There was an explicit proposal regarding the nature of scientific explanation on the table,

13. In attempting to clarify the nature of lawlike statements, Hempel and Oppenheim introduce the concept of a purely qualitative predicate. This concept arose in Carnap's earlier attempt to resolve Goodman's "grue-bleen" paradox. Fortunately, we do not have long to wait until we find out whether emeralds are grue or green.

14. A 1974 attempt by Michael Friedman to circumvent the difficulty was unsuccessful (Salmon 1990a, §3.5).

15. This paper follows immediately after Eberle et al. in the same issue of the same journal.

and it challenged philosophers to respond either positively or negatively. It elicited alternative analyses. The temptation to say that there is no such thing as scientific explanation seems to have vanished.

5. Hempel on Scientific Explanation (1965). The largest piece of business left unfinished in the 1948 essay was the characterization of statistical explanation. In a 1962 essay, Hempel dealt at length with this topic, but, dissatisfied with the results, he reexamined the whole subject of scientific explanation—including many of the criticisms that had been leveled against the 1948 account. The fruit of his further work was a monographic essay, “Aspects of Scientific Explanation,” which appeared in his 1965 book, *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science*. Here, in a large and detailed section (§3), he laid out two models of statistical explanation, namely, the *deductive-statistical* (D-S), in which statistical generalizations are explained by derivation from other statistical laws, and the *inductive-statistical* (I-S), in which particular facts are explained by subsumption under statistical laws. It seems to me that there is no important distinction between the two kinds of explanations of laws, be they explanations of universal laws (D-N) or statistical laws (D-S). The problem that precluded explanations of generalizations in the 1948 paper is inherited by the D-S model. However, Hempel explicitly expressed his view that I-S explanation is more important than D-S explanation, and he devoted a great deal more attention to I-S explanation.¹⁶

It is simple to say that the I-S model is analogous to the D-N model of explanation of particular facts, in that each is an argument that must exhibit correct logical form, each explanans must contain a law statement essentially, and the explanans must be true. The difference is that one argument is deductive, while the other is inductive because of its use of a statistical law.¹⁷ Hempel realized that this difference raised philosophical problems of the most serious sort. They arise because inductive arguments, in contrast to deductive arguments, are not erosion-proof.¹⁸ This means that a valid deductive argument remains valid (its validity cannot be eroded) by the addition of new premises

16. More recently, a number of philosophers have claimed that I-S explanations do not exist. They hold that only statistical regularities, not individual events, are amenable statistical explanation. For details, see Salmon 1988.

17. In the deductive case, the truth of the explanans entails the truth of the explanandum, whereas, in the inductive case, no such guarantee exists. Nevertheless, we presuppose the truth of the explanandum, since we do not try to explain facts that do not obtain.

18. Logicians characterize the difference by calling deduction monotonic, while induction is nonmonotonic.

as long as none of the original premises is removed, while a strong inductive argument can be rendered weak or worthless (its strength can be eroded) by the addition of new premises. The immunity of deduction to erosion is reflected in a principle of weakening, namely, if p entails q , then $p.r$ entails q , for any arbitrary choice of r . The susceptibility of inductive arguments to erosion is mirrored in the probability calculus, where a high value of $P(G|F)$ does not entail that $P(G|F.H)$ will be high; indeed, the addition of H to the specification of the reference class may render the latter probability zero. Thus, I-S explanation raises the notorious reference class problem, a problem that does not exist for D-N or D-S explanations. Hempel recognized the problem and articulated it as the doctrine of *essential ambiguity of inductive-statistical explanation*. He introduced the *requirement of maximal specificity* to deal with it.

His accomplishment in this essay was no less than a formulation of the *received view* of scientific explanation that was to dominate discussions of the topic for about two decades.¹⁹ Its importance is twofold, namely, in the issues that are raised and clarified, and in its power to engender a vast and fruitful literature devoted to the nature of scientific explanation. I believe it is fair to say that the received view of “Aspects” is no longer received; indeed, it seems to me that none of the leading contributors to this subject any longer holds that all and only explanations that conform to Hempel’s models are legitimate scientific explanations. It provoked various alternative models or conceptions of scientific explanation. This fact in no way diminishes the fertility and importance of Hempel’s work. Going beyond the limited goals of the 1948 article, it opened up an even broader target for philosophers to contemplate and at which they could take aim.²⁰ It is noteworthy that by 1965 Hempel had given up on the attempt, which we saw in the 1948 essay, to provide an explication of scientific explanation in terms of formal syntax and semantics alone. I take this to be a large step forward, not a retreat.

I remarked above that an awareness of the possibility of scientific explanations of natural phenomena is one of the most significant pieces of philosophical progress in the twentieth century. The bases of this claim are both intellectual and practical. Philosophers from Aristotle onward have observed that we want to know not only *what* but *why*.

19. Any reader who is unfamiliar with “Aspects of Scientific Explanation” should at least examine the detailed table of contents of the essay in (Hempel 1965, 331–332) to gain an appreciation of the scope of this essay.

20. My personal intense interest in scientific explanation was sparked by Hempel’s 1962 article on statistical explanation.

The same sentiment is expressed by Weinberg in the passage quoted above. The why-questions he poses are obviously requests for intellectual satisfaction. The crucial point is that we now believe that *scientific* knowledge can provide the answers. Going back to pre-Newtonian times, we find that mariners knew *that* there was a correlation between the tides and the position and phase of the moon. In answer to the why-question, they might have said that divine providence, in its goodness, had provided a sign to guide the sailors. (A similar guide to longitude would have been greatly appreciated; see Sobel 1995 for a fascinating account.) Newton, in contrast, provided a *scientific* explanation of the phenomenon. It is intellectually satisfying to identify a mechanism that ties the behavior of the tides to many other diverse phenomena such as the appearance of comets, the falling of apples, and the motions of planets. And it is significant that the Newtonian explanation relies on a theory that is amenable to extensive testing. In the twentieth century, we find it intellectually satisfying to realize that the anomalous precession of the perihelion of Mercury can be explained by general relativity on the basis of the fundamental character of universal spacetime. Although this explanation has no known practical utility—general relativity is not required for management of space vehicles—it satisfies a deep desire to understand the nature of our universe and how it came to be as it is.

But there is also a major practical consequence of the view that science can provide explanations of natural phenomena. As we enter the twenty-first century, we know that we will face enormous global problems—e.g., overpopulation, famine, pandemic diseases, inadequate supplies of safe water, global climate change, pollution of air and water, reduction in biodiversity, and . . . the list goes on. The challenge is to arrive at scientific understanding of the problems, based on hard scientific evidence. We need to know how and why these problems arise. When we have explained the nature of the problems, we may be able to summon resources to combat them. Again, we need scientific understanding of the consequences of whatever means may be undertaken. The point is clearly illustrated by attempts—not always successful—to explain why a particular airplane crashed, in order to try to eliminate the causes and prevent similar accidents in the future.

I certainly am not suggesting that we can come to complete scientific understanding of all the above-mentioned problems and find the means to solve them. What matters is the realization that scientific explanations are possible to some degree in some cases. It does not suffice, I think, to say with Pearson that science can be regarded merely as an instrument of prediction and control. Consider a couple of examples. First, it seems to me that considerable understanding of complex fac-

tors is required to ascertain whether recent apparent trends in the climate of Earth are results of human activities or merely natural fluctuations that would occur even in the absence of human intervention. Even to trace global temperature fluctuations over many millennia in the past requires theoretical knowledge of the mechanisms by which the records have been laid down. Second, I think that genuine understanding of physico-chemical mechanisms—many of which are not directly observable—is required to determine the relationships among the human use of fluorocarbons, the depletion of the ozone layer, and the incidence of skin cancer. The examples are numerous, and the complexities are extreme, but these cases should serve to illustrate the crucial practical importance of scientific understanding of nature. They show that scientific explanations are essential even when the goals are *entirely* practical; these needs exceed by far the instrumentalist rubric of prediction and control.

I certainly am not suggesting that science holds the keys to all policy decisions. What matters is that policy makers should have available to them the scientific understanding to make wise policy decisions. I certainly am not suggesting that everyone who is scientifically informed will make wise policy decisions. But decision makers should understand the consequences of their choices. *The point is to compare this situation to that at the beginning of our century, when scientific explanation and understanding were widely considered to be impossible.* One can hold out the hope, limited though it may be, that science *can* supplant superstition to some extent as scientific understanding becomes more widely available.²¹

6. The Spirit of Logical Empiricism. Let me now turn from Hempel's specific work on scientific explanation to the more general question of the status of logical empiricism at the close of the twentieth century. If one should ask which book, among all treatises in philosophy of science, gives the finest introduction to logical empiricism, I would an-

21. ICSU, formerly known as the International Council of Scientific Unions, but recently renamed as the International Council for Science, is a scientific organization formally connected to UNESCO. It has 75 National Members (represented by National Academies of Science or similar organizations) and 25 Scientific Unions as members, ranging from the International Mathematical Union and the International Union of Pure and Applied Physics to the International Union of Food Science and Technology. It is the largest nonpolitical and noncommercial scientific organization in the world. Through its Committee on Capacity Building in Science, it has undertaken a massive effort to make relevant scientific knowledge available throughout the world. These efforts are specifically designed to deal with the kinds of global problems to which I have referred. The International Union of History and Philosophy of Science is a member of ICSU (which retains its acronym despite its change of name).

swer, without hesitation, *Aspects of Scientific Explanation*. It treats all of the *core* issues in scientific methodology. It deals profoundly with theoretical realism, scientific explanation, confirmation, concept formation,²² and scientific meaning. Under this latter heading we find an essay on operationism and one on the empiricist criterion of cognitive significance.²³ These are the issues that extend into all of the empirical sciences. Reichenbach never gave up on his probabilistic version of the verifiability criterion, and Carnap (1956) attempted to reformulate the criterion in terms of meanings of theoretical concepts, but today logical empiricists and their direct descendents seem largely to have abandoned the effort to formulate such criteria. Hempel's "Empiricist Criteria of Cognitive Significance: Problems and Changes" (1965, 101–122) played a crucial role in this development.

In commenting on Hempel's work on the 'core issues' in philosophy of science, I am not suggesting that logical empiricists do or should confine their philosophical efforts to these issues. Reichenbach, who wrote classic works on philosophy of space and time, was a member of Einstein's department prior to Hitler's rise in 1933, and he had a deep understanding of the theory of relativity. In addition, he addressed problems in quantum mechanics and thermodynamics extensively. As mentioned above, he also made monumental contributions to probability and induction. Carnap, in addition to his equally monumental contributions to confirmation and probability, contributed profoundly to formal logic and semantics. We must keep in mind that logical empiricism embraces logic, and not only such simple systems as first order predicate logic. In their approach to empirical science, logical empiricists employ the most sophisticated logical and mathematical systems available at any given time.

It is well known that many philosophers with extensive training in physics currently are dealing with a plethora of issues in general relativity and quantum mechanics. Although many of them would decline to be listed as adherents of a philosophical movement such as logical empiricism, a great number of them are doing exactly the sort of work that Carnap, Hempel, and Reichenbach would applaud. As physical science progresses, new philosophical problems and perspectives arise. Philosophy of biology, as we understand it today, did not exist when these three great philosophers did their most significant work. Nevertheless, much recent and contemporary work on evolutionary and mo-

22. Hempel's contribution to the *International Encyclopedia of Unified Science* was his 1952 monograph, *Fundamentals of Conception Formation in Empirical Science*.

23. This essay is an artful combination of two previously published essays, Hempel 1950 and 1951. A 1964 Postscript is added.

lecular biology is equally well grounded in the scientific subject matter and would be gratefully accepted by logical empiricists as a growth in scope of their point of view. Similar comments could be made with respect to the social sciences. A striking case in point is the great import of Hempel's models of explanation in recent archaeology (see M. Salmon 1982, Ch. 6). Advances in neurophysiology offer major challenges to philosophy of science. Moreover, some serious work on psychoanalysis falls entirely within the scope of logical empiricism.²⁴ Although, as noted above, many of the practitioners of these philosophical endeavors would decline classification as logical empiricists, *the spirit of logical empiricism* deeply pervades their work. This *spirit* is very much alive today.

The question is bound to arise as to whether Thomas Kuhn killed logical empiricism with the publication of *The Structure of Scientific Revolutions*. The answer is unambiguously negative, as George A. Reisch (1991) has pointed out. Kuhn's work was, after all, first published in the *International Encyclopedia of Unified Science*, a compendium that was to encapsulate the results of logical positivism and logical empiricism. Carnap was one of the editors of the *Encyclopedia* when Kuhn's book was accepted for publication, and in Carnap's papers Reisch found correspondence between Carnap and Kuhn, and between Carnap and the other editors, which shows unequivocally that Carnap thoroughly approved of the work Kuhn had submitted.²⁵

24. As a matter of fact, I had serious discussion with Reichenbach shortly before his death in 1953 about the possibility of applying philosophy of science to psychoanalysis. He was enthusiastic and supportive. Although, for personal reasons, I did not pursue this line of research, it has been taken up by Adolf Grünbaum with extremely fruitful results.

25. Carnap (1949, 126) writes: "In translating one language into another the factual content of an empirical statement cannot always be preserved unchanged. Such changes are inevitable if the structures of the two languages differ in essential points. For example: while many statements of modern physics are completely translatable into statements of classical physics, this is not so or only incompletely so with other statements. The latter situation arises when the statement in question contains concepts (like, e.g., 'wave function' or 'quantization') which simply do not occur in classical physics; the essential point being that these concepts cannot be subsequently included since they presuppose a different form of language. This becomes still more obvious if we contemplate the possibility of a language with a discontinuous spatiotemporal order which might be adopted in a future physics. Then, obviously, some statements of classical physics could not be translated into the new language, and others only incompletely. (This means not only that previously accepted statements would have to be rejected; but also that to certain statements—regardless of whether they were held true or false—there is no corresponding statement at all in the new language.)" The similarity of this statement to some of Kuhn's views is striking. I am grateful to John Earman for calling this passage to my attention.

Kuhn deplored the degree to which the history of science was ignored or misused by scientists and philosophers. Logical empiricism in no way precludes history. It has no call to hold onto history badly done, though logical empiricists have sometimes been guilty of doing it badly. My commitment (as a logical empiricist) to the importance of the history of science is illustrated above in my invocation of turn-of-the-century work on Brownian motion and Avogadro's number in the discussion of scientific realism.

In 1983 a symposium on Hempel's philosophy was held at the Eastern Division meeting of the American Philosophical Association. I had the honor to share the floor with Hempel and Kuhn. Inasmuch as Hempel and Kuhn had been engaged for some time in a discussion of scientific rationality, that was the topic Kuhn chose to address. I was happy to go along with that decision. The first point I should mention is that the three of us were able to discuss the topic without breakdowns of communication. We found that our profound agreements far outweighed our differences.²⁶

The second point has to do with our audience. After Kuhn and I had given our initial papers, Hempel responded incisively to the issues that had been raised. The room in which the symposium was held was large, but all of the seats were occupied and many listeners stood in the back. When Hempel finished his comments, the audience gave him a standing ovation that endured for an unprecedented period of time. At the close of the symposium, after Kuhn and I had made our responses and dealt with questions and comments from the floor, Hempel briefly summarized his reactions to the discussion. When he finished, the audience repeated its standing ovation, with even greater enthusiasm. Never have I ever witnessed such a response from any audience at any philosophical gathering. It was a most beautiful expression of the respect and affection in which he was held by this large group of philosophers. While I cannot say what feelings motivated various members of the audience, I vividly recall my sense of the overwhelming vitality of Hempel's presence—his incisive articulation of his views without the slightest trace of dogmatism, his eagerness to know and appreciate the views of others without compromising the clarity of his own, his forward-looking problem-solving perspective, his intellectual integrity, and his personal warmth. These are characteristics of his life and his work; they were celebrated in this symposium. My heart is filled with joy whenever I recall this moving occasion.

26. I have discussed this point in detail in Salmon 1990b. To strengthen the bridge between logical empiricism and Kuhn I invoke Bayes's theorem.

REFERENCES

- Carnap, R. (1928). *Der Logische Aufbau der Welt*. Berlin-Schlachtensee: Weltkreis Verlag.
- . (1949), “Truth and Confirmation”, in H. Feigl and W. Sellars (eds.), *Readings in Philosophical Analysis*. New York: Appleton-Century-Crofts, 119–127.
- . (1950), “Empiricism, Semantics, and Ontology”, *Revue internationale de philosophie* 4^e année: 20–40.
- . (1956), “The Methodological Character of Theoretical Concepts”, in *Minnesota Studies in the Philosophy of Science* 1. Minneapolis: University of Minnesota Press, 38–76.
- . (1963), “Carl G. Hempel on Scientific Theories”, in Schilpp 1963, 958–966.
- . (1966). *Philosophical Foundations of Physics*. Edited by Martin Gardner. New York: Basic Books.
- . (1967), *The Logical Structure of the World*. Berkeley: University of California Press. Translation of Carnap 1928 by R. A. George.
- . (1974), *An Introduction to the Philosophy of Science*. New York: Basic Books. Reissue, with modifications, of Carnap 1966.
- Eberle, R. et al. (1961), “Hempel and Oppenheim on Explanation”, *Philosophy of Science* 28: 418–428.
- Friedman, M. (1974), “Explanation and Scientific Understanding”, *Journal of Philosophy* 71: 5–19.
- Glymour, C. (1980), *Theory and Evidence*. Princeton: Princeton University Press.
- Goodman, N. (1951). *The Structure of Appearance*. Cambridge, MA: Harvard University Press.
- Hempel, C. G. (1945), “Studies in the Logic of Confirmation”, *Mind* 54: 1–26, 97–121. Reprinted in Hempel 1965.
- . (1950), “Problems and Changes in the Empiricist Criterion of Meaning”, *Revue Internationale de Philosophie* 11: 41–63.
- . (1951), “The Concept of Cognitive Significance: A Reconsideration”, *Proceedings of the American Academy of Arts and Sciences* 80: 61–77.
- . (1952), *Fundamentals of Concept Formation in Empirical Science*. Chicago: University of Chicago Press. (*International Encyclopedia of Unified Science*, vol. II, no. 7.)
- . (1958), “The Theoretician’s Dilemma: A Study in the Logic of Theory Construction”, in H. Feigl, M. Scriven, and G. Maxwell (eds.), *Minnesota Studies in the Philosophy of Science* II. Minneapolis: University of Minnesota Press, 37–98. Reprinted in Hempel 1965.
- . (1962), “Deductive-Nomological vs. Statistical Explanation”, in H. Feigl and G. Maxwell (eds.), *Minnesota Studies in the Philosophy of Science* III. Minneapolis: University of Minnesota Press, 98–169.
- . (1963), “Implications of Carnap’s Work for the Philosophy of Science”, in Schilpp 1963, 685–709.
- . (1965). *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science*. New York: Free Press.
- Hempel, C. G. and P. Oppenheim (1948), “Studies in the Logic of Explanation”, *Philosophy of Science* 15: 135–175. Reprinted, with a 1964 Postscript, in Hempel 1965.
- Jeffrey, R. C. (1965), *The Logic of Decision*. New York: McGraw-Hill.
- Kaplan, D. (1961), “Explanation Revisited”, *Philosophy of Science* 28: 429–436.
- Kim, J. (1963), “Discussion: On the Logical Conditions of Deductive Explanation”, *Philosophy of Science* 30: 286–291.
- Kuhn, T. (1962), *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.
- Lewis, C. I. (1946), *An Analysis of Knowledge and Valuation*. La Salle, IL: Open Court.
- Nye, M. J. (1972), *Molecular Reality*. London: Macdonald.
- Parrini, P. (1994), “With Carnap, Beyond Carnap: Metaphysics, Science, and the Realism/Instrumentalism Controversy”, in Salmon and Wolters 1994, 255–277.
- Pearson, Karl ([1911] 1957), *The Grammar of Science*, 3rd ed. New York: Meridian Books.
- Perrin, J. (1913), *Les Atoms*. Paris: Alcan.

- . (1923), *Atoms*. Translation of Perrin 1913 by D. L. Hammick. New York: Van Nostrand, 255–277.
- Reichenbach, H. (1933), “Rudolf Carnap, *Der Logische Aufbau der Welt*”, *Kantstudien* 38: 199–201.
- . (1935), *Wahrscheinlichkeitslehre*. Leyden: A. W. Sijthoff’s Uitgeversmaatschappij.
- . (1938), *Experience and Prediction*. Chicago: University of Chicago Press.
- . (1949), *The Theory of Probability*. Berkeley: University of California Press. Translation of Reichenbach 1935 by E. H. Hutten and M. Reichenbach. Second enlarged edition of Reichenbach 1935.
- . (1956), *The Direction of Time*. Berkeley: University of California Press.
- Reisch, G. A. (1991), “Did Kuhn Kill Logical Empiricism?” *Philosophy of Science* 58: 264–277.
- Salmon, M. (1982), *Philosophy and Archaeology*. New York: Academic Press.
- Salmon, W. (1988), “Deductivism Visited and Revisited”, in A. Grünbaum and W. Salmon (eds.), *The Limitations of Deductivism*. Berkeley: University of California Press, 95–127.
- . (1990a), *Four Decades of Scientific Explanation*. Minneapolis: University of Minnesota Press.
- . (1990b), “Rationality and Objectivity in Science, or Tom Kuhn Meets Tom Bayes”, in W. Savage (ed.), *Minnesota Studies in the Philosophy of Science*. Minneapolis: University of Minnesota Press, 175–204.
- . (1994), “Carnap, Hempel, and Reichenbach on Scientific Realism”, in Salmon and Wolters 1994, 237–254.
- Salmon, W. and G. Wolters (eds.) (1994), *Logic, Language, and the Structure of Scientific Theories*. Pittsburgh: University of Pittsburgh Press/Konstanz: Universitätsverlag Konstanz.
- Schilpp, P. A. (ed.) (1963), *The Philosophy of Rudolf Carnap*. La Salle, IL: Open Court.
- Sobel, D. (1995), *Longitude*. New York: Walker and Co.
- van Fraassen, B. (1980), *The Scientific Image*. Oxford: Clarendon Press.
- Weinberg, S. (1992), *Dreams of a Final Theory*. New York: Vintage Books.