

Hempel, Carnap, and the Covering Law Model¹

Erich H. Reck

Abstract

Carl Gustav Hempel was one of the most influential figures in the development of “scientific philosophy” in the twentieth century, particularly in the English-speaking world. While he made a variety of contributions to the philosophy of science, he is perhaps most remembered for his careful formulation and detailed elaboration of the “Covering Law model” for scientific explanation. In this essay I consider why the CL model was, and still is, so influential, in spite of the fact that it has been subjected to many criticisms and is usually seen as superseded by alternative models. Answering this question involves a reexamination of Hempel’s relationship to other influential “scientific philosophers”, especially Rudolf Carnap. It also sheds new light on issues concerning the notions of analysis, explication, and modeling that remain relevant today.

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Carl G. Hempel (1905–1997) is usually not taken to be a philosopher of the same stature as Hans Reichenbach, the central figure in the Berlin Group and his doctoral advisor, or Rudolf Carnap, the leading member of the Vienna Circle and another important influence on him. Yet Hempel’s impact on philosophy was almost as widespread and lasting as theirs, particularly in the United States where he emigrated and where his career flourished. Hempel was educated at the Universities of Göttingen, Heidelberg, Vienna, and Berlin (Ph.D. in 1934). He first visited the US in 1937–1938 to work as Carnap’s research assistant at the University of Chicago. He came back in 1939, as a refugee, so as to stay permanently. His first teaching positions were in New York, at City College (1939–1940) and Queens College (1940–1948). Later he taught at Yale (1948–1955), Princeton (1955–1975), and the University of Pittsburgh (1976–1985). Over the course of his long career Hempel had many students. He was also active in the profession in other ways, e.g., as Vice-President of the Association for Symbolic Logic and as President of the American Philosophical Association. In retrospect, he has been called “one of the principal figures of scientific philosophizing in the twentieth century” (Rescher 2005a, 127).²

Hempel’s main contributions concern the philosophy of science.³ He is most well known for his writings on the notions of confirmation, explanation, rationality, cognitive significance, and scientific theory. In the present essay I will focus on his work on scientific explanation and its impact on philosophy in the English-speaking world. Central in this connection is Hempel’s article (co-written with Paul Oppenheim), “Studies in the Logic of Explanation” (1948), which Wesley Salmon, another main contributor to the corresponding debates, characterized as “epoch making” (Salmon 2000, 311). Concerning Hempel’s subsequent collection of essays, *Aspects of Scientific Explanation* (Hempel 1965b), James Fetzer has remarked that it “became a scholar’s bible for generations of graduate students” (Fetzer 2010, 1). Similarly, Hempel’s textbook, *Philosophy of Natural Science* (Hempel 1966), was read by generations of undergraduate students and it is still sometimes assigned today. My main goal in this essay will be to get clearer about why exactly these texts were so influential and, more basically, what their philosophical significance is. The quick answer, to be elaborated in what follows, is that this is where Hempel’s Covering Law Model for scientific explanation was presented and elaborated.

² For biographical information, cf. Fetzer 2000b, 2010, Rescher 2006, also Friedman 2000.

³ For overviews of Hempel’s main works, cf. Salmon 2000, Kitcher 2001, Fetzer 2010, and Curd 2012.

“Studies in the Logic of Explanation” (1948) is not the first work in which the Covering Law Model (CL model, for short) appeared. Its core idea had been suggested by other philosophers, e.g., by Karl Popper, Richard Braithwaite, and John Stuart Mill. In fact, it can be traced back all the way to Aristotle (Fetzer 2000a). And as far as Hempel’s own publications are concerned, the idea was already presented in “The Function of General Law in History” (1942). Nevertheless, it is the 1948 article that really set the stage for later discussions.⁴ It starts as follows:

The present essay [provides] an elementary survey of the basic patters of scientific explanation and a subsequent more rigorous analysis of the concept of law and the logical structure of explanatory arguments (Hempel 1948, 567).

In Part I of their essay Hempel and Oppenheim then introduce several motivating examples of scientific explanations and, most importantly, the following schema:

C_1, C_2, \dots, C_k	Statement of antecedent conditions
L_1, L_2, \dots, L_r	General laws
<hr/>	
E	Description of the phenomenon to be explained

The “basic pattern of scientific explanations” is thus: E (the “explanandum”) is deduced logically from C_1, C_2, \dots, C_k and L_1, L_2, \dots, L_r (the “explanans”). The two authors go on to spell out several additional requirements for explanation, divided into two groups. The “logical conditions of adequacy” are: (i) The corresponding argument has to be valid, i.e., E has to be in fact derivable from C_1, C_2, \dots, C_k and L_1, L_2, \dots, L_r ; (ii) at least one general law “must be required for the derivation”; (iii) the explanans must have “empirical content”, i.e., “be capable, at least in principle, of test by experiment or observation”. The one “empirical condition of adequacy” is: (iv) “The sentences constituting the explanans must be true”, so that the argument is sound (*ibid.*, 569–570). Hempel and Oppenheim also argue that, because of their underlying logical forms, there exists a “symmetry” between explanation and prediction in science. In later

⁴ In Wesley Salmon’s words: “The 1948 Hempel–Oppenheim article marks the division between the pre-history and the history of modern discussions of scientific explanation.” (Salmon 1990, 10)

parts of the essay they develop, among others, a “more rigorous analysis of the concept of law” by applying the concepts and tools of modern logic (syntax and formal semantics).

Implicit in the schema from “Studies in the Logic of Explanation” is that the explanandum is derived from deterministic laws together with relevant initial conditions. But Hempel acknowledged quickly that in science there are explanations based on statistical or probabilistic laws as well. In many of them the explanandum is not a deductive consequence of the explanans, but it follows only with a certain probability. Strictly speaking, the schema above applies thus only to “deductive-nomological” explanations, while “inductive-statistical” explanations have to be treated separately. Moreover, there are scientific explanations in which statistical claims are derived deductively from more general statistical laws, in which case we are dealing with “deductive-statistical” explanations. Then again, in all three kinds of cases the explanandum is subsumed under, or “covered” by, general laws; and hence, what is crucial for scientific explanations generally is “nomic expectability”. In Hempel’s later publications this view is articulated in terms of an all-encompassing “Covering Law Model”, based on a schema that generalizes the one from the 1948 essay. The most systematic, mature treatment of his position occurs in “Aspects of Scientific Explanation” (Hempel 1965), the centerpiece of Hempel 1965b, while a simpler and more accessible discussion lies at the heart of Hempel 1966.

It took some years after the publication of Hempel & Oppenheim 1948 for the CL model to attract much attention. However, from the 1960s on it became a central and entrenched part of “scientific philosophy”—it became the “received view” on explanation, the position against which all alternatives were measured. Why did it have such an impact? Hempel’s steadily increasing personal influence was important, no doubt, i.e. his recognition as a main player in the field. Yet there were more philosophical reasons as well, including the following: First, Hempel and Oppenheim’s careful, formally precise treatment rehabilitated the notion of explanation among scientifically oriented philosophers. While this may be surprising from today’s point of view, in the early twentieth century that notion was widely seen as problematic, e.g., as too subjective (too much anchored in a “feeling” of insight). One benefit of the CL model, in the eyes of many, was to secure its objectivity and rationality.⁵ Second, Hempel’s account of

⁵ As Salmon later put it: “[T]he 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

scientific laws was carefully crafted to get around Humean scruples concerning the notion of causation, as shared by many empiricists.⁶ Consequently the CL model was taken to provide an indirect but respectable way of talking about causation in terms of law-based explanations. In both respects, the approach was perceived as leading to substantive philosophical progress.

It was not just among philosophers that the CL model was noted and admired. The model also exerted a significant influence on other disciplines, such as history and some of the social sciences. In those contexts it was taken to be normative, i.e., as telling researchers to produce explanations of CL form.⁷ But soon its alleged universal applicability was called into question. (Eventually it came to be seen as a central part of the “positivist” legacy, where ideas and methods from one field, namely mathematical physics, were imposed on others in counterproductive ways; but that took a while). Within the philosophy of science doubts about the CL model also started to emerge. The initial ones concerned the specifics of the formal account of laws in Hempel & Oppenheim 1948, which were shown to lead to paradoxical consequences.⁸ While it may have appeared for a while that some minor tinkering would get around these problems, gradually further criticisms of the CL model arose, often in the form of “counterexamples” to it. These examples—many of which became classics in themselves (the flagpole, the moon and tides, syphilis and paresis, etc.)—called the CL model into question in a number of ways. Some challenged Hempel and Oppenheim’s “symmetry thesis” for explanation and prediction; others were meant to establish, very fundamentally, that for a scientific account to be explanatory it was neither necessary nor sufficient to have CL form; etc.⁹

explanation on the table, and it challenged philosophers to respond either positively or negatively. It elicited alternative analyses. The temptation to say that there is no such a thing as scientific explanation seems to have vanished.” (Salmon 1999, 315)

⁶ I take the logically based account of scientific laws in the later parts of Hempel & Oppenheim 1948 to be due mostly to Hempel. I will come back to Oppenheim’s role briefly later in this essay.

⁷ Cf. the discussion of archeology in Salmon 1990, 25-26. Hempel had applications to history in mind from early on; cf. Hempel 1942. Some of its earliest and longest lasting criticisms concern that application.

⁸ Cf. the discussion of the early, internal criticisms of the CL model (by R. Eberle, D. Kaplan, R. Montague, and others) in Salmon 1990, chapter 2.

⁹ The corresponding counterexamples focused attention on, among others, the necessity of laws, the deductive structure of explanations, and certain explanatorily relevant causal asymmetries not captured by the CL schema. For overviews, cf. Salmon 1990, chapters 2-3, and Fetzer 2000a.

While the CL model kept having defenders, including Hempel himself (who worked on improving his treatment of inductive-statistical explanations¹⁰), it began to be seen, more and more, as the foil against which to pit alternative accounts. The two primary alternatives became the “causal model”, with Wesley Salmon as the main initial proponent, and the “unification model”, represented by Michael Friedman and Philip Kitcher. In some respects these were not outright rejections of the CL model but modifications of it (especially the unification model). However, more radical alternatives also appeared, e.g., Bas van Fraassen’s “pragmatic” model (based on a formal analysis of explanation-seeking why questions) and, already earlier, a more informal, contextual approach to explanation championed by Michael Scriven (guided by a radically different methodology).¹¹ It seems fair to say that, as a result of the proliferation of alternative approaches, there is no “received view” about scientific explanation any more today, even though causal models tend to be more prominent than others. Some would even argue that it is misguided to look for a universal model in the first place and that what is needed, instead, is a plurality of models, since explanations come in a variety of different forms.

It is not my goal here to provide a comprehensive overview of the debate about scientific explanation, much less a resolution for it.¹² After having sketched at least some relevant developments, I want to return to Hempel, the CL model, and its significance. Often the attitude with respect to that model, especially by critics, appears to be the following: What Hempel and Oppenheim did, in their classic essay and elsewhere, was to start with some representative examples of scientific accounts (by Kepler, Galilei, Newton, Einstein, etc.) and then distill out their essential form, i.e., the aspect that makes them “explanatory”. If successful, this procedure would have provided us with an analysis of the notion of explication in a very strong sense: an articulation of jointly necessary and sufficient conditions for explanations in general. Moreover, as these conditions were formulated in terms of modern (deductive and inductive) logic, it would

¹⁰ Many of the “second-wave” challenges to the CL model concerned the inductive-statistical case. One attempt to improve on Hempel’s position was Salmon’s “statistical-relevance” model, which was subsequently also found wanting. Cf. Salmon 1990, chapter 3, and Fetzer 2000a.

¹¹ Pitt 1988 contains representative texts; cf. also Kitcher & Salmon 1989 and, again, Salmon 1990. For the basic difference between Scriven’s and the other approaches, cf. Reck 2012.

¹² An authoritative recent discussion of the topic, as presented by the proponent of a causal account, can be found in Woodward 2003. For a more general overview, see also, e.g., Psillos 2007.

have amounted to the reductive analysis of a notion central to science. This is, then, what the significance of the CL model is typically taken to amount to. It is just that the analysis it embodies does not work, as the counterexamples are supposed to have shown.

Two different reactions to the resulting situation are possible. First, one can hold on to the goal of providing a reductive analysis, and in particular, of articulating necessary and sufficient conditions for explanation. That is to say, while it may be true that the Hempel & Oppenheim's model does not work as such, one can try to modify it or replace it by a better analysis (along causal or unification lines, say). Second and more radically reaction, one can take the "counterexamples" to the CL to have shown, not only that this model is inadequate, but that the whole approach underlying it, in terms of a formal and reductive analysis, needs to be abandoned. That would not necessarily mean that we have to give up analyzing the notion of explanation; but we should, so the suggestion here, proceed in a non-reductive, contextual way. Now, these two kinds of reactions are not only quite different, they are opposed to each other.¹³ At the same time, they rely on a shared assumption about the CL model, namely: that it has been refuted, in some fairly direct way, by the "counterexamples". Or more generally, it is assumed that the model has been refuted by the careful description of scientific practice.

However, do the standard criticisms of the CL model really refute it so directly? First doubts may arise when one takes seriously Hempelian remarks such as the following:

[T]hese models are not meant to describe how working scientists actually formulate their explanatory accounts. Their purpose is rather to indicate in reasonably precise terms the logical structure and the rationale of various ways in which empirical science answers explanation-seeking why-questions. The construction of our models therefore involves some measure of abstraction and of logical schematization (Hempel 1965, 412).

Moreover, it is not just that the CL model (the deductive-nomological, inductive-statistical, and deductive-statistical models taken together) involves "abstraction" and "schematization", as

¹³ The first reaction, or the first kind of alternative, is much more common in the literature on scientific explanation. Even van Fraassen's pragmatic model can be seen as falling into this first camp. I take Michael Scriven's approach to be an example of the second kind of response, in the sense that he provided what Peter Strawson would later call a "connective analysis" of the notion of explanation. For further discussion of the latter point, cf. Reck 2012; for another, more recent representative of Scriven's camp, cf. Wright 2011.

Hempel readily admits. If the model is taken to provide a reductive analysis of explanation, as is usual, one misrepresents its nature and purpose more fundamentally—or so the argument I want to consider next. But if the CL model is not meant to constitute a reductive analysis, how else could we think about it? An answer to that question is provided by Carnap’s notion of explication. (I will consider a second, different answer later in the essay as well.)

Rudolf Carnap introduced the notion of explication for the first time explicitly in his book, *Meaning and Necessity* (1947); he then discussed it in more detail in his next book, *Logical Foundations of Probability* (1950). As he writes in the former:

The task of making more exact a vague or not quite exact concept used in everyday life or in an earlier stage of scientific or logical development, or rather of replacing it by a newly constructed, more exact concept, belongs among the most important tasks of logical analysis and logical construction. We call this the task of explicating, or of giving an *explication* for, the earlier concept; this earlier concept, or sometimes the term used for it, is called the *explicandum*; and the new concept, or its term, is called an *explicatum* of the old one (Carnap 1947, 7-8; original emphasis).

If one adopts Carnapian explication as one’s methodology, this does lead to abstraction and schematization, along Hempelian lines. But beyond that, descriptive accuracy is rejected, or downplayed, in an even stronger sense. The sense at issue is flagged by Carnap’s talk of “replacing” an earlier, vague concept by a new, more exact one. Here Carnap points to the fact that the main thrust in giving an explication, in his sense, is revisionary and normative rather than descriptive. And this makes it significantly different from reductive analysis.

Two closely related aspects of the relevant difference are the following: First, in an explication we start with a vague notion and replace it by a more exact one; and because of the vagueness of the former, it is misguided to judge the latter in terms of whether it “fully captures” what was there before. Second and more positively, what the new notion should be judged by instead is its usefulness. As Carnap writes in *Logical Foundations of Probability*:

Strictly speaking, the question whether the solution [the explicatum, thus the explication overall] is right or wrong makes no good sense because there is no clear-cut answer. The

question should rather be whether the proposed solution is satisfactory, whether it is more satisfactory than another one, and the like (Carnap 1950, 4).

Shortly after this passage Carnap list four main criteria for evaluating an explicatum: “(1) similarity to the explicandum; (2) exactness; (3) fruitfulness; (4) simplicity”. Note here that, while “similarity” is the first of the desiderata listed, there are three others; and those criteria typically bear more weight in Carnap’s and later applications of explication. Note also that, by only requiring “similarity” in a sense left fairly unspecific, descriptive adequacy with respect to earlier practice appears to be required only in a very weak sense.

Returning to Hempel, there are a number of reasons for seeing the CL model, as well as his approach more generally, as an instance of Carnapian explication. To begin with, many of the features distinctive of explication are present, e.g., the insistence on exactness and the use of formal tools (syntax and formal semantics). There were also personal connections between Hempel and Carnap, including during the period when both Carnap’s notion of explication and Hempel’s CL model took shape (the late 1930s and the 1940s). More concretely, Carnap is one of the people Hempel and Oppenheim thank explicitly for “stimulating discussions and constructive criticisms” in the first footnote of Hempel & Oppenheim 1948. In addition, Hempel mentions Carnap and the notion of explication positively in some of his later reflections on his work (Hempel 1973, 1988). Finally, other central participants in the ensuing debate about the CL model describe the underlying approach in Carnapian terms; thus Salmon writes: “The Hempel-Oppenheim article is an outstanding example of the use of an artificial language for the purposes of explicating a fundamental scientific concept.” (Salmon 1990, 35)

Suppose therefore that we interpret the CL model as a case of explication in Carnap’s sense. What exactly follows about that model, especially concerning how to evaluate it? As already noted, for Carnap “similarity” between the explicatum and the explicandum is a desideratum, but only one that plays a minor and subordinate role. Moreover, the only guidance with which he provides us in this connection is the following:

An indication of the meaning with the help of some examples for its intended use and other examples for uses not now intended can help the understanding. An informal explanation in general terms may be added (Carnap 1950, 1).

Notice the emphasis on “intended use” in this passage, which signals what is really crucial. Namely, in the end the evaluation of an explicatum is thoroughly pragmatic; if it serves its purpose its adoption is justified, even if this means discarding much of the old, vague “meaning” in the process. Now, if that is the underlying assumption, another question arises: What exactly is the purpose, or what are the purposes, in play here? Neither Hempel nor Carnap are very explicit in that connection (nor are many of their followers). This is partly because a thorough discussion of goals, thus of teleology and normativity, would not fit well into their empiricist framework, partly also, presumably, because an open-ended variety of goals is at issue. Yet specifying the relevant goal or goals is crucial for present purposes.

Let us assume, for example, that the primary goal in employing the CL model is the characterization of scientific practice, after all. In that case we are clearly back to descriptive accuracy as the main yardstick; and all the putative “counterexamples” are directly relevant. On the other hand, the force of the usual criticisms appears to be considerably weaker if what we are aiming at is one of the following: a) to contribute to the advancement of science, e.g., by clarifying its basic concepts or by improving its methodology; b) to contribute to the advancement of philosophy, by answering some distinctively philosophical questions. Yet even along such lines, one may wonder whether Carnap marginalizes descriptive accuracy, or what he calls “similarity”, too much. After all, might the right kind of similarity not play an important role for the effectiveness of the explicatum, as it takes over the role of the explicandum, in science? And might it not be crucial in philosophy too, depending on which particular questions we ask there? In either case, it would seem that at some point in the process there has to be a careful evaluation of whether, and to what degree, the “abstraction and logical schematization” involved in an explication do serve our purposes, whatever those are (cf. Reck 2012).

Let us suppose that, at least for some explications, questions about their descriptive accuracy, about the appropriateness of idealizations, etc. do remain. Arguably it is still the case that a Carnapian explication cannot be refuted by examples in any strict sense, because it is not meant to be right or wrong, only more or less useful, as we saw. This applies to the CL model, at least in contexts where the description of scientific practice is not our main goal. Thinking about it in such terms helps to clarify the model’s significance. It also allows us to make sense of what has happened since various alternatives to the CL model took center stage, thereby depriving it of its

status as “the received view”. Assume here, as is usual nowadays, that one or several of the counter-models are superior, in one way or another. This leaves us with the question: Why are we still talking about the CL model at all, i.e., why hasn’t it simply been discarded?

The answer is, as I would suggest, that the CL model has remained useful in various ways, even after its “refutation”. For one thing, it is still frequently taken to be a suitable starting point for introducing students to the explanation debate (as in Pitt 1988); similarly for giving retrospective accounts of the debate’s development (cf. Salmon 1990, Psillos 2007, etc.). Along less historical and more systematic lines, Hempel’s model has continued to play the role of a useful object of comparison too. As Philip Kitcher puts it:

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 (Kitcher 2001, 156).

And with the current situation in the explanation debate in mind, he adds:

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 (*ibid.*, 158).

In passages such as these, the CL model is put forward as exemplary for how philosophy of science, or analytic philosophy more generally, is to be done. Likewise, but with an opposite valence, one can try to use the CL model for illustrating the limitations of analytic philosophy, of formally oriented approaches more generally, or of Carnapian explication in particular, at least if they are understood too narrowly (Reck 2012). Finally, might it even be possible to argue that, by locating generality at the core of explanation, there is something right about the CL model, something to be rescued, even if Hempel articulated it in a misleading way?¹⁴

In the last two sections I considered reasons for interpreting the CL model as an explication in Carnap’s sense. This leads to insights concerning the model’s significance, as I argued, and to a

¹⁴ Note, moreover, that outside of philosophy something close to the CL model is still often taken for granted when people talk about scientific explanation, especially in the natural sciences.

more adequate evaluation of it. Now I want to turn the tables—at least to some degree or in a certain sense. That is to say, I want to challenge, or at least to refine significantly, a Carnapian interpretation of the CL model. I also want to reconsider Hempel’s attitude towards Carnap’s philosophical methodology more generally. In the end the situation is more complex and more interesting, both with respect to Hempel and the CL model. Let us start with Hempel.

A first observation in that connection is that, while Hempel was indeed close to Carnap at certain points in his career, including in the late 1930s and the 1940s, there were other influences on him too. Hempel met Carnap in 1929, while spending a semester in Vienna as a student. But not only Carnap had an impact on him then, other members of the Vienna Circle did so too, especially Otto Neurath (Friedman 2000, Wolters 2003). And while in Berlin, Reichenbach influenced Hempel’s research strongly, as evidenced by his acknowledgment of the role probabilistic laws play in science, a point often highlighted by Reichenbach. More proximately for present purposes, there was Hempel’s collaboration with Paul Oppenheim. Commentators uniformly mention the latter as a co-author of “Studies in the Logic of Explanation”; but the general tendency is to ascribe most of the ideas in this essay to Hempel. Might there not have been more to Oppenheim’s input? Note, for example, that the notion of explanation is much less central in Carnap’s, Neurath’s, or Reichenbach’s writings than in Hempel’s. The specific focus on that notion, also the strong emphasis on “covering laws”, as well as their application to history and the social science could thus well be due to Oppenheim, at least in part.¹⁵

Such additional influences on Hempel await further exploration.¹⁶ But we can already note now that he did not remain a strict Carnapian later on in his career. As a first piece of evidence, consider Hempel’s answers to some related questions in an interview with him from 1982–1983. In that context he states the following about the goals and the methodology of the philosophy of science: “[We must] come very close to what we find as a matter of fact in the actual research activities of scientists” (Hempel 2000a). Similarly, in one of his later published articles, entitled “On the Cognitive Status and the Rationale of Scientific Methodology” (1988), he declares:

¹⁵ It is worth adding here that Oppenheim didn’t just collaborate with Hempel but with other philosophers as well (including Kurt Grelling, Olaf Helmer, Nicholas Rescher, and Hilary Putnam). Moreover, often these collaborations involved working out Oppenheim’s ideas (cf. Rescher 2005a).

¹⁶ For further forays in that direction, compare the two essays by Nikolay Milkov in this volume.

[An explicatory theory] should not just prescribe norms for rational research procedures but should also have the potential for providing at least an approximate descriptive and explanatory account of some aspects of actual scientific practice (Hempel 1988, 209).

Such declarations are far from Carnap's relaxed attitude towards descriptive accuracy, as part of his more normative and revisionist methodology. It is tempting to read the last quotation even as a direct rebuttal, or disavowal, of Carnap's relatively cavalier stance towards "similarity" in explication. But maybe that is reading too much into the passage.

Beyond such evidence, it is well known that Hempel was influenced by Thomas Kuhn's work in the history and sociology of science later in his career (from the mid-1960s on), partly also by Quine's philosophical naturalism.¹⁷ It may be that encountering their approaches reawakened the influence of Neurath in him, who had emphasized sociological aspects in the study of science and promoted his own form of naturalism earlier.¹⁸ Hempel's parallel interactions with more descriptively oriented philosophers of science, such as Michael Scriven and N. R. Hanson, might also have played a role in his increasing emphasis on staying close to "the actual research activities of scientists". In those respects, the development of Hempel's views illustrates broader trends in the philosophy of science, from the 1960s to the 1980s. But actually, even in his earlier, classic work on explanation Hempel displays a significant amount of attention to examples and to scientific practice already. Insofar as that is the case, seeing Hempel and the CL model purely in the light (or in the shadow?) of Carnap is too quick and somewhat misleading.

Finally, an aspect of "Studies in the Logic of Explanation" (1948) that tends to be overlooked might be even more interesting here. In that essay it is the last part, in which Hempel and Oppenheim develop their "more rigorous analysis of the concept of law", that makes it look most Carnapian. Yet what is usually discussed under the label "CL model" in the literature—essentially the Hempel-Oppenheim explanation schema, divorced from their formal account of laws—occurs much earlier, namely right after the survey of motivating examples. What should

¹⁷ Hempel started interacting with Kuhn in 1963-1964, when both spent time at the Center for Advanced Studies in the Behavioral Sciences in Palo Alto. Subsequently, they became colleagues at Princeton. Quine's views were very prominent in the US during the 1960s and later, of course.

¹⁸ As Michael Friedman reports, Hempel himself later talked about "his conversion from the point of view of Carnapian 'explication' or 'rational reconstruction' to the point of view of Kuhnian historical and sociological naturalism as a return to Neurath's original conception" (Friedman 2000, 45).

one say, then, about that schema, especially from a Carnapian perspective: Is it part of the “clarification of the explicandum”, like the initial discussion of examples; or is it part of the formal explicatum instead? The answer is not clear, it seems to me, since the CL schema hovers somewhere in-between these two sides. And insofar as that is the case, it constitutes yet another non-Carnapian side of Hempel and “the CL model”.

At this point I want to reconsider the CL model one more time, from a slightly different angle, and so as to give my interpretation of Hempel yet another twist. My cue now is the fact that this account of scientific explanation is almost uniformly called a “model”. Usually not much is made of that fact; but might it not deserve separate attention? In my discussion so far, I contrasted two general perspectives on the Hempel-Oppenheim account: seeing it as a reductive analysis, thus as aiming at necessary and sufficient conditions for being an explanation; and seeing it as a Carnapian explication, to be evaluated pragmatically and not, or not primarily, in terms of descriptive adequacy. The CL model is very vulnerable to counterexamples if we adopt the first perspective, while these examples may be discounted to a considerable degree if we take up the second perspective. However, one might think that neither perspective is entirely adequate, since both lead to significant distortions. Is there no third alternative? I now want to indicate, at least briefly, that there is indeed room for such an alternative, or for an in-between position that might be truer to the CL account and is interesting in its own right.

So far we encountered two reasons for why the Hempel-Oppenheim account should not be seen as straightforwardly descriptive: it involves “abstraction and logical schematization”; it might be seen more as a useful tool, along Carnapian lines, than as a faithful description of scientific practice. Recall also that, even after its demise as the “received view”, the CL account has continued to be used fruitfully as an “object of comparison”. All three points suggest to me a comparison to the use of models in scientific research. What I have in mind here is not so much “models” in the sense of mathematical logic (set-theoretic structures), but, say, the Bohr model of the atom, Maxwell’s vortex model for the electromagnetic field, and similar models in biology and the social sciences. Just like the CL account, models in that sense involve idealization; they too are primarily tools; and here again, an old model may profitably be compared to a newer one even after its demise. In recent philosophy of science there has been a significant amount of

discussion concerning scientific models; the corresponding literature can thus be taken as a reference point (see, e.g., Morgan and Morrison 1999 and Bailer-Jones 2008).

Within the philosophy of science interest in the topic of models arose as part of the move from a “syntactic” to a “semantic” view of scientific theories. This move was meant to shed light on certain aspects of scientific research, especially current research, which would have remained obscure otherwise. What I am suggesting is a parallel shift with respect to the CL model. And in that case, the shift involves getting clearer about certain aspects of philosophical research. This is not to say that conceiving of the CL model as an explication, rather than as a reductive analysis, is not illuminating at all. Still, bringing in the notion of model can help in correcting distortions introduced by that conception. In particular, it provides a way in which the descriptive dimension of the CL model can be taken more seriously after all. My new suggestion is this, then: Hempel and Oppenheim’s model is descriptive of scientific practice in roughly the same (indirect, complex) way in which scientific models are representative of corresponding phenomena; and saying that is compatible with accepting, indeed emphasizing, its role as a tool, also with idealization and abstraction, etc.¹⁹

If this suggestion is on the right track—if it is appropriate to conceive of the CL account as a model in something like the scientific sense—the insights gained may apply more broadly, i.e., with respect to philosophical approaches beyond Hempel’s. However, I do not mean to suggest that every treatment of a philosophical problem, or every case of philosophical “analysis”, can and should be re-described as the use of a model; yet perhaps some can (including appeals to the unification “model”, the causal “model”, etc.). Moreover, the CL model may serve as a paradigmatic example here too, thus adding yet another dimension to its continuing usefulness. Actually, I suspect that significant differences in the uses of models—between science and philosophy, comparing different cases within each discipline, etc.—will emerge along such lines. For example, the CL model seems to be more a meta-theoretic tool than, say, the Bohr model; it also appears to be normative in a different sense.²⁰ These are all initial, rough-and-ready suggestions, of course. Much more will have to be done, in terms of thinking through their

¹⁹ For a survey of ways in which scientific models are representative, cf. Bailer-Jones 2008, chapter 8.

²⁰ For a few more comments on the meta-theoretic and the normative role of the CL model, cf. Reck 2012. For comparisons of different kinds of models within science (physical, mechanical, set-theoretic, etc.), see again Morgan and Morrison 1999, Bailer-Jones 2008, and the literature referred to in them.

implications, to make my over suggestion really convincing, as I am well aware. My hope is that I have said enough to make doing so look like a potentially profitable project, with respect to the CL model and more broadly.

If anything has become evident in this essay, it should be that the Hempel-Oppenheim model for scientific explanation is not as straightforward to categorize or evaluate as one might have thought initially. Often it is taken to constitute a strong kind of analysis and, as such, it is the target of various “counterexamples” intended to refute it. Yet that seems not entirely fair to the approach. A more appropriate way to conceive of it is arguably as an explication in Carnap’s sense. Indeed, there are several good reasons for doing so. But in the end that conception seems also distorting in certain ways. In particular, it downplays the model’s descriptive side too much. As a third, and in some ways intermediate, alternative I suggested viewing the CL model as functioning like a model in science, similar to the Bohr model of the atom, say. Admittedly, I did not spell out this alternative in any detail here. Nor has anyone else done so in the literature until now, as far as I am aware, in spite of the fact that it is almost universally called the CL “model”. My suggestion was that it may be worth doing so, also beyond the case of Hempel.

In conclusion, let me return to Hempel’s stature as a philosopher. I started out this essay by noting that Hempel is typically not regarded as a thinker of the same caliber as, say, Reichenbach and Carnap. Nevertheless, he too had a strong and lasting influence in philosophy, especially with his work on scientific explanation. As Nicholas Rescher wrote aptly:

[Hempel & Oppenheim’s “Studies in the Logic of Explanation” was] one of those unusual publications that set the agenda for a whole generation of investigators. It set in train an enormous body of discussions and publications which shaped the course of deliberations about scientific explanation over the next decades [...] (Rescher 1997, 334)

Similarly, James Fetzer has talked about Hempel’s “enormous influence”, especially in the English-speaking world; as Fetzer puts it, “during his two decades at Princeton [1955–1975], Hempel’s approach dominated the philosophy of science” (Fetzer 2010, 12). It seems to me that such claims about the significance of Hempel’s contributions, while somewhat partisan, are not indefensible. Indeed, one goal of the present paper was to establish that fact. At the same time, it remains true that Hempel was not as original and radical as Reichenbach or Carnap, including

methodologically, which justifies granting them an even higher status in the pantheon of twentieth-century “scientific philosophers”. Perhaps for that very reason, Hempel’s approach was easier to emulate by others, and that may have contributed to his widespread influence.

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