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ARTICLE

Beyond Hempel: Reframing the Debate about Scientific Explanation

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Abstract

I argue that Carl Hempel's pioneering work on scientific explanation introduced an assumption that Hempel never motivated, namely, that explanation is an aim of science. Ever since, it has remained largely unquestioned in analytic philosophy of science. By expanding the historical scope of the debate on explanation to philosophers from the first half of the twentieth century, I show that the debate should include a critical reflection on Hempel's assumption. This reflection includes two problems: how to motivate one's position on the aims of scientific knowledge and how to decide which examples count as expressions of those aims.

1. Introduction

Scientific explanation is a central point of contention in philosophy of science. In the last seven decades, numerous potential candidates for a general theory of scientific explanation have been constructed. Although there is no consensus on which theory of scientific explanation should be preferred, all participants in the debate share a common commitment. They all refer to the article "Studies in the Logic of Explanation" by Carl Hempel and Paul Oppenheim (1948) as the starting point of the debate. I will argue that this seemingly innocent historical reference reveals an underlying, unquestioned assumption in the debate on scientific explanation. I call this the *explanationist assumption*: that explanation is an epistemic aim of scientific inquiry that is distinct from its descriptive aim. As I will show, Hempel and Oppenheim introduced this assumption but offered no argument to support it. By expanding the scope of the debate to philosophers of science from the first half of the twentieth century, I argue that the debate on scientific explanation should include critical reflection on the explanationist assumption.

2. An interpretive challenge for theories of explanation

In *Aspects of Scientific Explanation* (1965), Carl Hempel used ideas from scientists about their own explanatory practice in two different ways. On the one hand, Hempel quoted scientists who apparently endorsed a covering-law model of explanation. For instance, in order to decide whether scientific explanations always

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reduce an unfamiliar phenomenon to a familiar one, Hempel quotes the sociologist George Homans, who claimed that to deduce low-level generalizations from a set of more general propositions is to explain them (Hempel 1965, 431–32). Hempel used Homans's idea to support his own position that although the more general propositions are not necessarily more familiar, they can be explanatory. In this instance, the pronouncement of a scientist on his explanatory practice was used by Hempel as evidence that confirms the consequences of the covering-law model of scientific explanation.

On the other hand, Hempel also reinterprets the pronouncements of scientists so that they fit his model. Two pages after the use of Homans's ideas, Hempel introduces a quote by Lord Kelvin: "as long as I cannot make a mechanical model all the way through I cannot understand" (Hempel 1965, 434). In this quote, there is no reference to a deduction from general laws. Yet, Hempel interprets the quote as if Kelvin demanded "not simply that an explanation somehow render a phenomenon plausible or familiar, but more specifically that it provides a model governed by the laws of mechanics" (Hempel 1965, 434). This leads to an *interpretive question*: Under what conditions can some pronouncement of a scientist be used as evidence confirming a philosophical theory of scientific explanation? Or, under what conditions should it be *reinterpreted* so that it conforms to the theory? Hempel never addressed this question.

This interpretive problem results from Hempel's larger project of explicating scientific explanation. This project is shaped by two central assumptions, and these assumptions are taken over by most participants in later debates. One assumption deals with scientific knowledge and one with philosophical theory:

> The explanationist assumption (E): Explanation is an epistemic aim of scientific knowledge that has been continually present in scientific practice as long as that practice has existed as science.
>
> The analytical assumption (A): The aim of the philosophical theory is to capture the relevant epistemic aims of the scientific practice so that the theory gives an adequate description of the epistemologically relevant aspects of that practice and a route to evaluate its results.

(E) is an assumption about the uniformity of the explanatory aim of science across time and across disciplines. It enables Hempel to use not only examples from the history of science but also more contemporary examples and examples from a varied set of scientific disciplines. If there were no such assumption, Hempel would have no reason to collect a varied set of examples as a set that expresses a single, uniform aim of scientific theories (i.e., explanation) that could be captured in a philosophical theory. (A) is an assumption about the task of the philosophy of science. Applied to explanation, (A) entails that the philosophical model of scientific explanation should be fateful to both actual scientific practice and the norms for explanation that are implicit in it. The philosophical model aims to clarify those implicit norms. Both assumptions are introduced in the opening paragraph of Hempel and Oppenheim's (1948) article "Studies in the Logic of

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Explanation," which is universally taken to be the foundation for all later debates on this topic:[1]

> To explain the phenomena in the world of our experience, to answer the question "why?" rather than only the question "what?", is one of the foremost objectives of all rational inquiry; and especially, scientific research in its various branches strives to go beyond a mere description of its subject matter by providing an explanation of the phenomena it investigates. While there is rather general agreement about this chief objective of science, there exists considerable difference of opinion as to the function and the essential characteristics of scientific explanation. In the present essay, an attempt will be made to shed some light on these issues by means of an elementary survey of the basic pattern of scientific explanation and a subsequent more rigorous analysis of the concept of law and of the logical structure of explanatory arguments. (Hempel and Oppenheim 1948, 135)

In 1948, the covering-law account still aimed at a definition of explanation in terms of necessary and sufficient conditions that could be expressed in first-order predicate logic and would fit all available examples (Hempel and Oppenheim 1948, 157). In *Aspects of Scientific Explanation*, which was published seventeen years later, Hempel retracts this stringent goal and is satisfied with a model that explicates the relevant epistemic features of the explanatory aim of scientific knowledge (Hempel 1965, 412). Although both assumptions (E) and (A) are stated in the opening paragraph of the 1948 article, they are never made explicit as starting points of inquiry that require extra motivation, nor are they introduced as hypotheses that have to prove their fruitfulness. Hempel and Oppenheim take them for granted and initiate a research program starting from these assumptions. A theory of scientific explanation, given this program, will have two challenges:

1. Selecting from scientific practice those examples (claims by scientists about explanation, purported examples of scientific explanations, disputes about explanation) that can be understood as expression of the explanatory aim of scientific knowledge
2. Constructing a model of scientific explanation that is justified by the selected evidence

In the 1948 article, the first challenge proved difficult: because there were no well-established examples of scientific explanation within the contemporary philosophy of science, Hempel and Oppenheim had to introduce relevant examples themselves. They mentioned the explanations of the immersion of a mercury thermometer in hot water—the thermometer first drops and then swiftly rises—the appearance of

[1] I do not claim that Hempel was the inventor of the deductive-nomological (DN) model of explanation, as one could claim that Popper or Mill had already written about explanation in a similar way. I only claim that Hempel's logical articulation of explanation is universally taken as the theory against which other theories of explanation were given. Hempel and Oppenheim themselves mention several earlier philosophers as inspiration to their article (Hempel and Oppenheim 1948, 140).

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a broken oar in water, and Galileo's law for the free fall of bodies (Hempel and Oppenheim 1948, 136). Unsurprisingly, in all three examples, a general law plays a crucial role in the explanation of the phenomenon. Later in the article, they also discuss "an explanatory argument from the field of linguistics," an explanation of the evolution of the Latin word *apis* into the English word *bee* and the French word *abeille*. However, they decide to conceive this explanation as incomplete because it lacks clearly specified general laws concerning the phonetical evolution of words (Hempel and Oppenheim 1948, 141). Deciding which examples are proper expressions of the explanatory aim of science, and especially how they are evidence for the offered philosophical model (incomplete attempts at explanation or outright counterexamples), is an interpretive problem that is similar to the use of scientists' ideas about explanation, which I previously highlighted.

Another paradigm case in the explanation literature is the purported explanatory power of Newton's law of universal gravitation: one can explain the position of Mars through information about the mass of the sun, the mass of Mars, Mars's present position and velocity, Newton's laws of motion, and Newton's universal law of gravity. Hempel mentions this in 1948 (Hempel and Oppenheim 1948, 135), and Norwood Hanson discusses it as the crucial evidence for Hempel's model (Hanson 1959). Unfortunately, there is no more contested case about explanation in the history of science. Whether the universal law of gravitation explains at all was contested in Newton's time, and it still is. Whether the examples that Hempel himself and later participants in the debate introduced are true expressions of the explanatory aim of science is a very difficult interpretive question to answer. Whatever the answers, it is crucial to see that the question only becomes possible because of a previous commitment. It is only because one assumes that explanation is an aim of science expressed in scientific practice that the choice of examples becomes a problem. Given (E), talk of explanation in the practice of science becomes a relevant and distinct aspect of the practice that one wants to understand on a philosophical level. But why assume (E)? Why do we think that scientific knowledge has an explanatory aim that is distinct from description? This assumption is part of the terms of the debate as they were set by Hempel.

Although most philosophers of science today believe that the covering-law model, at least in its Hempelian formulation, is mistaken, there is a widespread agreement that the model represented a major step in our understanding of science. It helped us approach questions concerning scientific explanation in a fruitful way. Wesley Salmon lauded the model as Carl Hempel's most important philosophical achievement and as "one of the most significant pieces of philosophical progress in the twentieth century" (Salmon 2000, 316). In Salmon's (1989) *Four Decades of Scientific Explanation*, the 1948 article marks the distinction between the prehistory and the history of the modern discussion of scientific explanation. According to Salmon's narrative, philosophers before the appearance of the DN model in Hempel and Oppenheim's version "had no clear idea of what scientific explanation might be" (Salmon 1989, 10). Most commentators on the debate follow Salmon in opening their discussion of the topic with the 1948 article (McCain 2016, 133; Woodward 2017; Skow 2016). In their article, Hempel and Oppenheim show how assumptions (E) and (A) give rise to a fruitful avenue of research on scientific explanation. However, conceiving this article as the fountainhead from which the

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discussion on scientific explanation should start is not a neutral historical choice. Salmon's decision to bracket all philosophical discussion on scientific explanation before Hempel as mere prehistory is problematic because it obscures a crucial question that Hempel's work on scientific explanation never addressed, namely, how to decide whether scientific explanation is a distinct aim of scientific knowledge and, consequently, whether it is necessary to evaluate scientific theories on the basis of their explanatory power.

3. The prehistory unraveled

From a historical point of view, Hempel and Oppenheim's article is a radical shift in logical empiricist philosophy. Whereas Reichenbach and Carnap, both important influences on Hempel's work, are mostly silent about explanation as an aim of science, Hempel brings it to the center of attention in the philosophy of science when the field emerges as a professional subdiscipline in the 1950s and 1960s. What happened to the philosophical interpretation of scientific knowledge in the 1940s so that most American philosophers of science became convinced that explanation had to be accounted for as an aim of science? In answering that historical question and reframing the history of ideas on scientific explanation, there is, I believe, much to learn about the debate on scientific explanation in general.

In order to understand the historical significance of Hempel's introduction of the DN model, one has to be familiar with the ideas about scientific explanation from the first half of the twentieth century. Contrary to Salmon's historical judgment that philosophers before Hempel had little clue what scientific explanation could be, there actually was a thriving philosophical debate about scientific explanation, especially in the decades before Hempel's article. Unlike the post-Hempel situation, this early-twentieth-century debate did not center on finding a comprehensive theory/model/analysis of scientific explanation that could fit all available examples and paradigm cases of scientific explanation. Instead, it questioned the validity of explanation as an aim of science and the role that explanation could play in understanding the nature of scientific knowledge.

In the logical empiricist circles around the younger Carl Hempel, most philosophers believed that explanation was not an acceptable concept in an epistemology of science. Philipp Frank identified three antiexplanatory philosophers as the intellectual predecessors of scientific philosophy: Mach, Poincaré, and Duhem. In Frank's account, these philosophers opposed the idea that physical theories should be evaluated on their explanatory power over and above their descriptive adequacy. As a consequence, these philosophers attempted to account for supposed explanations in science as expressions of the descriptive aim of scientific theories. Frank quoted the following insight of Duhem as an important inspiration to the Vienna circle (Frank 1949, 15):

> A physical theory is not an explanation. It is a system of mathematical propositions, deduced from a small number of principles, which aim to represent as simply, as completely, and as exactly as possible a set of experimental laws. (Duhem 1906/1991, 19)

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In a similar vein, Ernst Mach conceived the aim of physical theories as the most economical and briefest description of natural events (Mach 1893/1974, 6). For Mach, the explanation that physical theories afford is merely a useful psychological effect of their descriptive power: the theories accustom our thoughts to a great variety of disparate facts (Mach 1893/1974, 7). According to this understanding of scientific knowledge, explanation is not taken as a goal of scientific inquiry that is distinct from description: whatever explanatory virtues are attributed to scientific theories, they can always be recast within descriptive terms. I will call this general position on the aim of science *descriptivism*. For Mach and Duhem in particular, descriptivism entailed that scientific theories are mere systematized descriptions of our experience of the world. If one believes that scientific theories can achieve insight beyond this systematization, then one has a wrong understanding of what a scientific theory aims at. Both Duhem and Mach took this to be a general lesson that they could draw from the history of science: they were capable of describing the historical progress in mechanics without any reference to the explanatory power of physical theories, even though at several points in time, past scientists had preferred several theories because of their purported explanatory value.

A descriptivist position has to be accompanied by an account of explanation-talk in the practice of science. Duhem is most explicit in this regard: that some "geniuses" of modern physics have built their theories in the hope of giving an explanation of natural phenomena is not a conclusive argument against a descriptivist position. As Duhem phrased it, "chimerical hopes may have incited admirable discoveries without these discoveries embodying the chimeras which gave birth to them" (Duhem 1906/1991, 31). Although the explanatory aim and accompanying explanatory language of certain scientists is not a conclusive argument against descriptivism, it does present a challenge: to present a history of science without explanation as a driving force in its progress. Duhem's historical analysis of physics is an attempt to meet this challenge. In *The Aim and Structure of Physical Theory* (1906/1991), he always distinguishes the representative (descriptive) part of any physical theory from its explanatory part, defending that a theory never owes its power or fertility to the latter. In the development of theories, Duhem claimed, the explanatory aspects of theories are always abandoned, whereas the representative parts of the theories are transferred (Duhem 1906/1991, 32). Philipp Frank endorsed Duhem's historical dialectic of scientific theories in the following way:

> New physical laws are in contradiction with the old physical laws which appear now disguised as philosophic principles with pretensions of eternal validity. The old physical theory was a good description of a restricted group of facts. But to cover the new facts the old theory became inconvenient. (Frank 1953, 479)

Even though scientists often believe that their theories "explain," this aspect of a theory is taken by descriptivists to be of heuristic value only, as a driving force in the acquisition of new empirical phenomena. In Frank's reading, "explanation" as a term can still be applied to scientific theories—but only to the extent that theories systematize experience. Explanation-talk by scientists should be understood within the descriptive aim of science. Philosophers in the logical empiricist movement broadly conceived also use the term "explanation" in that sense. Moritz Schlick maintained

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that "description by means of laws achieves all that can possibly be demanded of knowledge" (Schlick 1953, 473). In his introductory work on positivism, Richard von Mises equally defended that "explanation is but a special form of description, namely, a description that is systematic, unified and, as far as possible complete" (von Mises 1968, 138). Science is only "a description of relations, of interconnections between phenomena" (von Mises 1968, 138). Rudolf Carnap also discusses the term "explanation" in this limited sense in 1931: it is the deduction of a singular scientific sentence from other singular scientific sentences with the help of a hypothetical general sentence. Thus, for Carnap, you can call connections between singular scientific statements through laws "an explanation" (Carnap 1931, 463). This does not imply that explanation is an aim of science that goes beyond its power to coherently describe the relations between empirical phenomena. In Hempel and Oppenheim's early book *The Type-Concept in Light of the New Logic* (1936, 1), they maintain a descriptivist reinterpretation of explanations similar to Carnap's: "Explanation is better called the deployment of laws that connect empirical data in a determined way. In the formulation of laws one uses concepts that describe the intertwined data."

Among early logical empiricists, it was common to assume that explanation was not an aim of science distinct from description. In contrast to Mach and Duhem, logical empiricists did not write about explanation in a negative sense. It played little role in their analysis of science, that is, until Hempel and Oppenheim's (1948) article, where they open with the statement that "scientific research in its various branches strives to go beyond a mere description of its subject matter by providing an explanation of the phenomena it investigates" (Hempel and Oppenheim 1948, 135).

In the first half of the twentieth century, there were also prominent philosophers in both France and Germany who opposed the descriptivist understanding of scientific knowledge and believed that explanation was a preeminent aim of science, distinct from description. Émile Meyerson, an important figure in the French epistemological tradition, disagreed with Mach, Duhem, and the neo-Kantian Ernst Cassirer on the aim of scientific knowledge, claiming that description and explanation were both distinct, proper aims of scientific theories—henceforth, I will refer to this general position as *explanationism*.

> It is not true that the sole end of science is action, nor that it is solely governed by the desire for economy in this action. Science also wishes to make us *understand* nature. (Meyerson 1908/1953, 462)

Limiting the aim of scientific theories solely to the (systematized) description of phenomena is, according to Meyerson, in contradiction with a central habit of human thought, a certain instinct to look for the cause of things, to answer the question of why something occurred (Meyerson 1908/1953, 464). For Meyerson, the practice of science is a further development of this human mental instinct, the development of a pre-science already present in our minds—a desire for intelligibility (Meyerson 1908/1953, 468). Meyerson did not believe that an interpretation of science as the search for explanation was self-evident. It had to be grounded in a historical account showing how both the descriptive and explanatory aims of science have been applied simultaneously and continue to be so applied (Meyerson 1908/1953, 462). Meyerson's aim was to produce a historical narrative of modern science as the

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expression of one and the same desire for understanding in his book *Explanation in the Sciences*. Meyerson's narrative was to prove that if one has a descriptivist view of scientific knowledge, "the entire course of science, past and present, becomes an enigma, or rather a sort of gigantic and monstrous absurdity" (Meyerson 1921/1991, 34). Although Meyerson's position diametrically opposed Duhem's descriptivism, he acknowledged that references to the statements of scientists about the aims of their theories do not yield any decisive argument for his position. Scientists throughout the past have had both explanationist and descriptivist inclinations: scientists' pronouncements on the principles that guide their thought should not be trusted (Meyerson 1921/1991, 465). In a critical review of Ernst Cassirer's historical narrative of modern science in *Das Erkenntnisproblem*, Meyerson expressed this methodological problem for the philosopher of science in the following way:[2]

> I would like to bring a general observation to light. The difficulty of which I speak, is particular to philosophy of science and it has, as I believe, often troubled the perspective of the researcher [in philosophy of science]. No one can see himself thinking. This is seemingly indisputable. If this were not so, then logic would not be a science that one had to acquire. If it were not the case, then everyone would know directly and with certainty how he had arrived at his conclusions. We do not possess this knowledge and we only achieve it little by little and over highly diverted routes. Rules that were only unconscious grow ever more conscious. However, we can also make a mistake and attribute to our own reason a rule that is very different from the one that we have actually followed: the fact that we debate about logic is manifest evidence of this problem. Now, in this, the scientist is no different from other men. No more than anyone else can he perceive himself thinking, since, just like everyone else, he follows rules that are not directly accessible to his own consciousness. (Meyerson 1911, 125–26)

Any argument for or against descriptivism or explanationism relies on an *interpretation* of scientific theories. No simple *reading* of scientific text could produce the meta-scientific concepts with which to evaluate scientific theories. For Meyerson, Duhem, Mach, and Cassirer, such interpretation had to be grounded in an account of the historical development of scientific knowledge. There were, however, also other possible routes.

In the German context, Heinrich Rickert, a notable neo-Kantian philosopher, defended an explanationist account of scientific knowledge. Rickert's position did not rely on a historical account of scientific progress but was grounded in his logical understanding of the nature of scientific concepts in general. According to Rickert, concepts that merely systematized the available empirical data were not proper scientific concepts because they did not aim for universal validity, a validity that governed beyond the available empirical data. Rickert believed that there were two ways

[2] For Meyerson, Mach, Duhem, and Cassirer, although all three had distinct epistemological theories, were representatives of the descriptivist position. Meyerson classified Cassirer as such, most likely because of Cassirer's opposition to Meyerson's explanationism in *Substance and Function* (Cassirer 1910/1923, 324). Although Cassirer rejects Mach's phenomenalism, he offers an account of the aim of physical theory that is explicitly akin to both Duhem's and Mach's accounts (Cassirer 1910/1923, 203).

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to think about scientific concepts. Concepts can make a phenomenon merely understandable. This implies that one uses classificatory concepts (*Gattungsbegriff*) to systematize objects or events of which one has experience (Rickert 1929, 102). In Rickert's understanding, a descriptivist position, like Mach's, would limit the aim of scientific concepts to this stage of concept formation. If this is the case, the distinction between explaining and describing collapses (Rickert 1929, 106). Rickert, however, distinguishes a classificatory use of concepts from a higher aim of concept formation, one that subsumes an event under a natural law. This subsumption shows that an event was *absolutely necessary* and could not have been different (Rickert 1929, 106). The search for natural laws was the highest possible aim of science. For Rickert, an explanation of an event entails knowing why an event had to occur and thus knowing how an event follows from a necessary principle. Explanation, in his account, follows from an understanding of the necessary coherence of events in nature, and in this sense, the natural sciences can go beyond the descriptive aim of scientific knowledge. Rickert's refutation of descriptivism mainly relies on his understanding of concepts. The concepts produced by our understanding *can* strive for a universal validity over their objects that is not conditioned by the available empirical phenomena. Concepts that express such absolute, necessary relations, even though they may not yet have been reached by science in practice, are still possible superior objects of thought and thus a possible higher goal for scientific knowledge.[3]

At stake in this European, pre-Hempelian debate is the following philosophical question: Is explanation an aim of science distinct from its descriptive aim, and what kinds of arguments can one develop in favor of one's answer to the former question? Mach and Duhem claimed that there is an account of science possible without explanation as its aim, whereas Meyerson claimed that no such coherent account could be given. Crucial in this debate is that all philosophers recognized that a decision on the aim of science could not be motivated merely by referring to the "explanation-talk" of scientists. It is not because scientists have used the terminology of explanation to discuss their work that one has to implement scientific explanation as an essential concept in the epistemological interpretation of scientific theories. Because there are no given examples that are evidence for the explanatory aim of science without prior commitments concerning its aims, any motivation for such commitments should look beyond direct talk of explanation in science and should rest on other grounds.

[3] At this stage, one might think that the earlier descriptivist–explanationist debate that I presented was a precursor to the debate of realism versus antirealism. There is an interesting relation between descriptivism and antirealism, on the one hand, and explanationism and realism, on the other. Historically, most explanationists are realists, and the same for their contraries. However, this relation is not necessary. It is conceivable that one will claim reality for theoretical entities, even if one does not believe scientific theories have explanation as their proper aim. However, it is common that there is a relation of motivation between both views. In the past, explanationists like Meyerson motivated their realism through their ideas on the explanatory aims of science. Alternatively, one could motivate explanationism through some form of realism. Similar relations hold between descriptivism and antirealism—for a contemporary example in Bas van Fraassen's work, see section 5.

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4. Hempel's assumption: Explanationism without motivation

Hempel and Oppenheim's (1948) article shifted the balance in logical empiricist philosophy completely. In the opening paragraph, Hempel and Oppenheim introduce it as a *given* that explanation is an epistemic aim of science distinct from description, and they add that there is no reason to doubt this: "there is a general agreement" (Hempel and Oppenheim 1948, 135). This statement is deceptive. Not only does it do no justice to the philosophical debates of the earlier decades in the twentieth century, but it is also in direct conflict with the previous position of most logical empiricist philosophers, who had consistently claimed that talk of scientific explanations could be interpreted within the descriptive aim of scientific knowledge. Hempel and Oppenheim abandoned this crucial aspect of the descriptivist position: it is the goal of their article to account for explanation as an aim of science in its own right. As a consequence, their work starts from the explanationist assumption (E). Given (E), talk of explanation in the practice of science becomes a relevant and distinct aspect of the practice that one wants to understand on a philosophical level. Just as the descriptivist has to interpret talk of explanation in science within descriptive terms, the explanationist has an equal challenge of *interpretation*, both in selecting which examples express the explanatory aim of science and in deciding how the offered model relates to these examples.

Based on the 1948 article, it is unclear how Hempel and Oppenheim came to have the commitment that scientific knowledge aims at explanation because they do not address this commitment at any length. In *Aspects of Scientific Explanation*, seventeen years later, Hempel only notes that human beings have a basic motive of "sheer intellectual curiosity," "a deep and persistent desire to know and to understand themselves and their world" (Hempel 1965, 333). This seems to be some kind of anthropomorphic ideal for science: the human intellect has a common desire to understand the world, and science, as an expression of that desire, aims to explain the world. This type of anthropomorphic motivation is still prevalent in the literature. Michael Strevens, one of the most prominent contemporary explanationists, holds that "when science is pursued as an end rather than as a means, it is for the sake of understanding—the moment when a small, temporary being reaches out to touch the universe and makes contact" (Strevens 2008, 3). In fact, as is often noted, this motivation for (E) dates back to Aristotle's opening of the *Metaphysics*: "All men desire to know by their nature," which is specified later in *Metaphysics Alpha* as a desire to know not only *what* but also *why* things happen (Salmon 1989, 3). Meyerson started his history of science from the same anthropomorphic view: "science has been established with the *tenacious hope* that nature will manifest itself as intelligible" (Meyerson 1953, 462). However, this provides no real argument for (E). Even if human beings all have such a uniform desire to understand, this does not mean that scientific knowledge should be interpreted as the (successful) execution of that desire. To be responsibly committed to (E), more is required than a vague theory about the desires of the human intellect.

Because Hempel, throughout his career, never explicitly addressed what motivated him to take (E) for granted, it is an open question how to understand the philosophical rationale behind Hempel's commitment to the idea that scientific knowledge aims at explanation. Given the lack of explicit argumentation, we can turn to the historical circumstances surrounding Hempel's shift. This historical question finds no easy

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answer. Hempel's diaries and letters in the 1940s contain remarkably little discussion of scientific explanation. There is no explicit eureka moment predating the shift of the 1948 article, nor is there evidence that Hempel suddenly found interest in the work of Meyerson or Rickert. Without addressing all relevant aspects of Hempel's intellectual development here, there are several interactions in the 1940s that both shaped the path toward the 1948 article and yield an interesting perspective on its philosophical significance.[4]

After Hempel's doctoral defense at the University of Berlin in 1934, he had no academic position but was employed as the private research assistant of Paul Oppenheim in Brussels. In the academic year 1937–1938, Carnap hired Hempel as his research assistant at Chicago University but could not sustain his employment for more than a year. In the summer of 1938, Hempel returned to Belgium to work under Oppenheim. Yet, he was set to continue his career in the United States and managed to acquire a travel visa for the next year. When Hempel arrived in the States in February 1939 without any prospect of employment, he was greeted by Ernest Nagel at the docks of New York Harbor. Nagel immediately integrated him into the flourishing scene of New York philosophy. Searching for an academic position, Hempel was eager to become more familiar with the research of his American colleagues. Nagel welcomed Hempel to his Logic graduate course at Columbia University and introduced him to what was called the *Nagel-Hook circle*, a discussion group of New York–based philosophers who regularly came together to discuss their works in progress.[5]

In his diary entry of February 14, 1939, Hempel noted that, to his surprise, Nagel's coursework in logic had "many historical perspectives; critical discussion of Aristotelian and neo-Tomistic conception of science."[6] Nagel's course in logic comprised not only "a formal and highly technical study of principles of inference" but also "a study of the generic features of the methods used in acquiring knowledge, and an account of the nature of scientific systems of knowledge."[7] For the discussion of the nature of science, Nagel did not start from logical empiricist ideas but discussed Aristotle's account of scientific knowledge from the *Posterior Analytics* in great detail. For Nagel, the *Posterior Analytics* was not an obsolete relic from the history of ideas. It could still provide the starting point to discuss the very nature of science because the book is "based on a sharp contrast between scientific knowledge and common sense knowledge."[8] In his course notes, Nagel noted that for Aristotle, scientific knowledge was "not simply knowledge of the fact, but of the reasoned fact, to know the reason

[4] For a more complete overview of Hempel's intellectual development in the context of German and American interbellum philosophy, see Dewulf (2018a).

[5] Hempel Diary, March 18, 1939, CH 2-1-1 Archives for Scientific Philosophy (henceforth, *ASP*), University of Pittsburgh.

[6] "Viele historische Ausblicke; kritische diskussion Aristotelische und neothomistischer Wissenschaftsauffassung." Hempel Diary, February 14, 1939, CH 2-1-1 ASP. At Columbia University, there was a strong interest in Aristotelian philosophy. John Dewey, John Hermann Randall, and Frederick Woodbridge all, at some point, lectured and wrote on Aristotle. In this sense, it is no surprise that Aristotle was discussed in detail in Nagel's graduate course in logic at Columbia University.

[7] Box 21, folder "Teaching Material: Logical Theory," Ernst Nagel Papers, Rare Book & Manuscript Library, Columbia University.

[8] Box 21, folder "Teaching Material: Logical Theory," Ernst Nagel Papers, Rare Book & Manuscript Library, Columbia University.

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why the fact occurs is the distinctive mark of science."[9] Nagel continued to discuss Aristotle's theory of the demonstrative syllogism in his notes as an attempt to capture the logical features of the distinctive mark of science, namely, to explain phenomena. Although Hempel only sat in on this course for the spring term of 1939, it could have been more influential on Hempel's larger commitments than Hempel himself may have realized. Nagel introduced the logic of scientific explanation as a way to understand scientific knowledge in these classes not only to Hempel but also to a number of Columbia graduates, such as Morton White and John Hospers, who would take up this perspective on the sciences in their early writings. During the summer of 1939, Hempel got a job as a philosophy teacher at the City College of New York, and from 1940 onward, he was an associate professor at Queens College. Because of these appointments, Hempel's intellectual environment became different from his previous logical empiricist surroundings in Berlin, Vienna, and Chicago.

In the Nagel–Hook circle, Hempel also gave a talk in 1941 on a topic that was characteristic of New York philosophy at that time: the validity of the distinction between historical and natural scientific knowledge. In this talk, Hempel defended the unity of science, arguing that the function of scientific concepts is the same in all scientific practices, including historiography.[10] Hempel does not yet mention that scientific concepts have an explanatory aim. Instead, they merely systematize the available phenomena through hypothetical generalizations (Hempel 1942, 36). This talk was published as "The Function of General Laws in History" in the *Journal of Philosophy* in 1942. It is often characterized as Hempel's first article on explanation (Salmon 1989, 25), even though both (E) and (A) are absent from it. The reason why it is characterized as an article on explanation is that Hempel's logical articulation of the function of laws resembles the logical sketch of an explanation from the 1948 article, and Hempel already refers to an instance of that sketch as something that might be called "an explanation." Yet, the crucial assumption that explanation is an aim of scientific knowledge distinct from description is still absent.

Even if Hempel's (1942) article on history does not yet propose a theory of explanation, its prominent use of the term *explanation* is striking. Otto Neurath, who was a vehement proponent of Machian descriptivism, noticed this and launched a campaign from Oxford during the Second World War to halt the American influence on the younger philosophers within the Unity of Science movement. Because Neurath believed that all members of the movement were committed empiricists and descriptivists, he wrote multiple letters to Feigl, Morris, and Carnap warning about the danger of introducing explanation as a worthwhile concept in the logical interpretation of scientific knowledge. To Carnap, he wrote:

> Of course, I am against the expression "explanation" because we—that is the Mach school, if you would use this term,—Philipp Frank, etc. try to avoid

[9] Box 21, folder "Teaching Material: Logical Theory," Ernst Nagel Papers, Rare Book & Manuscript Library, Columbia University.

[10] The target of Hempel's argument was Heinrich Rickert's theory that the historical sciences produce concepts that cannot operate within universal laws covering historical events (Rickert 1929). For a discussion of the broader philosophical and historical context of Hempel's talk (later published as an article), see Dewulf (2018b).

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> "explanation" as something besides finding correlations. You know the discussion about "Erklaerung" and Kirchhoff[']s statement on "description." Of course it would be pedantic always to avoid the term "explanation" but I personally should dislike to use the term as the heading of scientific analysis as such.[11]

He was especially worried about the evolution of Hempel's ideas and wrote to Carnap: "Hempel READING my papers LISTENING to my arguments did not REMEMBER afterwards these points, BECAUSE THEY ARE SO FOREIGN TO HIM."[12] Neurath also questioned Hempel's overzealous attempts to analyze concepts through logic. "I like Hempel's attitude very much, but I often suggested to him, he should deal a little more with scientific problems as such and then apply his analytical ability to them."[13]

Between 1934 and 1944, Neurath and Hempel had an extensive correspondence; a dominant theme of theirs was the use of formal logic to analyze science. Whereas Hempel thought it advantageous, Neurath considered it dangerous and warned Hempel:

> The introduction of old metaphysically-laden terminology through new logical definitions is not good: the traditional meaning slips in and confuses everything, not only the reader, but also its user. Even strong persons succumb to Word idols.[14]

Hempel generally did not engage with Neurath's concerns, and in the 1940s, he paid little to no attention to Neurath's empiricist warnings against supposedly metaphysically laden terminology, such as *explanation*. For Hempel, the ideas of his new American colleagues were more important than Neurath's references to older European philosophers like Ernst Mach.

In 1943 Morton White, at the time a Columbia graduate student, continued to work on Hempel's (1942) article and the ideas on explanation introduced in Nagel's graduate course. In the article "Historical Explanation," White argued that *explanations*, which he understood as scientific answers to "why" questions, have a similar structure in both physics and historiography (White 1943). John Hospers equally picked up on the topics familiar from Nagel's classes. He worked on an account of explanation by analyzing its use in ordinary language. Hospers, a regular attendant of the Nagel–Hook circle, published this work in the *Journal of Philosophy* in 1946. Hospers's article "On Explanation" is not an analysis of explanation as an aim of science per se. It is about "why" questions in general language use: "We are sometimes presented with a statement describing some observed fact, and when we ask 'Why?' we are presented with another statement which is said to constitute an 'explanation' of the first" (Hospers 1946, 337).

[11] Neurath to Carnap, April 1, 1944, RC 101-55-05 ASP. Gustav Robert Kirchhoff (1824–1887) was a German physicist who took up a descriptivist position similar to that of Ernst Mach.

[12] Neurath to Carnap, June 16, 1945, RC 102-55-11 ASP.

[13] Neurath to Carnap, June 16, 1945, RC 102-55-11 ASP.

[14] Neurath to Hempel, March 25, 1935, Nr. 244, Vienna Circle Archives, Noord-Hollands Archief Haarlem.

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For Hospers, "why" questions and explanations are intimately linked. In the article, he used his linguistic intuitions to evaluate several possible positions on what constitutes a proper explanation statement, which he understood as an answer to a "why" question. He comes up with several candidates but reaches no definitive conclusion. Hospers's article only attempts to form an ordinary-language model of answers to "why" questions. In Hempel and Oppenheim's article, published two years later, "why" questions also play a central role: scientific explanations should have continuity with ordinary-language explanations. This continuity still forms a central aspect of the post-Hempelian debate on scientific explanation (Woodward 2017; Skow 2016). It is very likely that the connection between "why" questions and scientific explanations originates from Hempel's familiarity with Hospers's work, which is also quoted as an inspiration in the 1948 article (Hempel and Oppenheim 1948, 140).

Thus, the historical record strongly suggests that Hempel and Oppenheim's introduction of the explanationist assumption (E) was not the result of an engagement with earlier debates. Most likely, for Hempel and Oppenheim themselves, the extent to which (E) departed from the logical empiricist tradition remained unnoticed. As I mentioned earlier, neither Hempel's correspondence nor his diaries of the 1940s attest to a change of mind.[15] The 1948 article can be read as Hempel and Oppenheim's way to extend the logical empiricist tradition with novel concerns from American colleagues. They present the DN model not only as an analysis of scientific explanation but also as a way to reconceive the concept of causality in science,[16] think about emergent properties in biology, and create a model for the systematic power of a theory. Although the DN model can be understood as a deflationary, empiricist view on these matters, Hempel and Oppenheim introduced and defended it specifically as an analysis of explanation, a distinct aim of science. By offering this analysis and introducing (E), they created the possibility for other philosophers to introduce nondeflationary models of explanations. Throughout this transition from deflationary to nondeflationary (often causal) accounts, (E) was generally accepted while glossing over important philosophical questions about (E) that had been central to the earlier debates. This raises important questions.

5. Reframing the debate: Descriptivism versus explanationism

How Hempel's article on scientific explanation became so popular after its publication is a story in its own right. I cannot go into its details here—it is connected with the fact that Hempel's university position changed after 1948, first to Yale and then to Princeton, and the fact that the 1948 article was reprinted in important volumes that would shape the budding field of the philosophy of science in the 1950s and 1960s.

[15] Hempel's diaries from 1939 to 1945 prove that Paul Oppenheim joined Hempel for most sessions of the Nagel–Hook circle. Their article in 1948 can best be understood as the joint response of Hempel and Oppenheim to the American philosophical climate that they experienced in New York.

[16] In the 1948 article, Hempel and Oppenheim maintain that "unexceptional connections between specified characteristics of events" can be referred to as *causal* connections, in contrast with regular connections of a statistical, indeterministic nature (Hempel and Oppenheim 1948, 139). Similar deflationary ideas about causation were defended by various earlier philosophers, most notably Russell (1912), Wittgenstein (1922/1981), and Reichenbach (1930). I am grateful to one of the referees for stressing the importance of this connection.

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What is, however, crucial about the reception of the article is that none of its readers questioned (E) and (A). As Salmon noted, because of Hempel's work, "the temptation to say that there is no such thing as scientific explanation seems to have vanished" (Salmon 2000, 315). Hempel's assumptions became the bedrock on which a steady research program could be formed (Reck 2013).

As the last eighty years of research have shown, both assumptions in tandem are fruitful. They give us many competing models of the explanatory aim of science, some models faring better than others. However, these models are all committed to the assumptions that made them possible, and those assumptions are not neutral. In *Depth*, Michael Strevens articulated the assumption (E) in the following way: "There is an underlying set of principles that has always determined what does and does not count as a good explanation for us. Any change in explanatory practice is due to some change in the parameters of those principles" (Strevens 2008, 37–38). This assumption is an explicitly historical one, that scientific practice has had a consistent explanatory aim as long as it existed. This is not a self-evident assumption; it requires a rich historical insight into scientific practice and a narrative of modern science that highlights a continuity that is not easily imposed on the record of intellectual history.

Strevens aims to take our explanatory practice as the most important evidence for a descriptive model of scientific explanation and defines our practice as the "sum total of explanations regarded as scientifically adequate in their day, together with an understanding of the background against which they seemed adequate" (Strevens 2008, 37). But as noted before, this only makes sense once one has already assumed that the search for explanation is a central, distinct aim of scientific practice both in time and across disciplines. Whether a reference to explanation in the practice of science, past or present, should be interpreted as an expression of the explanatory aim of science is itself something at stake, not something that can be taken for granted. Given (E), such references can be conceived as evidence for the philosophical model. Yet even then, any reference might also be interpreted as a mistaken expression of the explanatory aim of science. Even with (E) as the driving assumption, one still has to address the interpretive challenge of selecting which examples are expressions of the explanatory aim: there is no given, uncontested set of explanations regarded as scientifically adequate in their day. Any explanationist position shares this challenge.[17]

In most discussions of scientific explanation, an explicit motivation for (E) remains absent. One recent author who addresses this issue is Kevin McCain. He points to important scientific institutions, such as the National Research Council, that advocate the position that "the goal of science is the construction of theories that can provide explanatory accounts of features of the world" (McCain 2016, 134). According to McCain, the importance of explanation in scientific practice is related to science's aim of increasing our understanding of the world. Quoting Erwin Schrödinger,

[17] For example, in the introduction to *Making Things Happen* (2003), James Woodward makes clear that he is not interested in examples of scientific explanation in the abstract. Instead he aims to provide a model only for those examples that can be reconstructed as providing information that is potentially relevant to manipulation. According to Woodward, descriptive knowledge does not necessarily provide such information (Woodward 2003, 10).

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McCain claims that the scientific worldview rests on the hypothesis that nature can be understood (McCain 2016, 135). Yet, he never provides an argument that motivates such an interpretation of scientific theories and explains why other possible interpretations (e.g., descriptivism) should be excluded.

Currently, contemporary explanationists have no argument to support the assumption that enables their models of scientific explanation to become meaningful. In the search for such an argument, much might be gained: the presentation of explanation as a central aim of scientific theory and practice cannot be an epistemologically and metaphysically neutral image of science. Aristotle's model of explanation in the *Posterior Analytics* explicitly relies on an essentialist account of natural events and its associated standards for the knowledge of nature. Similarly, the debates between Meyerson, Duhem, Cassirer, Mach, and Rickert are embedded within questions concerning the nature of scientific concepts, the standards of rationality, and the potential variability of those standards throughout the history of science. In the contemporary philosophy of science, Bas van Fraassen's constructive empiricism develops a descriptivist position within such broader epistemological reflection.

According to van Fraassen, the acceptance of scientific theories involves only the belief that the theories are empirically adequate, that they yield correct descriptions of observable phenomena. Accordingly, explanation is not a distinct aim of science: van Fraassen rejects (E). Although van Fraassen's ideas are often presented as a "pragmatic" theory of scientific explanation, this is a deceptive categorization. As a committed descriptivist, van Fraassen has to address explanation-talk by scientists. Just like Duhem, Mach, or von Mises, a contemporary descriptivist has to interpret scientists talking about the explanatory power of their theory within a descriptivist understanding of science. As a consequence, what van Fraassen offered was a pragmatic theory of answers to "why" questions, a philosophical account of a common linguistic practice, namely, that people ask "why" questions, including scientists. van Fraassen disambiguated two kinds of philosophical research programs that Hempel had tied together in 1948: the analysis of answers to "why" questions (part of ordinary-language philosophy) and the discussion about the aim(s) of science. In *The Scientific Image*, van Fraassen gives a pragmatic account of "why" questions, arguing that explanations are essentially relative to the context in which a "why" question was posed (van Fraassen 1980, 156). With such an account, van Fraassen can say that requests for explanation in science merely "satisfy certain of our desires; and these desires are quite specific in a specific context, but they are always desires for descriptive information" (van Fraassen 1980, 156). For van Fraassen, there is no single desire for understanding that is uniformly expressed within scientific practice. Because van Fraassen's model of "why" questions does not aim to capture a uniform goal of science, it is deceptive to categorize it as a theory of scientific explanation, pragmatic or otherwise. It is best understood as a descriptivist position that uses a pragmatic model for answers to "why" questions to interpret explanation-talk by scientists.

Just like contemporary explanationist positions, van Fraassen's descriptivism faces a challenge in interpreting the history of science. He must argue that episodes in the history of science that are regarded as scientific progress *can* be interpreted within descriptivist terms. van Fraassen has recognized this challenge and has given a descriptivist interpretation of Perrin's experimental work, which is often taken as

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the paradigm case of a scientific episode where explanation plays a necessary role (van Fraassen 2009). At least since *The Empirical Stance* (2002), van Fraassen also addressed the broader challenge of how to motivate a denial of (E) beyond the *mere coherence* of a descriptivist interpretation of scientific knowledge. His categorization of empiricism as a stance is an attempt to embed his descriptivism within a voluntarist epistemological project: the empiricist stance against the desire to achieve a uniform explanation of the world is motivated by a coherent set of empiricist values.

6. Conclusion

Reframing the contemporary debate on scientific explanation from a broader perspective on the twentieth-century history of philosophy has proven fruitful for several reasons. First, the distinction between explanationism and descriptivism helps to capture the larger commitments and accompanying challenges that are at stake in discussing scientific explanation. Starting discussions on scientific explanation from Hempel and Oppenheim's article onward brackets such commitments and takes explanationism for granted. Motivating why one should accept explanation as an aim of scientific knowledge is a serious philosophical problem, one that theories of scientific explanation should address.

Second, the earlier-twentieth-century debates also offer insights into the challenge of defending or rejecting (E). A mere reference to "explanation-talk in science, perhaps quoting Schrödinger or an official statement of the National Research Council, does not provide proper motivation for (E), and neither does an anthropomorphic reference to the deep human need for understanding." Next to the problem of motivating one's position on the explanatory aim of scientific knowledge, there is also the interpretive challenge of deciding which examples count as expressions of the aim of science and which do not. Explanationists in the Hempelian tradition have given very little attention to this latter problem, even though it is an implicit interpretive decision that one constantly has to make if one shares the driving assumptions of Hempel's work on explanation. Most likely, finding an argument for explanationism will help to motivate an interpretive decision on the examples that can serve as evidence for the philosophical model of explanation.

Finally, the distinction between explanationism and descriptivism enables a better classification of contemporary positions. Not all models of explanation in science are models that rely on (E). van Fraassen's work is a good example: its pragmatic model of answers to "why" questions does not provide a model of scientific explanation as an aim of science. In future discussions about scientific explanation, the relation between offered models and (E) can be used to map how a theory about scientific explanation relates to broader commitments and challenges within the debate.

Even though Hempel and Oppenheim said that there was a general agreement on the explanatory aim of science, and Salmon was relieved that the descriptivist position had received little attention in analytic philosophy of science, I have argued that the acceptance of explanation as an aim of science is by no means philosophically neutral. As long as there is no convincing argument that proves the descriptivist position incoherent, the use of the concept of "explanation" in our epistemic understanding of science remains contentious and requires further motivation.

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