

Carnapian Explication: A Case Study and Critique¹

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During the last two decades, there has been a revival of interest in Rudolf Carnap. This has led to the revision of various stereotypes about him, e.g., of the view that his first major work, *Der Logische Aufbau der Welt* (1928), represents a crude form of positivism or the culmination of classical empiricisms. Careful historical studies of the *Aufbau* have, instead, brought to light its neo-Kantian and Husserlian roots, thus leading to a much subtler, more complicated story about the book's origins and goals. Another result of recent scholarship has been a shift of focus from the early *Aufbau* to Carnap's middle and later writings. Thus, his *Logische Syntax der Sprache* (1934d/1937) has been rediscovered as a major contribution to the philosophy of mathematics and logic, and Carnap's writings from the 1940s and 1950s have come into focus as well. One result of the latter shift is a renewed interest in his notion of explication, together with pragmatist elements in Carnap's mature philosophy.²

In this essay, I want to contribute to this last development, and especially, to the reconsideration of Carnap's notion of explication. My main goal will be to subject that notion, and Carnap's corresponding proposal for how to do philosophy, to critical examination. Besides contributing to Carnap scholarship, there are three motivations for such a task. First, I take the notion of explication to be one of Carnap's most important contributions to philosophy, one that it is of intrinsic significance. Second, a considerable amount of 'formal philosophy' is still informed by it today, explicitly or implicitly, so that it seems important to examine its nature and reach.³ Third, it certainly had a strong impact on analytic philosophy during the second half of the twentieth century. The latter will be illustrated by a case study involving a series of attempts to explicate the notion of scientific explanation, beginning with C. G. Hempel's well-known Covering Law model. Comparing these attempts will, in turn, shed light on the strengths and weaknesses of explication.

I will proceed as follows. In the first section of this essay I will introduce the notion of explication on the basis of Carnap's writings, including raising

some initial questions about it. In the second section, my case study will be presented, with its focus on two aspects: ways in which Hempel's model of scientific explanation can be seen as an example of Carnapian explication, and subsequent criticisms of it that reflect back on explication. Included in the latter will be the discussion of four alternative accounts of scientific explanation that represent more or less radical departures from explication. In the third section I will pick up on both my initial questions and themes from the case study to articulate my critique of explication further. Overall, my goal is to identify certain 'blind spots' in Carnap's methodology, i.e. limitations that are built into it systematically, that are not impossible to address from a Carnapian perspective, but that lead far beyond it if taken seriously.

Part 1: Carnapian Explication

(1a) Carnap's characterization of explication

While precursors of the notion, such as that of rational reconstruction, occur already in some of Carnap's earlier works, the notion of explication itself, in its full form, is first discussed in his book *Meaning and Necessity* (1947). There he states:

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)

More succinctly, Carnap writes in *Logical Foundations of Probability* (1950):

By an *explication* we understand the transformation of an inexact prescientific concept, the *explicandum*, into an exact concept, the *explicatum*. (Carnap 1950b, p. 1)

Carnap's main goal in these two books is, then, to provide explications for certain further notions. In his 1947 book, the two primary targets for explication are meaning and necessity, as indicated by its title. In the 1950 book, it is the notions of logical probability and of degree of confirmation that are at issue. Retrospectively, it is also clear that, in a series of Carnap's works from the 1920s to the 1950s, another central target for Carnapian explication is the notion of logical or analytic truth.

As Carnap's appeal to 'logical analysis' and 'logical construction' in our first quote indicates, he sees the task of providing explications as in line with what some earlier analytic philosophers did. And in fact, in *Meaning and Necessity* he lists three models for explication that reveal where he is coming from, namely: (i) Frege's and Russell's analysis of the natural numbers, where the explicandum is 'the term "two" in the not quite exact meaning in which it is used in everyday life and applied mathematics', while the explicatum is 'the class of pair-classes' as introduced by Frege and Russell; (ii) Frege's and Russell's works on definite descriptions, with 'phrases of the form "the so-and-so"' as the explicandum, and with the 'the various interpretations of descriptions by Frege, Russell, and others' as the explicatum; and (iii) Tarski's account of truth, where the explicandum is 'the concept of truth as used in everyday language [...] and in all of traditional and modern logic', and the explicatum is 'the semantic concept of truth' in Tarski's sense (Carnap 1947, p. 8).

The three paradigmatic examples just mentioned come from work in logic and the foundations of mathematics. But for Carnap, philosophy should also model itself on the natural sciences. Thus he mentions several additional paradigms in *Logical Foundations of Probability* (Carnap 1950b, pp. 5–6) that, as he puts it, originated in the 'actual procedure of scientists'. These involve the following transitions: from the prescientific 'fish' to the zoological notion of *piscis* (Carnap's term); from the prescientific 'salt' to sodium chloride (*NaCl*) in chemistry; and from our ordinary talk about 'warmer' and 'colder' to the quantitative notion of temperature. Yet another example mentioned by Carnap is the transition from the informal notion of dimension, as used in earlier stages of the development of mathematics, to Karl Menger's technical notion and theory of dimension.

(1b) Central aspects of explication

Beyond this brief characterization and motivating examples, several central aspects of Carnapian explication need to be made more explicit. The first is that, in giving an explication, the goal is not to capture a previously given 'concept', nor to spell out the 'meaning' of an ordinary phrase. Instead, a vague, pre-scientific explicandum is 'replaced by' (1947), or 'transformed into' (1950), an exact explicatum. Carnap basically rejects the assumption that there are concepts in some Platonic sense, existing 'out there', with which an explicatum could be compared. He also rejects the view that the 'meanings' embedded in ordinary practice are precise enough to be articulated in a fully determinate way; the point is, rather, to replace them with something tailor-made for more precise, systematic treatment. This distinguishes explication immediately from stronger forms of philosophical analysis.

A direct consequence of the fact that an explicandum is always vague and the corresponding explicatum more precise is that there can be no question

of whether an explication is right or wrong in any strict sense. Thus Carnap writes:

Strictly speaking, the question whether the solution 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 1950b, p. 4)

Not only is it always a matter of being 'satisfactory' or not, thus of pragmatic evaluation; it is also a matter of the comparative evaluation of explications, since there are usually several competing alternatives. In addition, implicit in the idea of explications being more or less satisfactory is that they need to be seen as relative to some purpose or purposes. But then, it is misguided to expect a best explication in any absolute sense. Taking these aspects together, Carnap's approach to explication can be seen to constitute a kind of pragmatic pluralism.

While an explicatum is not meant to be right or wrong relative to the original explicandum, the two are not totally independent of each other. In fact, Carnap requires the explicatum to be sufficiently 'similar' to the explicandum (Carnap 1950b, p. 5).⁴ As a consequence, any explication does involve a certain amount of attention to the explicandum to start with. In Carnap's own words:

There is a temptation to think that, since the explicandum cannot be given in exact terms anyway, it does not matter much how we formulate the problem. But this would be quite wrong. On the contrary, [...] we must [...] do all we can to make at least practically clear what is meant as the explicandum. (Carnap 1950b, p. 4)

And how is this making an inherently vague explicandum 'at least practically clear' to be achieved? Carnap suggests this procedure:

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. (*Ibid.*)

Noteworthy here is again that, for Carnap, the initial clarification involved in an explication is guided by the explicatum's 'intended use', thus pragmatically.

(1c) Further clarifications and initial questions

As we saw, an explication is meant to result in an explicatum that is exact, in contrast to the vague, inexact explicandum. But what does it mean to be

'exact' in this context? The answer to this question is somewhat implicit in Carnap's writings. A first indication of what it amounts to can be found in his well-known Principle of Tolerance, as formulated in his earlier *Logische Syntax der Sprache*:

In logic there are no morals. Everyone is at liberty to build up his own logic, i.e., his own form of language, as he wishes. All that is required of him is that, if he wishes to discuss it, he must state his methods clearly, and give syntactical rules instead of philosophical arguments. (Carnap 1937, pp. 51-2)

Here Carnap is talking about setting up different logical frameworks for studying the foundations of mathematics. What is rejected are 'philosophical arguments', presumably ones to the effect that one approach is 'right' while others are 'wrong'. What counts instead is to be 'clear' and, crucially, to give 'syntactic rules'. The relevance for Carnap's later work is this: individual explications are always given within some logical framework; and thus, their 'exactness' also has to do with the precision of syntactic rules. Of course, by the time of *Meaning and Necessity* Carnap has accepted semantics as well, not just syntax, as his appeal to Tarski's account of truth as a paradigm illustrates. But it is formal semantics that he has adopted, so that he has not moved away very far from the emphasis on logical syntax. Carnap's standard for 'exactness' remains to be provided by formal logic.

What is the motivation for this insistence on providing 'syntactic rules' and for the rejection of 'philosophical arguments'? Once more it has to do with modeling philosophy on the mathematical sciences. Another inspiration for Carnap in this context, besides those already mentioned, is David Hilbert's foundational work from the 1920s and 1930s. In Hilbert's metamathematics the goal is to turn philosophical questions about mathematics into mathematically tractable problems. The idea is that, when such a transformation is possible, one can apply precise mathematical techniques and establish results once and for all. For both Hilbert and Carnap, the contrast is with traditional philosophy, where the inexactness of the concepts and tools available makes progress hard to come by, if not impossible. Along such lines, the insistence on formal precision is an antidote to what one may call the 'infinite ambiguity' of philosophy, which contributes strongly to its inconclusiveness.

If this captures Carnap's notion of exactness and its motivation, it also leads to an immediate question. While the insistence on formal precision has proven its worth in certain areas, is it clear that it is equally appropriate in all others? Carnap was rightly impressed by early successes in mathematical logic (from Frege and Russell to Hilbert and Tarski); but do the tools of modern logic constitute a 'magic bullet', an approach that

is always applicable and most fruitful? Perhaps other kinds of cases will require that we enlarge or modify our toolbox. A first step in that direction would be to use, not only formal syntax and semantics, but other formal techniques as well. Yet sometimes we may need to go even further. This would not necessarily mean giving up the project of explication; but it could lead to narrower and broader senses of it. For certain purposes the insistence on formal 'exactness' might even be called into question more radically.⁵

Let me add two further questions about explication, again in a preliminary way. My second question concerns the following. It would seem that the issue of which tools are most appropriate is inseparable from what one's goal is. Earlier we saw that Carnap was quite aware of the purposive, pragmatic side of explication; indeed, he insisted on it. But what exactly is the purpose, or what are the purposes, in connection with explication? Carnap himself is less than fully explicit about this issue, and his motivating examples indicate a certain ambiguity in this respect. On the one hand, the goal in providing an explication may be to answer a philosophical question, and thus, to make progress with a philosophical project (cf. Frege's response to Kant's claim that arithmetic is synthetic *a priori*). On the other hand, it may be to advance science, including its practical usefulness (by, e.g., introducing quantitative ways of measuring temperature). Or is Carnap's idea that both philosophy and science should aim at improving our lives in the end, so that this is the ultimate goal?⁶ But even then one may ask: is it clear that philosophical, scientific, and practical usefulness always benefit from the same tools?

The issue of purposes also leads to my third initial question. Above we saw that, for Carnap, any explication involves the clarification of the explicandum; again, we need 'an indication of [its] meaning with the help of some examples for its intended use'. Now, insofar as the choice of examples is relative to one's 'intended use', this might again be quite sensitive to what one's purposes are. However, Carnap tends to treat this first step in a relatively quick, cavalier manner. Why does he give it such short shrift? Partly he does so because he wants to circumvent misguided questions about any precise fit between explicatum and explicandum; and this might look attractive since we avoid getting bogged down in imprecise questions and pseudo-problems. But can we really leave behind the vague, ordinary, or pre-scientific explicandum so quickly? (The force, or 'philosophical bite', of my initial questions, and especially of this third one, will become clearer as we go along.)

After having introduced the notion of explication along Carnap's own lines, and after having raised some preliminary questions about it, I now want to turn to my case study. It involves a series of accounts of scientific explanation.

Part 2: A Case Study

(2a) The Covering-Law Model of explanation

The starting point for my case study is C. G. Hempel's 'Covering Law model' (CL model) of scientific explanation. This model, or its core idea, was first presented in Hempel's 'The Function of General Law in History' (1942); but it found its classic expression in the subsequent essay, 'Studies in the Logic of Explanation' (1948), co-written by Hempel and Paul Oppenheim.⁷ The latter starts as follows:

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 the beginning part of their essay, Hempel and Oppenheim then present several examples of scientific explanation as motivation and to set the stage. After that, they suggest that all explanations can be seen as having the following form:

C_1, C_2, \dots, C_k (the statement of antecedent conditions)
 L_1, L_2, \dots, L_r (one or several general laws)

E (the description of an empirical phenomenon)

For Hempel and Oppenheim, 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').⁸ They go on to spell out some additional requirements for explanations, e.g., that the explanans must have 'empirical content' and be testable, also that the sentences of the explanans must all be true. Finally, in later parts of the essay they develop, among others, a 'more rigorous analysis of the concept of law'.

My basic suggestion is now to consider the CL model as an example of explication in Carnap's sense. There are several ways in which this can be justified. To begin with, Hempel was closely associated with Carnap just before he started publishing on the topic of scientific explanation.⁹ Also, Carnap is one of the people Hempel and Oppenheim thank for his input, in the first footnote of their essay. A more specific connection to Carnap becomes apparent in the third part of their essay. While the term 'explication' is not used prominently there, what its authors do corresponds quite closely to Carnap's procedure in his 1947 and 1950 books, namely: they restrict themselves specifically to syntactic and formal semantic means while developing their account of scientific laws. In addition, Carnap himself endorses a

version of the Covering Law model, e.g., in his later book, *An Introduction to the Philosophy of Science* (1966/1974, pp. 6–8). Finally, various people have referred to the CL model as a case of explication since then. Thus, in his well known survey, *Four Decades of Scientific Explanation*, Wesley Salmon states: 'The Hempel–Oppenheim article is an outstanding example of the use of an artificial language for the purpose of explicating a fundamental scientific concept' (Salmon 1990, p. 35).

While this justifies seeing the CL model as an example of explication, perhaps even as another paradigm case, I have further reasons for focusing on it. Most importantly, there has been a sustained and detailed debate in the philosophy of science, from the 1950s until today, concerning Hempel's model and several alternatives to it. By reflecting on that debate we can see how all three questions formulated above played themselves out in a real case. Also, I intentionally want to consider an example that goes beyond logic and the foundations of mathematics (although we are still dealing with scientific inference and judgment). This will allow me to pursue the issue of explication's general appropriateness further. Overall, my goal will not be to come to a final decision about scientific explanation, but rather, to use reflections on it for assessing Carnap's notion of explication.¹⁰

(2b) Problems with the CL model

The earliest criticisms of the CL model concern what, at the time, might have seemed like minor technical problems with the treatment of scientific laws in the third part of the Hempel and Oppenheim paper. And the initial response to those criticisms was some formal tinkering with that treatment.¹¹ However, soon additional, more basic arguments against the CL account surfaced, often in the form of 'counterexamples'. What these now classic examples (the flagpole, the pendulum, the moon and tides, the barometer, etc.), introduced by various prominent philosophers of science (S. Bromberger, M. Scriven, etc.) were taken to establish is this: It is neither necessary nor sufficient for being a scientific explanation to have CL form. As such, the examples were seen as refutations of the CL model, thus as requiring not just minor tinkering but major revisions, or even its replacement by an alternative.¹²

If we consider the CL model as an explication in Carnap's sense it is clear, however, that this way of looking at the situation is somewhat misguided. Again, it is a misunderstanding to think that an explication can be shown to be incorrect, in the sense of providing a descriptively false account, since it is not meant to be correct or incorrect in the first place, only more or less useful. In other words, strictly speaking there can be no counterexamples to an explication. Yet the more 'recalcitrant examples' for an explication there are, the more it becomes a question of how useful it actually is. Moreover, critics started to argue that even some of the examples provided by Hempel and Oppenheim, as part of their initial 'clarification of the explicandum',

were problematic, in the sense of not constituting genuine cases of scientific explanation. If so, this seems especially worrisome.

The result is this: even if the 'counterexamples', together with the challenges to Hempel and Oppenheim's own examples, don't refute the CL model outright; what they suggest is that the two authors paid insufficient attention to the explicandum. Yet, hadn't they followed Carnap's injunction to the letter: 'to make at least practically clear what is meant as the explicandum [...] with the help of some examples of its intended use', supplemented by some 'informal explanations'? In fact, the beginning section of 'Studies in the Logic of Explanation' contains arguably a fuller treatment of such examples than Carnap himself ever provided for his own cases. This points towards a deeper problem. It seems inadequate, in this and other cases, to handle the clarification of the explicandum by just providing a few examples together with a rough-and-ready explanation. Much more attention to the context and the practices surrounding the examples seems called for.

In any case, the reaction of most philosophers of science to the 'counterexamples' to the CL model was to start thinking about alternatives. Subsequently, three main counter-models were proposed: the 'causal model'; the 'unification model', and the 'formal pragmatic model' (each of them coming in several variants).

(2c) The three main alternatives

The main initial proponent of the causal model of scientific explanation was Wesley Salmon.¹³ One of Salmon's core suggestions is that Hempel and Oppenheim were too much, or too exclusively, committed to the tools of formal, deductive logic in their methodology (thereby exhibiting 'deductive chauvinism'). What we need instead is to bring in the notion of causation, even if that notion cannot be captured fully in formal-logical terms.¹⁴ What constitutes the explanation of a phenomenon is, then, that relevant causal factors or processes that lead to it are identified. Now, it would be too quick to conclude that Salmon was thereby giving up on Carnapian explication altogether, especially since he still tried to provide a quasi-formal account of causation (in terms of 'signal transmission'). Instead, his move leads, at least implicitly, to the differentiation of a narrow sense of explication, where only the tools of formal logic are allowed, from a broader sense, where the use of quasi-formal, more broadly naturalistic tools is acceptable as well.¹⁵

Not everyone was inclined to move even this far beyond Carnap's and Hempel's procedure, however. One way to stay closer to it is to accept that causation is central to scientific explanation, but to try capturing it in more formal terms after all (e.g., by appealing to counterfactual conditionals).¹⁶ Alternatively, a second main approach was to modify the Hempel-Oppenheim account by bringing in the notion of unification, as suggested by Michael Friedman and Philip Kitcher.¹⁷ The basic idea here is that the scientific treatment of a phenomenon is explanatory if it unifies

our understanding of the world in certain ways. Crucially for us, both Friedman and Kitcher attempt to articulate the notion of unification in formal terms (in Kitcher's case, by considering 'argument patterns'). As such, their approach is still explication in a fairly narrow sense, even though the way in which inference informs scientific explanation is now more complicated. The two authors also added a discussion of their own motivating examples, again along Carnap-Hempel lines.

The ensuing debate between proponents of causal and unification models can be seen as a discussion about how far and in what direction to expand the toolkit for explication when accounting for scientific explanation. Another kind of expansion underlies the third main approach in this connection – what I am calling the 'formal pragmatic account'. Its most well known proponent has been Bas van Fraassen.¹⁸ His core suggestion is to characterize scientific explanations as answers to certain why questions. Hempel and Oppenheim had mentioned this idea in their original article, but only in passing. In van Fraassen's work it is taken quite seriously and developed in a new direction, by employing a formal theory of why questions (derived from an 'erotetic' literature). Thus, in this account of explanation not only tools from formal syntax and semantics are employed, but also ones from formal pragmatics. In one respect, this is very much in the spirit of Carnapian explication: by adhering to formal precision. In another, it constitutes a radical departure, as 'explanatoriness' now becomes strongly context-relative. The latter is also reflected in a much more context-sensitive treatment of examples.¹⁹

(2d) A fourth alternative—beyond explication

In the literature on scientific explanation the assumption is often, explicitly or implicitly, that the three kinds of accounts just sketched represent the main alternatives to the CL model. However, a fourth alternative can be found in that literature as well, at least if one looks closely. The latter is guided by the following related convictions: First, Salmon is right that the notion of cause is crucial for scientific explanation. But attempts to reduce that notion to anything else are futile; we should treat it as a basic, irreducible notion. Second, what we can do instead is to relate the notion of cause, that of explanation, and further notions in their vicinity to each other, in an informal and mutually illuminating way. Third, the best way to proceed in this context is by considering a variety of examples; and they have to be treated in a highly context-sensitive way, as van Fraassen rightly noted.

In the philosophy of science, the original proponent of these views was Michael Scriven.²⁰ However, Scriven tends to be seen as a mere critic of the CL model in the literature. He did, in fact, suggest some classic 'counterexamples' to it. Moreover, a radical challenge does arise from their consideration, namely: by taking the 'clarification of the explicandum' not seriously enough – by only doing the kind of thing Carnap suggested in this

connection – almost everyone has failed to develop a good enough sense of the target phenomenon, ‘explanatoriness’. In addition, the consideration of examples plays a more positive role for Scriven as well. When we consider a plethora of examples, and when we apply the informal procedure sketched above to them, a new suggestion emerges. Being explanatory should be thought of as ‘filling a gap in our causal understanding’. Here the metaphor of a ‘gap’ is left somewhat vague and open-ended on purpose, as it varies from case to case and as it depends on one’s inarticulate competences what will ‘fill the gap’.

What differentiates Scriven’s account of scientific explanation most from both Hempel’s and its more prominent alternatives is a combination of taking examples very seriously and the insistence on proceeding informally. Thus I am calling it the ‘informal pragmatic approach’. Concerning the general methodology behind it, Scriven sometimes talks about ‘context analysis’. And while Hempel’s and his successors’ approaches are largely based on Carnap’s works, Scriven’s is rooted in writings by the later Wittgenstein, Austin, and Ryle. Two things these philosophers share are: the focus on examples, to the point of using a ‘method of examples’; and the attention paid to surrounding practices. There are also connections between Scriven’s ‘context analysis’ and what Peter Strawson – another ‘ordinary language philosopher’ – later calls ‘connective analysis’. In *Analysis and Metaphysics* (1992), Strawson characterizes the relevant informal, non-reductive and contextual procedure thus:

Let us imagine [...] the model of an elaborate network, a system, of connected items, concepts, such that the function of each item, each concept, could, from a philosophical point of view, be properly understood only by grasping its connections with others, its place in the system [...]. (Strawson 1992, p. 19)

Strawson also mentions an example that makes his affinity to Scriven’s project clear: the ‘connective analysis’ of scientists’ informal, interrelated use of concepts such as ‘explanation, demonstration, proof, conclusion, cause’ (*ibid.*, p. 12).

Part 3: A Critique of Explication

(3a) History, sociology, and philosophy

Let me now turn to my general critique of explication. So far a basic complaint against the Carnapian approach was that it tends not to take the ‘clarification of the explicandum’ seriously enough. To be sure, Carnap himself does not discount this task entirely. And when Hempel and Oppenheim set the stage for their CL model, they follow him, i.e., they provide some examples and general explanations. Yet judging from the subsequent criticism in

terms of ‘counterexamples’, including challenges to their own examples, it is hard to avoid the sense that, not only Hempel and Oppenheim’s approach to explanation, but the Carnap–Hempel methodology more generally has a systematic, or systemic, problem here.

However, a defender of Carnapian explication may find this charge too quick and somewhat unfair. Perhaps there is a weakness here; but does it constitute a principled problem? Can’t one supplement a Carnapian formal approach in some ways to get around it? In fact, two sources for where the needed supplement may come from suggest themselves: the history and the sociology of science. These are natural suggestions as soon as one realizes that, while Carnap never explored the history or the sociology of science much himself, other members of the Vienna Circle did. (Otto Neurath is well known for his emphasis on the sociology of science; and works by Philip Frank, among others, engage seriously with the history of science.) Finally, the members of the Vienna Circle clearly saw themselves as having adopted a division of labor, with some of them focusing on formal aspects of science, others on sociological and historical aspects.²¹

Then again, from the point of view of the critics such a response is unsatisfactory. For both Scriven and Strawson, it won’t do to simply exhibit further examples, by observing the behavior of scientists, by studying the history of science, or however one does it. What is required, instead, is to subject such examples to philosophical scrutiny. What also needs to be resisted, according to these critics, is the urge to derive sweeping generalization from a few cases, as tends to be encouraged by a formal approach. Wittgenstein characterized this urge (to which he succumbed in his own early work) as a ‘craving for generalities’ on the part of philosophers (Wittgenstein 1958, p. 17ff.). It leads to problems when the phenomenon at issue is tied closely to a feature of the context that grounds its significance but from which the generalization abstracts right away. The latter is exactly what, according to Scriven, happens in the case of Hempel’s treatments of explanation (which abstracts from the crucial role of largely inarticulate background knowledge).

Confronted with such a criticism, a Carnapian may still be unmoved, but try to be conciliatory. Yes, examples need to be scrutinized carefully before we can draw relevant conclusions from them. Maybe there is also a danger of hasty generalizations in formal approaches. Still, does this invalidate explication completely? Couldn’t we try to combine the exactness and other advantages of a formal approach, such as explication, with informal and contextually grounded attention to examples? In other words, a Carnapian might envisage a synthesis of the two approaches, one that combines the best of both worlds. In fact, this is pretty much what Carnap himself, in his usual conciliatory manner, suggested when confronted with a related criticism by Strawson. Or at least, Carnap admitted in this context that what Strawson would later call ‘connective analysis’ might be useful in ‘clarifying the explicandum’, so concerning that particular aspect of explication.²²

Yet again, from a Strawsonian point of view this admission does not go far enough. It still misjudges the depth or real force of the criticism. Similarly, such a defense of Hempel's approach would not fully satisfy Scriven, if at all. For both, informal considerations are inescapable in a much stronger sense. To understand how and why, it helps to return to another of my questions from section one.

(3b) Explications and their purposes

The second of my initial questions was: what exactly should we take the purpose, or the purposes, of Carnapian explications to be? Once more, particular explications are not meant to be correct or incorrect, only more or less fruitful. But: fruitful with respect to what goal? In addition, what goal is explication itself intended to serve, as a general methodology? Both questions are not unrelated to the treatment of examples, as I indicated. They are also not addressed much by Carnap or Hempel. And here too, our case study can bring the relevant issues into sharper focus.

As explications are not meant to be correct or incorrect, one purpose they might serve seems ruled out from the start, namely: to provide an accurate descriptions of scientific practice. Part of the reason is that explications usually involve abstraction and idealization; indeed, a formal-logical approach can hardly avoid that. Hempel is admirably explicit about this aspect when he writes:

[T]hese models are not meant to describe how working scientist 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)

Yet it is not true that no connection to scientific practice is intended. For Carnap, the explicatum is supposed to be sufficiently 'similar' to the explicandum; and the passage from Hempel just quoted is more specific: his particular explication is meant to capture 'the logical structure and the rationale' of scientific explanations. In that sense, or to that degree, descriptive adequacy is a desideratum after all. But then the question becomes: why exactly do we have that desideratum?

One possible answer to the latter question, hinted at above, is this: Ultimately, the purpose of an explication is for the explicatum to become a tool used in science, and perhaps also, in everyday life. What matters, along such lines, are scientific progress and the improvement of our ordinary lives, in a relatively direct way. However, in Hempel's case the notion of scientific explanation looks more like a meta-theoretic notion, playing a role at a relatively high level

of abstraction, than one that can be applied so directly. (Similarly for Carnap on, say, confirmation and analytic truth.) How could it be useful with respect to scientific and everyday (object-level) endeavors? Well, an explicatum may play a more general prescriptive or normative role. Thus, Hempel's CL model might be taken to tell scientists to produce 'explanations' of a specific form, ones that then impact everyday life. And in fact, Hempel did envisage his model playing such a role, e.g., in connection with history. A main complaint against it was, then, that he had taken a model from one discipline, mathematical physics, and simply imposed it on others. (Later the issue became whether it was even appropriate as a general model for physics.)²³

If the CL model is meant to be prescriptive as such, questions about its descriptive accuracy resurface. Whenever we offer an explication, the explicandum, as one kind of tool, is supposed to be replaced by the explicatum, a better tool. But then there was some role for the explicandum originally, and the explicatum is supposed to play that role too, just better; or at least, it is supposed to play a closely related role. When judging whether the latter is the case, it is evidently crucial to have a fairly good handle on what the original role was. Now, I take Scriven's criticisms of the CL model to be at bottom a challenge to the assumption that Hempel did have such a handle – here on 'explanatoriness'. Put more positively, a main purpose of 'context' or 'connective analysis' is to get clearer about exactly such aspects. The deeper charge against Hempel, and against Carnap's methodology more generally, is then: explication is not very fruitful for that task, in some ways even detrimental. With its strong focus on formal aspects it can, among other things, make us blind to questions about the appropriateness of the abstraction and idealization involved.²⁴

(3c) Explication and philosophy

Is their normative role for science, and with it for everyday life, the only purpose of Carnapian explications? Is it even their primary purpose? We can again consider the CL model here. I actually take one of its main roles, even if not its only role, to be philosophical. It is part of an overarching attempt to conceive of science in logical empiricist terms (as are, e.g., Carnap's proposed explications of the notion of analyticity). That this is a central point of the CL model is evidenced, e.g., by Hempel and Oppenheim's detailed attempt to treat the notion of law in terms acceptable to logical empiricists. Another aspect of the CL model is revealing here as well: the fact that it is supposed to be a universal model. This is related to a core part of the logical empiricist agenda: to account for the unity of science. (Parallel remarks apply to the causal, the unification, and the formal pragmatic model.)

So far we have considered what the purpose of a particular explication might be; or better, its main purposes. One can ask the same question about explication itself, as a general methodology. The answer to the latter question is, pretty clearly, that it is a proposal for how to proceed in

philosophy (a new, radical one). This proposal is modeled on the methodology of the sciences and, especially, of mathematical logic, to be sure; but that just means that philosophy is to become more 'scientific'. If one looks at the whole debate about scientific explanation from this perspective, what becomes apparent is, once more, a pattern of disagreements about whether Carnap's and Hempel's original methodology needs to be broadened and, if so, how far and in what direction to broaden it. Also put into sharp relief is, again, the fact that Scriven's approach is informed by a rival philosophical methodology.

This brings us to another blind spot of Carnapian explication. If we consider the fate of Scriven's approach to scientific explanation, it is striking that what I am calling the 'informal pragmatic approach' is not discussed much as a rival to the CL model in the literature. In fact, typically the approach is not recognized as an alternative at all. It looks like Carnap's methodology, in narrower or broader forms, has had such a stronghold on the philosophy of science that a very fundamental challenge to it, such as Scriven's, is hard to fit in; similarly for Strawson. Perhaps the attitude is: why shouldn't we aim for 'exactness' along broadly Carnapian lines, since doing so is our best chance for progress in philosophy, something we are in dire need of? In other words, why not use the sharpest tools available? From such a perspective, Scriven's and Strawson's insistence that we proceed in an informal way may seem baffling and almost perverse. (Compare telling a physicist not to use mathematics.)

However, from the point of view of the critics this attitude begs the question; the superiority of a formal approach (not just in physics but also in philosophy) is simply taken for granted. What Carnapian explication makes room for is to evaluate whether a particular explication is more fruitful than another. But as a consistent pragmatist, shouldn't one also inquire into the fruitfulness of one's overall methodology? Or course, the question is then right away: fruitful for what purpose? Well, consider again the explanation case. If our aim is to get a better grip on 'explanatoriness' – and it seems that we need such a grip also for Carnap's and Hempel's purposes – then there is a question of whether the Carnap–Hempel approach or the Strawson–Scriven approach is more fruitful. Or at least it is one of the critics' main challenges that there is such a question. Here my point is not that the critics' approach is evidently better.²⁵ Rather, it is that Carnapian explication makes little room for a comparative exploration. Why not? Well, a comparison would involve 'inexact' matters, or it would lead us back to 'philosophical arguments'; and their consideration is not exactly encouraged.²⁶

(3d) Carnapian Dialectics

If we look again at the question of which purposes inform the methodology of explication, it is not that there aren't answers available. But it is striking how little attention has been paid to them. Typically these purposes

remain implicit, not just in Carnap' and Hempel's writings but also those of their followers (up to the present). All too often, the main focus, or even the exclusive focus, is on formal details. This is in sharp contrast to the Scriven–Strawson approach where questions about purposes, or about the 'significance' of what has been proposed, take center stage, while formal details are secondary. However, let me ask one more time: Have we really done justice to Carnapian explication along such lines? Let us assume that it does tend to create the blind spots illustrated by our case study. Is this more than a tendency, i.e., are we dealing with some kind of in-principle limitation?

In the recent literature, it has been suggested that not only are such limits not forced on us along Carnapian lines, but Carnap himself came to see the need to say more about relevant 'pragmatic' issues. Thus, in his reply to Charles Morris, in the *Library of Living Philosophers* volume dedicated to him, he writes:

[Many] problems concerning conceptual frameworks seem to me to belong to the most important problems of philosophy. I am thinking here both of theoretical investigations and of practical deliberations and decisions with respect to an acceptance or a change of frameworks [...]. (Carnap 1963b, p. 862, as quoted in Stein 1992, p. 288)

As explications are always tied to a conceptual framework, one can take Carnap's emphasis on the importance of 'practical deliberations and decisions' to apply to them as well. Can this perhaps include explicit reflections on one's purposes, on how conducive one's methodology is relative to those purposes, etc.?

Building on earlier suggestions by Howard Stein, André Carus has recently attributed an implicit 'dialectic' approach to the later Carnap in this connection (in Plato's sense of dialectic). For Carus it is the dialectical dimension that most distinguishes explication from its predecessors in Carnap's writings. As he puts it:

In this new, implicitly dialectical conception, the applied discipline of 'conceptual engineering' – the successor to philosophy – still seeks to improve our understanding by piecemeal replacement of vague concepts with more precise ones. (Carus 2007a, p. 20)

But what does it mean for Carnap's approach to be 'dialectic'? This is explained further in terms of the choice between corresponding frameworks:

On the one hand, then, are frameworks of (relatively) precise, hard concepts, on the other hand is the activity of practical decisions among such frameworks. These decisions are at best partly extricable from the entire

worlds of practical decisions, which are generally conducted in ordinary, pre-systematic language, i.e., in softer, less precise concepts. They may be hardened up, just as, in the perspective of rational reconstruction, the concepts of ordinary scientific language were progressively upgraded and replaced. But in the new perspective, such progress is no longer a one-way street. The practical realm kicks back. (*Ibid.*, p. 21)

Carus then argues that, understood in such a dialectic way, Carnapian explication is still a viable methodology today (superior to, e.g., Quine's and Habermas').

The resulting (unorthodox) reading of Carnap is striking. It can make one wonder, however, whether the line between interpreting Carnap and improving on him, or between being faithful to his writings and philosophizing in a broadly Carnapian spirit, has not been blurred. This is partly admitted by Carus:

To understand the full scope and revolutionary implications of this idea, we have to go beyond Carnap's own formulations, and perhaps even beyond anything he actually thought. Certainly he did not immediately recognize (and never made quite explicit) the mutual feedback introduced by [the dialectic] between the theoretical and the practical. (Carus 2007a, p. 19)

Now, let me grant that the attribution of a dialectic approach to the later Carnap, in something like Stein's and Carus' sense, is justifiable (and potentially fruitful, in a variety of ways). The following question still remains: What exactly should be included in Carnapian 'dialectics', i.e., how far is the corresponding broadening supposed to go? This is both an interpretive and a systematic question.

Taken in the interpretive sense, the question may be hard to answer, as Carnap says so little that is relevant. But if we think about it in more systematic terms, turning once more to our case study can help. With respect to the debate about scientific explanation we have already encountered a whole spectrum of relevant options. On one side of it, there is the CL model, which involves adhering strictly to the narrow conception of explication. One step removed, we can find the kind of broadening that the unification model and, to take another step, the causal model illustrate. Then one can accept Strawsonian 'connective analysis' for the clarification of the explicandum. Significantly further removed is the acknowledgment that such clarification might well lead to changes in one's understanding of 'explanatoriness', and with it, to challenges concerning earlier abstractions and idealizations. Yet further along, we encounter more general discussions about goals, tools, and their relations. And all the way on the other side of the spectrum, there is the choice of a different methodology altogether, such as

Scriven's and Strawson's, with its emphasis on informal 'significance' rather than on formal 'exactness'.

If we take an approach such as Stein's and Carus' seriously, the question becomes: Where on this spectrum of broadenings do we end up? From their dialectic point of view, it seems not hard to accept the kind of extensions that Friedman's, Kitcher's, and even Salmon's models of scientific explanation involve. We also saw that Carnap, when pushed, was willing to grant a role to Strawsonian clarifications of the explicandum. But do we get more than that? For example, what kind of substantive questioning of goals can a Carnapian dialectic approach accommodate?²⁷ And to what degree, or in what form, is there room for a challenge to its own fruitfulness? Finally, the further we move to the other side of my spectrum, the less we are dealing with a Carnapian position any longer, even in a broadened sense, it seems. In other words, we are led, more and more, to give up on Carnapian explication. Does talking about a 'dialectic conception' open the door, or the floodgates, for that? And if not, why not? This seems to me a crucial question, one that calls for more attention.

Concluding remarks

In this essay, I used the case of Hempel's CL model and the various responses it has elicited to reveal some blind spots in Carnapian explication. A quick response might be that some or all of them are not really problems for Carnap's methodology; they are just artifacts of the particular case. In other words, Hempel's CL model may well be problematic; but that is because it is a bad application of, or a bad case for, explication. Then again, the points I raised are more general and less easily dismissible, I think. Among others, I am convinced that very similar issues arise for several of Carnap's own explications (as indicated parenthetically), although establishing that fact more conclusively would require one or several further essays. Another reply might be that, while Carnapian explication does have limitations, the Scriven-Strawson alternative to it has serious weaknesses of its own. Yet again, my goal was not to establish the general superiority of that alternative (although I do find it attractive in several respects). Instead, it was to show that there should at least be room for considering alternatives, especially for a pragmatist.

Carnap articulated the notion of explication in the late 1940s, as a successor to earlier notions. Subsequently his approach became more flexible; arguably he even added a dialectic dimension to it. But the question remains how far he went in that direction, or more systematically, how inclusive a Carnapian should take dialectics to be. By allowing a larger and larger role for the informal clarification of examples, for the systematic discussion of goals and the evaluation of corresponding tools, etc., one might be driven so far that one is hardly dealing with explication any more. At the same time,

if one stops too soon with such broadenings – if one emphasizes formal ‘exactness’ too much, is overly dismissive of ‘philosophical arguments’, and basically, takes explication to be a ‘magic bullet’ – then the effectiveness of explication is undermined. Large parts of this essay were meant to make evident such points, and thereby, to reveal the complexities of the ‘dialectics’ at issue.

Perhaps the most basic point of my critique is this, however. If we take certain questions arising for Carnapian explications seriously, as we should, we are led right back to ‘philosophical arguments’. Examples from our case study include: the argument that the explicandum from which a Carnapian starts may be in need of much more clarification, in informal terms, than initially assumed; the argument that the abstractions and idealizations built into an explicatum early may have to be reconsidered critically later on; the argument that, if we acknowledge explications to be purpose-relative, this might lead us into substantive considerations of the goals and tools at hand; and the argument that there might remain important, indeed crucial, questions about the general significance of the explication, exact and otherwise admirable as it is. Where does this leave us? Proceeding formally, as in explication, has various strengths, no doubt. But it tends to leave a ‘philosophical residue’, one that has to be addressed informally in the end. If this is right, we cannot escape the ‘infinite ambiguity’ of philosophy – even along Carnapian lines.

Notes

1. Earlier versions of this paper were presented at a meeting of the Southern California *History and Philosophy of Logic and Mathematics* Group, University of California at Riverside, May 7, 2009, and at the conference *Carnap's Ideal of Explication: Logic, Metalogic, and Wissenschaftslogik*, Université Paris 1 Panthéon-Sorbonne, May 14, 2009. I would like to thank Pierre Wagner for inviting me to the Paris conference, and the audiences at both events for their comments and criticisms.
2. For a general overview of recent scholarship on Carnap, cf. Friedman and Creath (2007); for *Logische Syntax*, cf. Wagner (2009a); and for Carnap's notion of explication, cf. Carus (2007a).
3. For ‘formal philosophy’, cf. Hendricks and Symons (2005); for Carnap's lasting impact on the philosophy of science, cf. Kuipers (2007), pp. viii–ix; and for formal logic, cf. Reck (2007), (2012a).
4. Carnap writes: ‘A concept must fulfill the following requirements in order to be an adequate explicatum [...]: (1) similarity to the explicandum; (2) exactness; (3) fruitfulness; (4) simplicity’ (1950b, p. 5). In this essay, his first three criteria will be examined critically; Carnap doesn't emphasize the fourth much, and neither will I. (In the end it leads to parallel problems, I think.)
5. Aristotle comes to mind here: ‘For the educated person seeks exactness in each area to the extent that the nature of the subject allows it [...]’ (*Nicomachean Ethics*, Book I, 1094b24ff.)
6. This would be in line with seeing Carnapian explication as part of a broader Enlightenment project; cf. Carus (2007a). For an alternative view of his goals, cf. Richardson (2012).
7. Similar ideas can be found in R. B. Braithwaite, K. Popper, and J. S. Mill; indeed, the basic suggestion goes back to Aristotle. But most discussions of the topic begin with Hempel and Oppenheim's essay.
8. For simplicity's sake, I restrict myself to the case of ‘deductive-nomological’ explanations in this essay; for the parallel case of ‘inductive-statistical’ explanations, cf. Hempel (1965a).
9. Hempel spent a semester at the University of Vienna in 1929; he was also Carnap's research assistant at the University of Chicago in 1937–38, and he remained in communication with him.
10. The literature on the CL model and on scientific explication is huge; for more, cf. Pitt (1988), Kitcher and Salmon (1989), Salmon (1990), Woodward (2003), and the further references in them.
11. For a summary of both these early criticisms (by R. Eberle, D. Kaplan, R. Montague, and others), and Hempel and Oppenheim's initial response, cf. Salmon (1990), chapter 2.
12. For details, see again Salmon (1990), chapter 2. Most of these examples concern the physical sciences. I will come back to the adequacy of the CL model in other areas in section three.
13. Cf. Salmon (1984) for a systematic presentation. Salmon first developed a ‘statistical-relevance model’ which he then came to see as incomplete; for more, cf. Salmon (1990), chapter 3.
14. As many of the initial participants in the debate shared Humean scruples about the notion of causation, i.e., found it suspect on empiricist grounds, this was a major reversal.
15. For more on Salmon's approach to causation, cf. Salmon (1998). For a more recent causal model, cf. Woodward (2003) (where the notion of ‘invariance’ is used to account for causation).
16. David Lewis' work was important here; for more, cf. Collins, Hall and Paul (2004).
17. Cf. Friedman (1974), Kitcher (1981), and, for the most detailed defense, Kitcher (1989).
18. Cf. Van Fraassen (1980b); similar views can be found in Garfinkel (1981) and Sintonen (1989).
19. In van Fraassen's formal treatment, ‘context’ includes things like a ‘contrast class’, a ‘relevance relation’, conditions for when relevant why questions ‘arise’, etc.
20. Cf. Scriven (1958), (1962), and (1966). I am much indebted to Larry Wright for pointing me towards Scriven's approach and its significance; cf. also Wright (1995), (2002), and (2012).
21. Cf. Thomas Uebel's contribution to the present volume. By now it is also well known that Carnap was quite sympathetic to Thomas Kuhn's historical-sociological work; and compare Carus (2012).
22. Cf. the criticism in Strawson (1963) and the response in Carnap (1963b).
23. For the case of history as a discipline, cf. already Hempel (1942); see also the discussion of, e.g., archeology in Salmon (1990), pp. 25–26. Subsequent ‘counterexamples’ to it also concern physics.

24. Here the point is not that formalizations cannot help. Rather, it is that, once certain abstractions and idealizations have been built into them, they tend not to be examined critically again later.
25. In the present context, it may be that a combination of the Strawson– Scriven approach with van Fraassen’s approach, which is somewhat in-between, is most fruitful; cf. Wright (2012).
26. In his response to Quine, Carnap does consider the fruitfulness of different methodologies; cf. Stein (1992), pp. 278–80. His response to Strawson’s informal approach is significantly different.
27. This is related to the question of how much room there is, from a Carnapian perspective, for a substantive discussion of ethical and political values; cf. Carus (2007a), chapter 11.