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Carl G. Hempel (1905–1997)

PHILIP KITCHER

Introduction

Carl Gustav Hempel was one of a group of philosophers from Central Europe who emigrated to the United States in the 1930s and who profoundly modified the character of American philosophy. Together with Rudolf Carnap, Ernest Nagel, and Hans Reichenbach, Hempel was central to the transition from logical positivism to logical empiricism. His writings not only set the agenda for philosophy of science in the middle decades of the twentieth century but also continue to shape this important field of philosophy.

Educated in Berlin, Hempel was influenced by early twentieth-century attempts to apply the concepts and techniques of mathematical logic to the empirical sciences, pioneered by Reichenbach in Germany and Carnap and his co-workers in Vienna. The Vienna Circle had hoped to diagnose most traditional philosophical discussions as treatments of pseudo-problems, by formulating and applying a precise criterion of cognitive significance. The envisaged criterion was intended to pick out, as meaningful, statements of logic and mathematics conceived as analytic truths, and those non-analytic statements that admit of empirical test. The sciences, paradigmatically the physical sciences, were to count as meaningful because they satisfied the latter condition, whereas the statements of traditional philosophy were to be exposed as neither analytic truths nor susceptible to empirical test, and consequently devoid of cognitive significance.

During the 1930s, logical positivists made successive attempts to make the criterion of cognitive significance sufficiently sharp to perform the planned surgery. Hempel participated actively in these discussions, examining various proposals and identifying difficulties. His efforts culminated in synthetic essays that argued for the impossibility of the positivist project and drew the blueprint for logical empiricism, the philosophical structure within which he would build substantive positions.

Cognitive significance

Hempel never wavered in his commitment to the idea that "the general intent of the empiricist criterion of meaning is basically sound" (1965: 102), that is, that meaningful empirical statements are those that admit of experiential test. He believed, however, that attempts to state precise logical criteria of empirical significance encounter systematic difficulties. If we insist that empirical statements are those that admit of conclusive verification, then the criterion will debar universal statements (including the laws of the various sciences); to propose that empirical statements must allow conclusive falsification is equally hopeless, since this would deny meaningfulness to existential claims. Nor can we propose to formulate a criterion of meaningfulness by requiring definability of all terms in some language whose nonlogical vocabulary can be learned in application to observational entities and properties, for the special terms that play so fruitful a role in the physical sciences, expressions like "state function" and "covalent bond" cannot be given explicit definitions. Hempel concludes that the requirement of meaningfulness must imitate the way in which scientists extend the resources of everyday language, to wit by systematically connecting sentences in which unfamiliar expressions appear with sentences whose vocabulary is unproblematic.

My reconstruction condenses a wealth of subtle points that Hempel articulates with characteristic lucidity, but it brings out two important aspects of the critique of formal criteria of cognitive significance. First, Hempel is not simply concerned with a philosophical enterprise that intends to transcend what are taken to be sterile disputes, but, in addition, to provide an assessment of movements in the natural and social sciences that call for empirical conditions for the application of bits of theoretical vocabulary. He offers a definitive rebuttal to the operationalist demand that newly-introduced terms must be associated with an experiential procedure for applying them (see Hempel 1965: ch. 5). Second, that rebuttal depends on taking the unit of appraisal to be "sentences forming a theoretical system" (1965: 117). Explicitly acknowledging that this makes cognitive significance, "a matter of degree" (ibid.), he offers four main ways in which theoretical systems should be evaluated: by scrutinizing the connections among theoretical terms and the connections to statements couched in observational vocabulary; by considering the explanatory and predictive power of the system; by appraising the simplicity of the system; and by assessing the degree of confirmation by empirical evidence.

For the positivist eager to prick the pretensions of traditional metaphysics or the operationalist concerned that some area of science should be rigorously developed, this concluding catalogue must come as a disappointment. In place of a clear criterion that can put an end to disputes, it appears that these fuzzy virtues of theoretical systems will be hard to identify and that we are fated to continue the wrangles of the past. Hempel's reply would be twofold. First, he would point out that the weapons for which positivists and operationalists yearn are simply not to be had. Second, he would insist that his list of modes of appraisal may be the end of a reflection on the fate of positivism, but that it is only the beginning of a serious philosophy of science. The task for logical empiricism is to say, as precisely as possible, what kinds of linguistic connections are present in virtuous theoretical systems, what makes for explanatory and predictive power, what counts as simplicity, and how empirical statements are confirmed by the results of experiential tests.

For Hempel, then, there are four main problems of the philosophy of science. The last problem, the issue of empirical confirmation, is also central to the theory of knowledge, and, since ontological questions depend on obtaining a clear view of the kinds of entities to which the sciences commit us, clarification of theoretical structure is pertinent to discussions in metaphysics. Throughout his career, Hempel made major contributions to three of the four problems he highlighted: he offered a theory of qualitative confirmation, provided magisterial discussion of the fruitful use of theoretical vocabulary, and delineated an account of scientific explanation against which all subsequent treatments of this topic must be measured. Other logical empiricists, particularly Reichenbach (1938) and Nelson Goodman (1949), took up the problem of understanding simplicity, although this topic never achieved the same prominence within logical empiricism as the other issues.

A philosopher's life work rarely conforms to a neat structure, and Hempel's is no exception. During the course of his career, he wrote important essays on the character of mathematics and the relations between mathematics and the natural sciences (1945a, 1945b). He also retained a strong interest in the properties of systems of classification, from his early attempt to apply logical notions in taxonomy (Hempel and Oppenheim 1936) to various attempts to evaluate proposed classificatory schemes in psychiatry and in the social sciences (1965: chs 6 and 7). Despite the undeniable influence that these studies have had, Hempel's attacks on the problems of confirmation, theory-structure, and explanation are, I believe, his most enduring accomplishments. The following sections will consider them in order of ascending importance.

Qualitative confirmation

Suppose that h is a hypothesis in whose truth-value we are interested, and that e is a statement that reports the result of some empirical test. The general problem of confirmation is to understand the nature of the relation of evidential support between h and e. This general problem encompasses several specific questions: we might ask the *degree* to which e would support h (the quantitative problem); alternatively, we might inquire after the conditions under which e would support h at all (the qualitative problem); an intermediate question probes the conditions under which e supports h more than e' supports h' (the comparative problem; note that e might be the same as e' or h identical with h'). In his extensive investigations of inductive logic, Carnap focused on the quantitative problem, considering formal languages adequate for the formulation of fragments of science, and attempting to define, for a wide class of statements h and e within these languages, the degree to which e would confirm h. By contrast, Hempel took the qualitative problem to be more fundamental, and endeavored to specify conditions under which singular statements (conceived as ascribing properties to objects) would confirm, disconfirm, or be neutral to, a hypothesis (characteristically thought of as a lawlike generalization).

Hempel's treatment is noteworthy not just for his positive proposal but for his disclosure of interesting difficulties with apparently plausible ideas. Suppose that the hypothesis of interest is the generalization that all ravens are black, formalized as $(x)(Rx \supset Bx)$. It is very natural to believe that the generalization is supported by observing black ravens. So we might arrive at the general proposal of confirmation by instances: the hypothesis $(x)(Rx \supset Bx)$ is confirmed by any sentence $Ra\&Ba$. As Hempel points out, the manner in which we formulate a hypothesis should make no difference to the class of statements that confirm it. Thus, if h and h' are logically equivalent any

e that confirms h should confirm h', and conversely. By elementary logic, $(x)(Rx \supset Bx)$ is logically equivalent to $(x)(-Bx \supset -Rx)$. The thesis of instance confirmation now tells us that the latter is confirmed by $-Ba\&-Ra$, which, by the principle about logical equivalence, must also confirm $(x)(Rx \supset Bx)$. Reverting to our interpretation of the nonlogical vocabulary, we discover that the generalization "All ravens are black" is confirmed by statements that tell us that a particular object is neither black, nor a raven. Apparently, learning that the rightmost shoe in my closet is white would support an ornithological generalization!

The "paradox of the ravens" has inspired a large subsequent literature. Hempel's own diagnosis was that there is no genuine paradox, and that any sense of surprise stems from "misguided intuitions" (1965: 20). Whether or not this is so, there is no doubt that Hempel's further investigations disclose severe problems in natural conceptions. Many philosophers, and scientists reflecting on methodological issues, have accepted the fundamental idea of hypothetico-deductivism, to wit that hypotheses are confirmed when their consequences are found to be correct. The most straightforward way to formulate that idea is as the *Converse Consequence Condition*: if e is a consequence of h, then e confirms h. Unfortunately, that condition, coupled with a requirement that is hard to resist, generates the conclusion that any statement will confirm any hypothesis. The further requirement is the *Entailment Condition*: evidence statements confirm the logical consequences of the hypotheses they confirm. Given any statement e, e is a consequence of $h\&e$ (whatever h may be); so by the Converse Consequence Condition e confirms $h\&e$; h is a consequence of $h\&e$; hence by the Entailment Condition e confirms h.

Hempel's own account of qualitative confirmation avoided this difficulty by abandoning the Converse Consequence Condition. Instead, he proposed that direct confirmation results when an evidence statement entails a restricted version of the hypothesis, effectively what the hypothesis would say if there just existed the individuals mentioned in the evidence statement. More exactly, suppose that the evidence statement e contains the names of exactly the individuals $a_1, \ldots, a_n$; then e directly confirms $(x)(Rx \supset Bx)$ just in case e entails each of the statements $Ra_1 \supset Ba_1, \ldots, Ra_n \supset Ba_n$. The general notion of confirmation is obtained by proposing that e confirms h just in case h is entailed by a class of sentences each member of which is directly confirmed by e.

Hempel's approach is unable to account for the confirmation of sentences containing theoretical vocabulary by statements formulated in more basic terms. Clark Glymour attempted to extend the Hempelian approach to offer an account of qualitative confirmation (or of relevant evidence) that would address this difficulty (Glymour 1980), and his proposal has given rise to extensive subsequent discussion. Hempel himself became convinced that the general approach could not succeed, on the grounds that Nelson Goodman's "new riddle of induction" showed the inadequacy of any purely syntactical analysis of qualitative confirmation (see GOODMAN). Arguing that not all universal generalizations are supported by their instances, Goodman (1955) exposed the difficulties of distinguishing those generalizations that can be confirmed in this way from those that cannot.

Ironically, much contemporary thinking about confirmation diverges from Hempel's treatment at a very early stage. The most influential more recent proposal is

Bayesianism, a position that descends from Carnap's investigations in inductive logic and that attempts to understand how degrees of confirmation of hypotheses adjust in the light of evidence. For Bayesians, the problem of quantitative confirmation is primary and the solution to the qualitative problem is generated in a trivial fashion from a solution to the quantitative problem. To solve the latter, we need to be able to assess the value of the probability of the hypothesis given the evidence, $Pr(h|e)$; assuming that that can be done, we can say that e confirms h (or confirms h relative to background information B) just in case $Pr(h|e) > Pr(h)$ (or $Pr(h|e\&B) > Pr(h|B)$). Hempel's discussions of confirmation remain of interest not so much because of his positive proposal about qualitative confirmation as for his careful exposure of difficulties in intuitively attractive ways of thinking about confirmation, and for his recognition of constraints on any adequate solution.

Theories

Logical empiricism began with the conviction that the tools of logic developed by Frege, Russell, and their successors could be used to make clear and explicit the structure of scientific theories (see FREGE and RUSSELL), and, indeed, even in the 1920s, Reichenbach had offered an axiomatization of the special theory of relativity, intended to exhibit which parts of the theory were conventional stipulations and which made substantive empirical claims. Almost unselfconsciously, the logical empiricists took over the logician's conception of a theory as a deductively closed set of sentences in some suitable formal language. Recognizing that an important – and, from the empiricist viewpoint, problematic – feature of the theories in physics and chemistry that most impressed them was the presence of special vocabulary that resists explicit definition in observational terms, logical empiricists formulated a distinctive view about scientific theories. A scientific theory is a deductively closed set of statements in a first-order language whose nonlogical vocabulary divides into two subsets, the basic vocabulary (often understood as containing those terms whose application can be made on the basis of more or less direct observation) and the theoretical vocabulary (the remainder); the statements whose essential nonlogical vocabulary contains only theoretical terms are the theoretical postulates of the theory, while statements whose essential nonlogical vocabulary contains both theoretical and basic terms are the correspondence rules (a variety of other designations for this last class of statements appears in logical empiricist writings, but "correspondence rules" is the most popular locution); the function of the correspondence rules is to provide the theories with empirical content, and they (or a subset of them) are often conceived as providing an interpretation (or partial interpretation) of the theoretical vocabulary.

This conception of scientific theories became fully explicit in the writings of Carnap (1956), Nagel (1962), and Hempel (1958, 1965: ch. 8), and, perhaps as a residue of the concerns about cognitive significance, each of these authors sought ways to characterize those theoretical contexts in which the introduction of theoretical vocabulary served important scientific purposes (see CARNAP). Hempel's discussions revolved around a family of questions. To what extent can the correspondence rules be viewed as functioning as definitions? Is it problematic to concede that the correspondence rules only offer partial interpretations of the theoretical vocabulary, or should this be seen as a symptom of the openendedness of scientific research? Can we eliminate the theoretical vocabulary without any scientific loss? Is it reasonable to treat the theoretical terms as components of a formal apparatus for making experiential predictions, or should we suppose that those terms refer to entities and properties that underlie the observable phenomena?

Hempel's discussion of these questions embodies a cautiously realistic attitude. He does not think that we can provide a full explicit definition of theoretical vocabulary in observational terms, not even for those relatively low-level parts of science that make use of dispositional concepts. Although he believes that correspondence rules are vehicles of partial interpretation, he suggests that we cannot neatly separate the parts of a theory that function to pin down the meanings of our terms from those that make genuinely empirical claims, a point in which he concurs with W. V. Quine's celebrated critique of the analytic/synthetic distinction (Quine 1953) (see QUINE). Partial interpretation, Hempel believes, has heuristic advantages, allowing us to introduce new correspondence rules as we extend the theory to cope with previously untreated phenomena. Further, to the extent that theoretical vocabulary is ineliminable, he holds that the evidence leading us to adopt the theory ought to incline us to accept its postulates as true and thus to treat its theoretical vocabulary as referring to entities beyond the reach of ordinary observation.

The crucial issue thus turns out to be whether or not there is a generally available method for eliminating theoretical vocabulary. Hempel approaches the problem by formulating a dilemma. Starting from the premise that the function of a theory is to "establish definite connections among observable phenomena" (1965: 186), he suggests that when such connections are established we do not need any detour through a theory, since the theory will imply a conditional statement, couched in the basic vocabulary, that asserts the connection. So, if the theoretical terms and principles serve their purpose, they can, in principle, be eliminated, and are thus unnecessary. If, on the other hand, the theoretical terms and principles do not establish the intended connections, they do not serve their purpose, and are consequently unnecessary. This is "the theoretician's dilemma" (1965: ch. 8).

Behind the dilemma stands a view of the use of theories in scientific practice. It is as though the scientist feeds some description of observables into the theoretical machinery, the gears turn, and the output is another statement about observables. The point may be to predict something (the output statement is one we didn't know before) or it may be to explain something (we already knew the output statement but didn't see why it was true). In either case, the theoretical statements function to license an inference from the input to the output, and thus must support the conditional statement, "If input then output." Why then can we not manage with the set of all such conditional statements corresponding to the transitions that the theory would license?

A first response is that the resultant set would be extremely unwieldy, that the theory as actually presented provides a concise way of representing a disparate class of consequences. As Hempel and his colleagues clearly saw, however, a result due to the logician William Craig constructs a recursive procedure for generating the class of consequences, without stepping outside the basic vocabulary. (It is interesting to reflect that the significance attributed to Craig's theorem revealed the hold that the logician's conception of theories as recursively axiomatizable continued to exert on logical empiricist discussions of scientific theories.) In the end, Hempel's response to this and to kindred suggestions for eliminating theoretical vocabulary draws on the proposal that the task of a scientific theory is not only to achieve deductive systematization of observable phenomena but also to provide inductive systematization, and we have no reason to believe that any of the elimination procedures will satisfy this further constraint (1965: 214–15; note that in the case of the Craigian surrogate, Hempel is able to argue that the substitute will not achieve any inductive systematization).

It is at first sight ironic that the philosopher who contributed most to our understanding of scientific explanation overlooked a relatively obvious point about proposals for eliminating theoretical terms: even though they might be able to mimic the predictive successes of genuine theories, it seems that they would incur severe explanatory losses. Whatever set of brute empirical rules we might devise for predicting the outcomes of bringing substances together in various proportions would fail to deliver the explanatory benefits we obtain from embedding empirical generalizations within the theoretical treatment of shell-filling and of ionic and covalent bonds. On deeper reflection, however, we can see that appeal to explanatory power would have led Hempel into uncomfortable questions about the sufficiency of his preferred account of scientific explanation. For there is every reason to think that some of the procedures for eliminating theoretical terms would not just deliver singular conditional statements but generalizations from which such singular conditionals could be derived; because the generalizations would serve as the needed "covering laws" in Hempel's schemata for explanation, by the standards of the Hempelian account of explanation the explanatory loss would be indiscernible.

During the 1960s and 1970s, the account of theories favored by Hempel, Nagel, Reichenbach, and other logical empiricists acquired the name "the received view," and like most doctrines so designated came under vigorous attack (see Suppe 1970 for thorough analysis). One principal difficulty, recognized by Hilary Putnam, was that the account conflated two distinctions, the distinction between observational and theoretical *terms* and that between observable and unobservable *things*; as Putnam noted, some theoretical terms name observable things ("oscilloscope") and unobservable things can be picked out using observational terms ("people too little to see"). Putnam's observations, and the discussions of reference that he and others initiated (Kripke 1971, Putnam 1973) undercut the old concern that scientists are simply unable to specify the referents of their theoretical vocabularies.

A different line of objection attacked the idea that a theory is a linguistic item. Several authors (including Suppes 1967, van Fraassen 1980) drew inspiration from model theory rather than from the syntax of logical systems, proposing that theories are to be identified with families of models. Their critiques initiated a debate, as yet unresolved, about the correct analysis of the notion of a scientific theory. It is perhaps worth recalling that both the newer "semantic conception of theories" and the older "syntactic account" (or "received view") are philosophical reconstructions of the practices of scientists, and that the standards of adequacy for a reconstruction depend on what purposes – philosophical or scientific – one intends to achieve. The question "What is the *real* structure of a scientific theory?" may simply be a bad question, and, depending on our aims, we may draw on one or another of the proposed accounts (or on something completely different). To the extent that the syntactic conception continues to be valuable in such enterprises, we can expect that Hempel's lucid and careful delineations of possibilities and constraints will remain pertinent.

Explanation

In the early decades of the twentieth century, the thought that one of the aims of the sciences is to provide explanations, traditionally popular, suffered a temporary eclipse. Thinking that appeals to the explanatory power of a theory reflected purely subjective judgments, scholars writing about science tended to concentrate on the criterion of predictive success (see, for one among many examples, Pearson). Hempel played the leading role in restoring the respectability of the concept of scientific explanation. From his earliest discussions of the topic, he insists on the objective character of scientific explanation (see 1965: 234 (originally written in 1942); also 1966: ch. 5). Taking up a theme already sounded by earlier empiricists (for example John Stuart Mill), and perhaps as old as Aristotle, he suggests that explaining a fact, state, or event consists in showing why that fact, event, or state could have been expected to occur, given the laws of nature. The key to explanation is nomic expectability.

Hempel proposed that an explanation is an argument whose conclusion is a statement describing the phenomenon to be explained (this statement is the *explanandum*) and whose premises (the *explanans*) include at least one law of nature. Although his early writings concentrated on cases in which the argument is deductive, he was explicit, from the beginning, that some explanations are non-deductive arguments. He also took considerable pains to point out that, as actually given, explanations may not take the ideal form he specified. So, for example, historians develop explanatory narratives that are far from complete arguments, and yet, Hempel contended, the explanatory force of their work derives from the possibility of recognizing general laws of nature, which in combination with the claims they advance would yield a compelling argument for the explanandum.

The *Deductive-Nomological* (D-N) model of explanation can be encapsulated in a schema: deductive explanatory arguments take the form

$$\begin{array}{l} C_1, \ldots, C_m \\ \underline{L_1, \ldots, L_n} \\ E \end{array}$$

where the C_i are statements reporting particular facts, the L_j are laws of nature, whose presence is essential to the validity of the argument, and E is the explanandum. (Hempel's model does not require that there be any Cs, although there must be at least one L; his presentations sometimes identify E as a statement of particular fact, but he allows for explanations of this form whose conclusions are laws, including probabilistic laws.) To be a genuine explanation, the premises of an argument fitting the schema must all be true. If one or more of the premises is not true, then the argument counts as a *potential* explanation.

In the 1940s, Hempel hoped to articulate the D-N model more precisely, and he proposed a formal explication of the notion of law and of deductive explanation (Hempel and Oppenheim 1948; see Hempel 1965: ch. 10). Unfortunately, this attempt proved vulnerable to trivializing counterexamples, and, in any event, Goodman's explorations of laws, counterfactuals, and induction, convinced Hempel that no formal account of scientific laws could be given. Thus, throughout the 1950s and 1960s, his work on scientific explanation focused on showing how his preferred approach to explanation illuminated aspects of the natural and social sciences and how it could be extended to include non-deductive arguments.

The latter task was complicated by an important disanalogy between deductive and inductive arguments. Adding extra premises to a deductively valid argument preserves validity, but the incorporation of new information into an argument that is inductively strong may not only undermine the argument but even support a contrary conclusion. So, to take one of Hempel's own examples, to claim that Jones is suffering from a streptococcal infection and that he is being treated with penicillin, together with the probabilistic law that 99 percent of those treated with penicillin recover from such infections confers high probability on the conclusion that Jones will recover, but if we now learn that this particular streptococcal infection is penicillin-resistant then we have strong reasons for thinking that Jones will not recover.

Hempel's model of *Inductive-Statistical* Explanation (I-S) proposed that I-S explanations are arguments with true premises of the form:

$$\begin{array}{l} Pr(B|A) = r \\ Ac \\ \hline\hline Bc \end{array} \quad [r]$$

Here the double line indicates that the premises bestow on the conclusion the probability r, which is supposed to be close to one. To block the problem of the "ambiguity of statistical explanation," Hempel imposes the "requirement of maximal specificity." If s is the conjunction of the premises of the explanation, and if k is a statement logically equivalent to the set, K, of accepted sentences, then if $s\&k$ implies that c belongs to a subset A^* of A, then $s\&k$ must also imply that $Pr(B|A^*) = r^*$ where $r^* = r$ unless the conditional probability of B on A^* is simply a matter of probability theory (as, for example, when A^* is the null set). This intricate condition is intended to require that we always employ the most specific probabilistic information we have, and, as Hempel explicitly noted, it introduces an unwelcome relativization into the account of explanation, for, unlike D-N explanations, I-S arguments only qualify as explanations relative to a particular state, K, of our knowledge.

The *Covering Law Model of Explanation*, comprising the D-N and I-S models, was enormously influential, not only restoring the respectability of the concept of explanation but also sparking methodological discussions in the social sciences. The many-sided character of Hempel's lucid discussions, especially in the title essay of *Aspects of Scientific Explanation*, provides a model for philosophical exploration of an important metascientific concept. Nonetheless, for all the subtlety of his treatment, Hempel's account is no longer widely accepted among philosophers of science (although it continues to be adopted in other philosophical debates and in the methodological reflections of natural and social scientists).

Some of the difficulties stem from problems with which Hempel struggled. The intricate requirement of maximum specificity proved inadequate to salvage the notion of

I-S explanation, yielding the unwelcome consequence that *bona fide* inductive explanations turn out to be tacitly deductive (Coffa 1974). Another difficulty of the model of probabilistic explanation lies in the fact that we can apparently explain events that are unlikely to occur: even though it may be improbable that an atomic nucleus will undergo a particular sequence of decay, we can still, it seems, use quantum physics to explain the rare occurrences that do take that path. Even in the realm of purely deductive explanation, there are formidable challenges. As Hempel himself noted, a derivation of Boyle's Law from the conjunction of this law with Kepler's laws would satisfy the D-N schema, even though any such derivation is explanatorily worthless. How do we distinguish such arguments from the explanatory derivations of laws, for example the derivation of Kepler's laws within Newtonian gravitational theory?

Perhaps the most severe problems came from a cluster of examples that showed how familiar asymmetries that occur in the context of causal judgments also affect our assessments of explanatory power. Suppose that a flagpole casts a shadow of a particular length. Using the law of rectilinear propagation of light, together with facts about the height of the pole and the elevation of the sun, it is possible to derive the length of the shadow, a derivation that fits the D-N model. So far, so good, since that particular derivation seems genuinely explanatory. The trouble is that we can also work in the opposite direction. Given the propagation law, the elevation of the sun and the length of the shadow, we can derive the height of the pole, and this derivation fits the D-N schema equally well (Bromberger 1966). A natural response to examples like this is to declare that opaque objects *produce* (or cast) shadows and that shadows do not *produce* the associated objects, so that there is a causal asymmetry unrepresented in the Hempelian schema.

Hempel's scattered remarks about the connection between explanation and causation present a clear picture of his position. Influenced by Humean worries about the notion of causation, he holds that our understanding of causal relations is grounded in our ability to subsume phenomena under lawlike regularities. The concept of explanation is prior to that of causation, in that a claim that *c* caused *e* is always derivative from the thought that the occurrence of *e* would be properly explained by an argument in which a description of *c* figured among the premises, an argument satisfying the covering-law model. Hence, Hempel cannot appeal to causal asymmetries to reformulate his account of explanation, and, on the few occasions on which he confronts examples that embody such asymmetries, he argues strenuously that our intuitive responses to these cases should not be trusted (for example, 1965: 352–3). With the articulation of a family of instances like that of the shadow-casting flagpole, attempts to deny differences in explanatory worth came to appear ever more desperate, and most philosophers of science (including Hempel himself) have concluded that the problem of explanatory asymmetry cannot be dismissed as illusory.

For about a quarter of a century Hempel's account of scientific explanation almost achieved philosophical consensus. Since it succumbed to a host of problems and criticisms no successor approach has garnered similar support. Inspired by the problem of asymmetry, several philosophers have offered accounts that invoke the concept of causation (see, for example, Humphreys 1989, and Salmon 1984, 1998). Others have tried to preserve the main features of Hempel's account by developing an idea that received passing attention in his own writings, and suggesting that explanation consists in the unification of the phenomena (Friedman 1974, Kitcher 1981). Yet others have contended that explanation is an activity whose crucial properties vary with context (Achinstein 1983, van Fraassen 1980: ch. 5). All the existing accounts face major obstacles (often gleefully noted by the partisans of rival accounts). If there is a consensus, its central tendency is that, while Hempel's covering-law model is inadequate, it is exemplary in demonstrating the range, rigor, and clarity that any satisfactory theory of explanation should strive for.

Hempel's legacies

In the past decades, logical empiricism has been criticized for various shortcomings: neglect of the historical development of science (Kuhn 1962/1970), overemphasis on the search for lawlike regularities in nature (Cartwright 1983, 1999), and failure to appreciate the autonomy of experimental practice (Galison 1987, Hacking 1983). At the same time, many philosophers have proposed that Hempel and his co-workers adopted an unnecessarily restrictive view of the formal resources on which accounts of confirmation, theories, and explanation might draw. Despite these complaints, philosophy of science continues to pursue the agenda that Hempel so lucidly articulated, and, if the set of questions has been enlarged and the Hempelian answers are no longer widely accepted, it would be foolhardy to tackle these problems without thorough awareness of Hempel's many insights.

No essay on Carl ("Peter") Hempel would be complete without some recognition of his extraordinary pedagogical influence. Not only was he the author of one of the great introductions to any field of philosophy (Hempel 1966), but, through lectures and seminars, he was an inspiration to generations of undergraduates, graduate students, and younger philosophers. Those who knew him saw, again and again, a rare combination of high scholarly integrity and personal kindness, acute intelligence and gentleness, and his daily actions reminded those around him that philosophy began with the desire for wisdom and for understanding the good. In his life, as well as in his work, Hempel was a true philosopher.

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