Hempel, Carnap, and the Covering Law Model
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Carl G. Hempel (1905-1997) is usually not seen as 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 an important influence on him. Yet the impact of Hempel's philosophical work was almost as widespread and lasting as theirs, especially 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 1937-38, to work as Carnap's research assistant at the University of Chicago. He came back in 1939, as a refugee, and stayed permanently. His first full-time teaching position was at the City College of New York. From there he moved to Yale, in 1948, to Princeton, in 1955, and finally to the University of Pittsburgh, from 1976 to 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 the President of the American Philosophical Association. In retrospect, it seems right to say that he was "one of the principal figures of scientific philosophizing in the 20th century" (Rescher 2005a, p. 127).

Hempel's main contributions to philosophy concern the philosophy of science. He is most well known for his writings on the notions of confirmation, explanation, rationality, and cognitive significance, among others. 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

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Salmon, another major contributor to the corresponding debate, characterized as "truly epoch making" (Salmon 2000). Concerning Hempel's subsequent, related collection of essays, *Aspects of Scientific Explanation* (1965b), James Fetzer has remarked that it "became a scholar's bible for generations of graduate students" (Fetzer 2010, p. 1). Similarly, Hempel's textbook, *Philosophy of Natural Science* (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 fundamentally, 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 elaborated.

* "Studies in the Logic of Explanation" (1948) is not the first publication in which the Covering Law Model (CL model, for short) appeared. Its core idea had been suggested by other philosophers, e.g., by Richard Braithwaite, Karl Popper, 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 already came up in "The Function of General Law in History" (1942). Nevertheless, it is the 1948 article that really set the stage for later discussions.4 Its main theme is stated thus:

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

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

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\begin{align*}
C_1, C_2, \ldots, C_k & \quad \text{Statement of antecedent conditions} \\
L_1, L_2, \ldots, L_r & \quad \text{General laws} \\
\hline E & \quad \text{Description of the phenomenon to be explained}
\end{align*}
\]

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, p. 10).
The "basic pattern of scientific explanations" is thus: \( E \) (the "explanandum") is deduced logically from \( C_1, C_2, \ldots, C_k \) and \( L_1, L_2, \ldots, L_r \) (the "explanans"). The two authors go on to spell out several additional requirements for explanations, 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 \( C_1, C_2, \ldots, C_k \) and \( L_1, L_2, \ldots, L_r \); (ii) at least one general law "must be required for the derivation"; and (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 this: (iv) "The sentences constituting the explanans must be true", so that the argument is sound (ibid., pp. 569-70). Hempel and Oppenheim also argue that, because of their underlying logical form, there exists a "symmetry" between explanation and prediction in science. And in later 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 there are also explanations in science based on statistical or probabilistic laws. In many of them, the explanandum is not a deductive consequence of the explanans, but follows only with a certain probability. Thus, strictly speaking the schema above applies 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 that case, we are dealing with "deductive-statistical" explanations. Then again, in all three cases the explanandum is subsumed under, or "covered" by, general laws; and hence, what is crucial about scientific explanation in general 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 provided in the 1948 essay. The most mature and systematic treatment of this position occurs in
"Aspects of Scientific Explanation" (1965), the centerpiece of Hempel (1965b), while a simpler, more accessible discussion lies at the heart of Hempel (1966).

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It took several years after the publication of Hempel & Oppenheim (1948) for the CL model to attract much attention. But from the 1960s on, it became a central, entrenched part of "scientific philosophy"—it became the "received view" on scientific explanation, the position against which all alternatives were measured. Why did it have such an impact? Hempel's steadily increasing personal influence, i.e. his recognition as a main player in the field, was important, no doubt. But there were more philosophical reasons as well, including the following two: First, Hempel & Oppenheim's careful, formally precise treatment rehabilitated the notion of explanation among scientifically oriented philosophers. Perhaps surprisingly 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 scientific laws was carefully crafted to get around Humean scruples concerning the notion of causation as shared by many empiricists.\(^6\) 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 seen as leading to substantive philosophical progress.

It wasn't just among empiricist philosophers that the CL model was much noted and admired. It also exerted a strong influence outside philosophy, on disciplines such

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\(^5\) 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 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, p. 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.
as history and some of the social sciences, where it was taken to be normative, i.e., as telling researchers to produce explanations of CL form. But its alleged universal applicability was soon called into question. (Eventually it came to be seen as a central part of the "positivist" approach to take ideas and methods from one field, namely mathematical physics, and impose them on others, in ways that are counterproductive; but that took a while.) Within 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 was shown to lead to paradoxical consequences. And while it may have appeared at the time that some minor tinkering would get around these problems, further criticisms of the CL model soon 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.)—challenged the CL model in other ways as well. Some called into question Hempel and Oppenheim's "symmetry thesis" for explanation and prediction. Others were meant to establish, very basically, that for a scientific account to be explanatory it was neither necessary nor sufficient to have CL form.

While the CL model kept having defenders, including Hempel himself (who, in particular, tried to improve on his treatment of inductive-statistical explanations), 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

7 Cf. the discussion of archeology in Salmon (1990), pp. 25-26. Hempel had applications to history in mind from early on, as the initial presentation of the CL model in Hempel (1942) shows.

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

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).
Michael Friedman and Philip Kitcher. In some respects these two models were not outright rejections of the CL model, but modifications of it (especially the unification model). But 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, an informal, contextual approach to explanation championed by Michael Scriven (guided by a radically different methodology).\textsuperscript{11} It seems fair to say that, as a result of the proliferation of alternative approaches, there isn’t a "received view" about scientific explanation today, although causal models tend to be more prominent than others. Some philosophers might even argue that it is misguided to look for a universal model in the first place; what is needed, instead, is a plurality of models, since explanations come in a variety of forms.

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


\textsuperscript{12} An authoritative recent discussion of the topic, as presented by the proponent of a causal account, can be found in Woodward (2003). For an overview, see also, e.g., Psillos (2007).
science. This is what the significance of the CL model is taken to amounts to. It is just that the analysis it embodies does not work, as the counterexamples show.

Two common reactions to the resulting situation are the following: First, one can hold on to the goal of providing a reductive analysis, and in particular, of articulating necessary and sufficient conditions for explanation. Thus, while it may be true that the Hempel & Oppenheim's model itself doesn't work, one can try to modify it or replace it by a better analysis (along causal or unification lines, say). A second and more radical reaction is to take the "counterexamples" to the CL model as having 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. This doesn't necessarily mean that we have to give up analyzing the notion of explanation; but we should proceed in an informal, non-reductive way. These two common reactions are quite different; indeed, they are opposed to each other. But they share an important assumption about the CL model: that it has been refuted, in some fairly direct way, by the "counterexamples"; or more specifically, that the model has been refuted by the careful description of scientific practice.

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

> These 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, p. 412)

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

13 Of the two, the first reaction is much more common in the literature on scientific explanation; even van Fraassen's model can be seen to fall into this first camp. I take Michael Scriven's approach to be an example of the second kind of response; 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 (forthcoming), and for another representative of Scriven's camp, cf. Wright (forthcoming).
and "schematization", as emphasized in this passage. If one takes it to provide a reductive analysis of the notion of explanation, one misrepresents its nature and purpose in a more fundamental way—or so the argument I now want to consider. Then again, if the CL model is not meant to be a reductive analysis, how else could we think about it? A first answer to that question is provided by Carnap’s notion of explication. (I will add a second, somewhat different answer later in the essay.)

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> 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, pp. 7-8; original emphasis).

If one adopts Carnapian explication as one’s methodology, it does lead to abstraction and schematization, along Hempel’s lines. But descriptive accuracy is rejected, or at least downplayed, in a stronger sense as well. That sense is flagged by Carnap’s talk of "replacing" an earlier, vague concept by a new, more exact one. Here Carnap points towards the fact that the main thrust in giving an explication, as conceived of by him, is revisionary and normative, not descriptive. In this and some related respects, explication is significantly different from reductive analysis.

Two main parts of the difference at issue are the following: First, in an explication we start with a vague notion and replace it by a more exact one; but then, it is misguided to judge the new notion in terms of whether it "fully captures" what was there before. Second, what the new notion should be judged by, instead, is its usefulness. Thus 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, p. 4)

Shortly afterwards, he list four criteria for evaluating an explicatum: "(1) similarity to the explicandum; (2) exactness; (3) fruitfulness; (4) simplicity" (p. 5). To be sure, "similarity" is one of the desiderata listed here; but there are three others, which typically bear more weight (in Carnap's as well as later applications of explication). Also, by only requiring "similarity", in a sense left fairly unspecified, descriptive adequacy with respect to earlier practice is only required in a weak sense.

Now, there are several good reasons for seeing the CL model, and Hempel's approach more generally, as an instance of Carnapian explication. First, 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, in particular). Second, there were direct 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). Third and 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). Fourth, central participants in the debate about the CL model described 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, p. 35). And finally, Hempel mentions Carnap and the notion of explication explicitly, and admiringly, in some of his later reflections (Hempel 1973, 1988).

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Suppose then that we interpret the Hempelian CL model as a case of explication in Carnap's sense. What exactly follows about that model and how to evaluate it? As already noted, for Carnap "similarity" between the explicatum and the explicandum is a desideratum, but one that plays a minor and subordinate role. Moreover, the
only guidance with which he provides us in that 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, p. 1).

Notice here the emphasis on "intended use" which signals what is really crucial: for Carnap, the evaluation of an explicatum is thoroughly pragmatic in the end. If it serves our purpose, its adoption is justified, even if this means discarding much of the old, vague "meaning" in the process. But if that is the attitude, one may ask the following question: What exactly is the purpose, or what are the purposes, in play? Neither Hempel nor Carnap says much in that connection, partly because a thorough discussion of goals, or of normativity more generally, 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 seems crucial for us.

Let us assume, for the moment, that the main 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. Moreover, all the putative "counterexamples" are directly relevant. On the other hand, the force of the usual criticisms appear to be considerably weaker if what we are aiming for 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 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, may the right kind of similarity not play a crucial role for the effectiveness of the explicatum, as it takes over the role of the explicandum, in science as well? Likewise, might it not play an important role in philosophy, depending on which questions we ask there? More particularly, it would seem that, at some point in the process, there has to be a careful evaluation of whether and how much the "abstraction and logical schematization" involved in an
explication, such as the CL model, do serve our purposes, whatever those are.  

Suppose then that, at least for some explications, questions about their descriptive accuracy, about the appropriateness of certain idealizations, etc. remain. Arguably is still the case that an explication cannot be refuted by examples in any strict sense, since it is not meant to be right or wrong, only more or less useful, as Carnap emphasized. This applies in particular 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, then, to clarify the model's significance. It also allows us to make sense of what has happened since various alternatives to the CL model have taken center stage, thus depriving it of its status as "the received view". Assume here, as most contemporary philosophers of science do, that one or several of the counter-models are superior to it, in one way or another. In that case the question becomes: Why are we still talking about the CL model at all; why hasn't it simply been discarded? The answer is that the CL model has remained useful in a variety of 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), or for giving a retrospective account of the debate's development (in Salmon 1990, also in more recent accounts). In more systematic ways too, Hempel's model has continued to play the role of a useful object of comparison. 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, p. 156).

And he adds the following about the current situation:

> 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., p. 158).

In these passages, Kitcher takes the CL model to be exemplary for how philosophy of

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14 Some of these critical themes are explored further in Part 3 of Reck (forthcoming).
science, or analytic philosophy more generally, is to be done. In contrast, but along parallel lines, one can use the model for illustrating the limitations of analytic philosophy, of formally oriented approaches more generally, or of Carnapian explication understood in an overly narrow sense (cf. Reck forthcoming). Finally and more positively again, might it still 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?15

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In the last two sections I considered reasons for interpreting the CL model as an explication in Carnap's sense. Doing so does lead to insights into the model's significance and to a 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 and refine a Carnapian interpretation of the CL model, as well as reconsider Hempel's attitude towards Carnap's philosophical methodology more generally. In the end, the situation is more complex and more interesting, it seems to me, both with respect to Hempel and the CL model. Let us start with Hempel.

A first observation in this connection is that, while Hempel was indeed close to Carnap at certain points in his career, including the late 1930s and the 1940s, there were other influences on him as well. Hempel met Carnap in 1929, when he spent a semester in Vienna as a student. But it was not only Carnap who had an impact on him then; other members of the Vienna Circle did so too, especially Otto Neurath (Friedman 2000). Beyond that, Reichenbach undoubtedly influenced Hempel's research, as evidenced in our context by the acknowledgment of the role probabilistic laws play in science, a point often emphasized by Reichenbach. And more proximately for us, there was Hempel's collaboration with Paul Oppenheim.

\[\text{\footnotesize\textsuperscript{15}}\] One might add 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.
While commentators uniformly mention him as a co-author of "Studies in the Logic of Explanation", the general tendency is to ascribe most of the ideas in this classic article to Hempel. But might there not have been more to Oppenheim’s input? One can note here, 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 it, the strong emphasis on "covering laws", as well as the related interest in history and the social science might well be due in part to Oppenheim.16

These additional influences on Hempel await further exploration. We can already observe now, however, that he did not remain a strict Carnapian with regard to methodology later on in his career. This is evidenced by his answers to questions in an interview from 1982-83. He states there, among others, 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 a late published article, entitled "On the Cognitive Status and the Rationale of Scientific Methodology" (1988), he declares:

> [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, p. 209)

Such statements are pretty far from Carnap’s attitude towards descriptive accuracy and his more revisionist methodology. It is tempting to read the last passage even as a direct rebuttal, or a disavowal, of Carnap's relatively cavalier stance towards "similarity" in explication; but maybe that is reading too much into this remark.

Beyond such passages, it is well known that Hempel was influenced by Thomas Kuhn's work in the history and sociology of science during later stages in his career.

16 For more on other influences on Hempel, including Oppenheim, cf. Nikolay Milkov's contribution to this volume. It might be worth adding that Oppenheim didn't just collaborate with Hempel, but also with other philosophers (including Kurt Grelling, Olaf Helmer, Nicholas Rescher, and Hilary Putnam), and often these collaborations involved working out his own ideas; cf. Rescher 2005a.
(from the mid-1960s on), partly also by Quine's philosophical naturalism.\textsuperscript{17} It seems that encountering their approaches reawakened the influence of Neurath, who had emphasized sociological aspects in the study of science and represented his own form of naturalism already earlier.\textsuperscript{18} In addition, Hempel's interactions with more descriptively oriented philosophers of science in the US, such as Michael Scriven and N. R. Hanson, might have played a role in his increasing emphasis on staying close to "the actual research activities of scientists". In these respects the development of Hempel's views is illustrative of broader trends in the philosophy of science, from the 1960s to the 1980s. But actually, even in his earlier works, including his classic writings on explanation, Hempel displays a significant amount of attention to examples and to scientific practice, more so than Carnap in his applications of explication. 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, seldom noted detail of "Studies in the Logic of Explanation" (1948) seems worth mentioning here as well. 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 their approach look most Carnapian. But what is usually discussed under the label "CL model"—essentially the Hempel-Oppenheim explanation schema, divorced from their formal account of laws—occurs much earlier in the essay, namely right after the survey of motivating examples. What should one say, then, about that schema 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—the CL schema hovers somewhere in-between these two sides of an explication. And insofar as that is the case, it

\textsuperscript{17} Hempel started interacting with Kuhn in 1963-64, when both spent time at the Center for Advanced Studies in the Behavioral Sciences in Palo Alto. Later they became colleagues at Princeton. Quine's views were rather prominent in the US during the 1960s and later, of course.

\textsuperscript{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, p. 45).
constitutes another non-Carnapian side of Hempel and of "the CL model".

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Let me reconsider the CL model one more time, now from a slightly different angle, and so as to give my interpretation of Hempel another twist. My cue, at this point, is the fact that this account of scientific explanation is almost universally called a "model". Usually not much is made of that fact; but might it not deserve some attention? In my discussion so far, I contrasted two general perspectives: seeing the Hempel-Oppenheim account as a reductive analysis, thus as aiming at necessary and sufficient conditions for something being an explanation; and seeing it as a Carnapian explication, to be evaluated pragmatically and not, or at least not primarily, in terms of descriptive adequacy. The account is highly 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. But one might think that neither is entirely adequate, since both lead to distortions. Moreover, I now want to indicate, very briefly, that there is room for a third alternative—an in-between position that is more true to the CL account and interesting in itself.

So far we encountered two reasons for why the Hempel-Oppenheim account should not be seen as straightforwardly descriptive: First, it involves "abstraction and logical schematization", thus idealization. Second, we can see it more as a useful tool, along Carnapian lines, than as a faithful description of scientific practice. In addition recall 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 comparing it and its role 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 rather, 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 for research; and here again, an old model may profitably be compared to a
newer one even after it has been discredited in certain ways. In the philosophy of science, there has been a significant amount of debate about such scientific models during the last decade or more, and the corresponding literature is my reference point here (cf., e.g., Morgan & Morrison 1999 and Bailer-Jones 2008).

Within the philosophy of science, the relevant discussions were part of the move from a "syntactic" view of scientific theories to a "semantic" view. 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 account. And in our case, the shift involves getting clearer about aspects of philosophical research as well. This is not to say that conceiving of the CL model as an explication, rather than as a reductive analysis, isn’t justifiable and illuminating at all. Still, bringing in the notion of model can help in correcting distortions introduced along the way. In particular, it suggests a sense in which the descriptive dimension of the CL model might be taken seriously after all. Namely, it is descriptive of scientific practice in roughly the same (indirect, complex) way in which scientific models are representative of corresponding phenomena. And this is compatible with its role as a tool, with idealization, etc.  

If this 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 thereby may apply more broadly and systematically, i.e., with respect to philosophical approaches beyond Hempel. I am not suggesting 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. But perhaps some can (including appeals to the unification "model", the causal "model", etc.); and the CL model may serve as a paradigmatic example here too, thus adding another dimension to its continuing usefulness. I suspect that significant differences in the use of models—between science and

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19 For more on the ways in which scientific models represent, cf. Bailer-Jones (2008), chapter 8.
philosophy, also between different cases within each discipline—will emerge as well. For example, the CL model seems much more of a meta-theoretic tool than, say, the Bohr model; it also appears to be normative in a different way.\textsuperscript{20} Now, these are all initial, rough-and-ready suggestions, as I am well aware. Much more would have to be said, in terms of thinking through their implications, to make any of them really convincing. Then again, I hope I have said enough to make doing so look like a potentially profitable project, with respect to the CL model and beyond.

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If anything has become evident in this essay, it is perhaps that the Hempel-Oppenheim model for scientific explanation is not as easy to categorize or to evaluate as one might have thought. Often it is taken to constitute a strong kind of analysis; and seen as such, it was the target of various "counterexamples" intended to "refute" it. But that seems not entirely fair to the approach. A more appropriate way to conceive of it is, arguably, as constituting an explication in Carnap's sense. There are several good reasons for doing so. Yet in the end, this interpretation is also distorting in some ways. In particular, it downplays the model's descriptive side too much (as applications of Carnap's notion of explication tend to do more generally). As a third, intermediate alternative, I suggested thinking of the CL model as functioning like a model in science, similar to the Bohr model of the atom, say. However, I did not spell out this alternative in any detail. Nor has anyone else in the literature, as far as I am aware, in spite of the fact that it is almost universally called the CL "model". Much work remains to be done on that count.

In conclusion, let me return briefly to Hempel's stature as a philosopher. I started out by saying that Hempel is typically not regarded as a thinker of the same caliber as, say, Reichenbach and Carnap. Nevertheless, he had a strong and lasting

\textsuperscript{20} For more on the meta-theoretic and normative role of the CL model, cf. Reck (forthcoming). For the use of different kinds of models in science (physical, mechanical, set-theoretic, etc.), see again Morgan & Morrison (1999), Bailer-Jones (2008), and the literature referred to in them.
influence, especially with his work on scientific explanation. Reflecting on that influence, Nicholas Rescher has made the following historical observation:

[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, p. 334)

Along similar lines, James Fetzer has recently talked about Hempel’s "enormous influence" in philosophy, especially in the English-speaking world; indeed: "During his two decades at Princeton [1955-1975], Hempel’s approach dominated the philosophy of science" (Fetzer 2010, p. 12). It seems to me that such claims about the significance of Hempel’s contributions are apt, and one goal of the present essay was to establish that fact. At the same time, it is probably fair to say that he was not as original and radical as Reichenbach or Carnap, also 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, which may have contributed to his widespread influence.

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