

Chapter 4

The Emergence of Scientific Explanation as a Problem for Philosophy of Science: Aristotle, Nagel, and Hempel



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Abstract In this paper I trace Ernest Nagel's earliest ideas on explanation by investigating his course-notes of the 1930s. At Columbia University there was an increasing interest in the study of Aristotle. As I show, Nagel's focus on the explanatory aim of science originated from his reading of Aristotle's *Posterior Analytics*. Through his teaching of Aristotle, Nagel inspired his New York colleagues to focus on a philosophical analysis of explanation. I claim that this resulted in Carl Hempel's earliest work on scientific explanation. Although scientific explanation was not a central topic for philosophers of science in the 1940s or 1950s, Hempel and Nagel's interest in the topic helped to canonize it in a period when the topics and methods in philosophy of science became increasingly standardized.

Keywords Aristotle · Scientific explanation · Carl Hempel · Ernest Nagel · Posterior analytics

4.1 Introduction

Reflections on systematic knowledge (science) have been around, at least, since pre-Socratic philosophers. However, philosophy of science as a domain of inquiry that is institutionally individuated as a subdiscipline of academic philosophy is a much more recent phenomenon. Most of the institutional infrastructure for philosophy of science only arose in the 1960s: standard textbooks, separate research centers for philosophy of science, graduate programs and a professional association with its own conference series (Richardson, 2012). Alongside the institutionalization of philosophy of science in the 1960s, the topics of research and teaching for the domain were also standardized. During this period, the analysis of scientific explanation became a central topic—which it still is to this day. From the 1940s onwards, two philosophers in particular defended that an analysis of scientific explanation should

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take the center stage of teaching and research in philosophy of science: Carl Hempel and Ernest Nagel.

In the influential summary-essay, “Aspects of Scientific Explanation”, Hempel assumed that two “enduring concerns” provide the stimulus for humanity’s desire for scientific achievement. On the one hand, human beings had always desired to control the surrounding environment. Science was an important means to this end. On the other hand, science was meant to quell “man’s deep and persistent desire to know and to understand himself and his world” (Hempel, 1965, p. 333). As a consequence, any understanding of science had to enlighten the logical structure of explanation which enables human beings to fulfill the desire to understand their world. Hempel’s covering law model was designed to offer such enlightenment. In a similarly influential philosophy of science book from the 1960s, *The Structure of Science*, Nagel started his inquiry from a similar assumption:

It is the desire for explanations which are at once systematic and controllable by factual evidence that generates science; and it is the organization and classification of knowledge on the basis of explanatory principles that is the distinctive goal of the sciences. (Nagel, 1961/1979, p. 4)

Because Nagel believed that the quest for explanatory knowledge is an expression of a basic impulse of human nature and represented a “distinct variety of human experience”, he argued that science, as the most systematic expression of this human impulse, was a necessary element in any humanist education (Nagel, 1959, p. 57). Today, philosophers of science still eagerly use this anthropological legitimation to argue that scientific explanation is the pre-eminent goal of scientific knowledge. Michael Strevens 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, p. 3). Bradford Skow (2016, p. 525) notes that “[t]he natural curiosity that we are all born with, and that most of us retain throughout our lives, is in part a curiosity about why things happen”, and answering such why-questions is the most important aim of science.

Since the 1960s philosophers of science have taken for granted that explanation is an aim of science in need of some kind of analysis and they intended to use that analysis for the improvement of scientific practice through philosophical modeling, or for an insight into the nature of science. However, this assumption was not shared by many philosophers from the first half of the twentieth century. As Wesley Salmon (1999, 338) already remarked, logical empiricist philosophers before Hempel and Nagel had little to say about scientific explanation as an aim of science. Both Carnap and Reichenbach never attempted to explicate explanation as a separate aim of scientific inquiry.¹ Neither did they conceive scientific knowledge as an expression of the human desire to understand the world. This should not

¹ In *The Rise of Scientific Philosophy*, Reichenbach (1951, p. 5) states that the essence of scientific knowledge is generalization. Similarly, Carnap (1966, p. 3) believed that science ultimately aimed to capture the regularities in our observations. They both claimed that the subsumption of a singular event under a generalization can be called an *explanation*. However, neither defended that explanation was an aim of scientific inquiry distinct from description. As I will argue below, this distinction

be surprising, since logical empiricist philosophers, like Carnap, Reichenbach or Neurath advanced their analyses of science from an empiricist point of view. This implied that scientific theories should only be evaluated on their potential to account for available empirical phenomena, both past and future. In the philosophical generation before logical empiricism, Pierre Duhem and Ernst Mach had already argued that such an empiricist understanding of science did not distinguish an explanatory aim of science from a descriptive one (Mach, 1893/1974; Duhem, 1906/1991).

As Bas van Fraassen (1980, p. 156) pointed out, this empiricist stance toward the evaluation of scientific theories was abandoned once philosophers of science began to search for an objective model of scientific explanation in general—a model that intended to capture an objective relation between theory and observations that was distinct from the descriptive relation. Arguably, Nagel's and Hempel's most important contribution to philosophy of science as a discipline lay exactly in initiating the shift towards research on such an objective model of scientific explanation. Due to their efforts to promote the topic, explanation became an important subject for research and teaching in philosophy of science at the precise moment when philosophy of science as a domain of inquiry was institutionalized as a subdiscipline in American philosophy.

So far, there is no historical account how Hempel and Nagel, who are often grouped alongside other logical empiricist philosophers, became convinced of the position that explanation is a distinct aim of science—a position that was never defended by earlier logical empiricists and stands in contrast with earlier empiricist interpretations of science. In *Four Decades of Scientific Explanation*, Salmon opened his historical narrative of the debate on scientific explanation with the publication of Hempel and Oppenheim's article "Studies in the Logic of Explanation" (1948). However, this paper already starts from the assumption 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 & Oppenheim, 1948, 135). In the paper, Hempel and Oppenheim already assume that there is "a rather general agreement" on explanation as an aim of science distinct from description.² As I will show in Sect. 4.5, this was not the case: until the 1950s philosophers of science had little interest to analyze scientific explanation as a distinct aim of scientific theories.³

is central to the Aristotelian tradition in philosophy of science that Nagel, and indirectly Hempel, pursued.

² In the paper Hempel and Oppenheim do not give a proper account of how to understand the distinction between the descriptive and explanatory aim of science. In *Aspects of scientific explanation*, Hempel would relate explanation to the human search for understanding—something that cannot be achieved through mere description (1965, p. 333). When Hempel and Oppenheim wrote their paper in 1948, its thesis was not yet connected to the distinction between a realist and an anti-realist understanding of science.

³ Hempel's covering law model had several precursors (Hempel and Oppenheim 1948, p. 140), perhaps most notably in the work of John Stuart Mill, Karl Popper and Stanley Jevons. What makes Hempel's covering law model stand out from these precursors is its analytic perspicuity. It is offered as an analysis of the explanatory aim of science. In this regard, it can also be critiqued by showing how examples of the analysandum do not correspond to the analysans (see 4.4 for Further Discussion).

Thus, Hempel and Oppenheim's introduction of a consensus on explanation as an aim of science stands at odds with the contemporary intellectual disinterest to analyze explanation as a distinct aim of scientific theories.

Commentators on the history of the debate about scientific explanation usually refer to Aristotle as the origin of the debate. Salmon (1989, p. xiii) starts *Four Decades* by saying that “[p]hilosophers and scientists, since at least the time of Aristotle, have tried to say in what such scientific understanding consists.” James Woodward in his article on scientific explanation in the *Stanford Encyclopedia* claims that “[i]ssues concerning scientific explanation have been a focus of philosophical attention from Presocratic times through the modern period” (Woodward, 2017). Aristotle was the first philosopher to systematically analyze the nature of scientific knowledge, and he too took for granted that “all men desire by nature to know” (Met I, 1, 980a1), which implied a desire not only for knowledge of what happens, but also of why things happen (Met I, 1, 981a27). How Aristotle's understanding of scientific knowledge is historically related to Hempel's model of scientific explanation has so far never been explored.

Salmon (1989, p. 10) simply labeled all philosophical accounts of explanation before Hempel and Oppenheim's paper as prehistory. In this paper, I aim to close the historical gap by showing how Nagel's interest in the Aristotelian conception of science engendered an intellectual context in New York during the 1940s such that the nature of scientific explanation became an intellectual problem for the philosopher of science, despite Nagel's and Hempel's empiricism (Sect. 4.2). I will show that various elements of Hempel's covering law model of explanation were the result of this intellectual context (Sect. 4.3). I will also investigate how the new intellectual problems concerning explanation subsequently expanded from the New York context and became standardized for philosophy of science as a field in the 1960s (Sect. 4.4).

4.2 Ernest Nagel, Aristotle, and New York's Philosophy

After his doctoral defense in 1934 at the University of Berlin, Hempel had no academic position. Fortunately for him, Paul Oppenheim employed him as a personal research assistant 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, but was committed to continue his career in the United States. Eventually, he managed to acquire a travel to initiate his return. In January 1939, Hempel arrived in the US without any explicit prospect of employment, but he was warmly welcomed by Ernst Nagel at the docks of the New York Harbor. Nagel immediately integrated Hempel in 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 many of his New York philosophy colleagues. In his diary entry of 14 February 1939 Hempel noted that, to

his surprise, Nagel's coursework in logic had "many historical perspectives; critical discussion of Aristotelian and neo-Thomistic conception of science".⁴ Unlike what Hempel was used to from his experiences with Carnap or Reichenbach, Nagel was not an a-historical philosopher who only focused on contemporary science or formal problems. Nagel's philosophical interests were very different from what Hempel was used to.⁵

Nagel got his Bachelor degree at City College New York, where he was inspired mainly by Morris Cohen. Cohen set him on the path to discover modern, formal logic and the new European logical empiricism in the late 1920s. However, Nagel did not stay at City College, but pursued a graduate program at Columbia University where he received instruction from two philosophy professors. The first was John Dewey. Nagel was not fond of Dewey's teaching style, nor of Dewey's philosophy in particular. At the end of his life, Nagel recollects: "I found Dewey extremely impressive as a human being, but I'm not really sure that I understood the pragmatism that he represented".⁶ Nagel's second teacher at Columbia, who also supported his application to the graduate program, was Frederick Woodbridge. He taught the history of philosophy at Columbia and turned out to be much more influential on Nagel than Dewey.

When I started graduate work in philosophy, Woodbridge influenced me. He was a kind of Aristotelian who in part took a view similar to that of Cohen, believing in the reality of universals. Be he didn't take a Platonistic view, but rather an Aristotelian view on these matters. ("Interview with Ernest Nagel," this volume, p. XY)

Woodbridge had a great interest in Aristotle's philosophy: posthumously, one of his lecture series on Aristotle was edited by John Hermann Randall (Woodbridge, 1966).⁷ When Woodbridge retired from Columbia University in 1931, Nagel became an instructor there. At the time, philosophy education at Columbia was mainly oriented towards the history of philosophy. Discussions of contemporary problems in philosophy were largely absent. Nagel was hired to change this and provide courses on the new developments in logic. In his logic course, Nagel combined a discussion of modern logic with a discussion of problems in epistemology and philosophy of science.

I was hired to do something about the Logic courses. ... I thought if you are going to have students who will be prepared to teach all over the lot in philosophy, they ought to be exposed to some discussion in traditional epistemology. So I began giving courses in the theory of

⁴ "Viele historische Ausblicke; kritische diskussion Aristotelische und neothomistische Wissenschaftsauffassung." Hempel Diary, 14 February 1939, Carl Hempel Papers (CH) 02-1-1 Archives of Scientific Philosophy (ASP), Hillman Library, University of Pittsburgh, all rights reserved.

⁵ On Nagel's historical approach to science and philosophy, see Thomas Mormann's chapter in this volume.

⁶ "Interview with Ernest Nagel," in this volume, p. XY.

⁷ Randall was Woodbridge's successor. He was specialized in the history of philosophy, played an important role as editor of the *Journal of Philosophy* and as staunch opponent to formal approaches in philosophy (Jewett, 2011).

knowledge, both on the college level and graduate level. And the third thing that I think was an innovation—and that was hardly a regular subject when I was a student—was the work in the philosophy of science, and so that, too, became a rather major part of the offering of the department. (“Interview with Ernest Nagel,” this volume, p. XY)

In the 1930s there was no standard textbook in philosophy of science, nor any anthology of important literature in the field. Such volumes only became available in the 1950s and 1960s. There were no standard topics, like scientific explanation or standard entry-points into debates, like the deductive-nomological model. Nagel could decide entirely on his own what philosophy of science or the logical investigation of science entailed. Therefore, it is interesting to investigate what Nagel actually taught in his classroom in these early years. In the above quoted recollection from 1983 Nagel separates logic, epistemology and philosophy of science as three distinct domains of inquiry, but in the 1930s, when he started his logic course at Columbia, his actual classes were an interplay of all three.⁸ Rather than initiate his students in modern formal logic alone, Nagel decided to introduce them to the different, “warring” conceptions of logic in the history of philosophy, from traditional syllogistic, over mathematical logic, to metaphysical, idealist logic, and transcendental logic which studied “the presuppositions which the mind employs in organizing the sensuous material into significant experience”.⁹ In this early stage of Nagel’s career, he believed that an overview of contemporary logic was ipso facto an overview of the struggle of the various philosophical traditions, of which logical empiricism was only one among many.

The aim of his course was not “primarily historical” as he remarked in his introductory lecture.¹⁰ However, by discussing the history of the logic of knowledge, Nagel wanted to introduce his students to two important aspects of rational inquiry in general. The first was a narrow conception of logic as “a formal and highly technical study of principles of inference and of statements which may be certified simply on the basis of these principles” (ibid.). The second was a broader conception of logic as “a study of the generic features of the methods used in acquiring knowledge, and an account of the nature of scientific systems of knowledge”. The latter conception of logic involved a “rational reconstruction” of the operations yielding knowledge, and “to the extent that this is normative, it involved questions as to the source and validity of the norms proposed”. Both aspects of logic were related to each other: the technical study of inference was often used to clarify the norms for scientific knowledge.

Nagel introduced this intimate link between the two aspects of logical inquiry through an extensive investigation of Aristotle’s philosophy of science (ibid.). Aristotle’s completed works was consistently mentioned on the reference list for Nagel’s

⁸ The book *An Introduction To Logic And Scientific Method*, co-authored with Morris Cohen, is a good example of the same mix of genres (Cohen and Nagel 1934).

⁹ “Logic 101–102, 1933–4”, Ernest Nagel Papers (ENP), Box 16, folder “Notebooks Logic”, Rare Book and Manuscript Library, Columbia University, New York. Quoted by permission of Columbia University. All rights reserved.

¹⁰ “Teaching Materials Logical Theory”, ENP, Box 21, folder “Teaching Materials”.

logics course from the 1930s to the 1950s, and it is clear that his students were required to read passages from the *Posterior Analytics* in particular.¹¹ 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 scientific knowledge, because in this work Aristotle had captured some of the central norms for scientific knowledge that were still relevant in the modern age, and especially how these norms should be understood from a logical point of view. In his course notes from the 1930s Nagel used Aristotelian philosophy of science to discuss the distinction between common sense and science. According to Nagel, Aristotle made “a sharp contrast between scientific knowledge (which is of the stable and the universal) and common sense knowledge (which is of the particular, the impermanent, being discordant, and based on a variety of different authorities)”.¹²

This distinction would also be the starting point of Nagel’s own introduction in *The Structure of Science* (1961). There, he argued that common sense knowledge is distinguished from scientific knowledge, because the latter has an interest in systematically explaining the facts (1961/1979, p. 6). Like Nagel in *Structure*, Aristotle too identified the search for explanations as the distinguishing mark of scientific knowledge: science is “not simply knowledge of the fact, but of the reasoned fact.”¹³ For Aristotle, knowledge of the reason why the fact occurs is the distinctive mark of science. Just like Nagel’s own theory of scientific explanation, Aristotle’s theory of the demonstrative syllogism was not meant to be used as a guide for scientific inquiry in practice, but as an analysis of “the logical patterns exhibited by explanations in the sciences”.¹⁴ Aristotle was engaged in the same project as Nagel, to perform a rational reconstruction of the distinguishing feature of scientific knowledge, i.e. how it systematically offered insight into why phenomena occurred. Although some of Aristotle’s norms for explanation were no longer valid, Nagel understood Aristotle’s theory as a good stepping stone to an up-to-date normative account of the explanatory nature of scientific knowledge. As Nagel said to his students in the introductory course: “Though historical literature will be employed, the aim is to examine this literature critically, in light of the best available contemporary evidence.”¹⁵

In his course notes, Nagel focuses mainly on the logical, demonstrative structure of Aristotle’s model of explanation. Explanations have an inferential character to them. “By demonstration Aristotle means a syllogism productive of scientific knowledge, a syllogism, that is, the grasp of which is eo ipso such knowledge.”¹⁶ Crucial to Aristotle’s theory was that only those syllogisms were explanatory which exhibit a

¹¹ “Reading Lists for Philosophy 103, 1944/1946/1948”, ENP, Box 21, folder “Teaching Material Logical Theory”.

¹² “The aim of the Posterior Analytics”, ENP, Box 21, folder “Teaching Material Logical theory”.

¹³ Ibid. Nagel quoted APo I, 13, 79a15: “It is for the doctor to know the fact that circular wounds heal more slowly, but it is for the geometrician to know the reason for the fact”.

¹⁴ Ibid.

¹⁵ Ibid.

¹⁶ Ibid.

middle term that expresses the cause of the phenomenon to be explained.¹⁷ Nagel emphasized that this condition made Aristotelian explanations asymmetrical. He gave the following example of this characteristic. One might prove the fact that the planets are near as follows:

All planets do not twinkle.
 All things that do not twinkle are near.
 Hence, the planets are near.¹⁸

But, this syllogism, although formally valid and true, is not explanatory. It merely proves the fact and not the reason why: the planets are not near because they do not twinkle. It is the other way around: the planets do not twinkle in virtue of the fact that they are near.

All planets are near.
 All things that are near do not twinkle.
 Hence, the planets do not twinkle.

In this syllogism, the middle term (“all things that are near do not twinkle”) expresses the cause of the conclusion. The middle term expresses an aspect of the nature of planets (their proximity to the earth) which causes the planets to lack any twinkling. Explanatory arguments should always include information on the inherent nature of things, and the universal statements that make up the middle term should be an expression of that nature.¹⁹ At the end of the section on Aristotle, Nagel compared Aristotle’s theory of demonstrations with the norms for contemporary scientific theories: both had an ideal of a systematic classification of natural phenomena—an ideal that can be expressed in logical terms. The greatest difference lay in the norms for this classification. Aristotle demanded that the explanations should be found in the definitions of things, expressing their form or nature, and not in the lawful systematization of their sequential behavior.²⁰ When Nagel explicated the logical form of scientific explanation in his *Structure*, he abandoned this Aristotelian demand: the scientific laws that have to be used in explanations, are not required to exhibit causal information that express the nature of the things to be explained. As long as the laws express phenomenal regularities they can legitimately operate in explanations, even though some regularities function better because of their capacity to capture more and different kinds of phenomena (Nagel, 1961/1979, p. 78). In *Structure*, Nagel did not explicitly contrast this approach to the Aristotelian one, even though, given his course notes, he was clearly aware of the contrast.

¹⁷ “Teaching Material 1939: Aristotelian Theory of Science”, ENP, box 25, folder “Teaching Material: miscellaneous”.

¹⁸ “The aim of the Posterior Analytics”, ENP, Box 21, folder “Teaching Material Logical theory”.

¹⁹ Ibid.

²⁰ Ibid.

4.3 The Logic of Explanation: Hempel's New Puzzle

Hempel only sat in on Nagel's logic course for the spring term of 1939, since he was unemployed at the time. However, the course could have been more influential on Hempel's larger commitments than Hempel himself may have realized. Nagel introduced Hempel to the topic of scientific explanation as a way to understand the central aim of scientific knowledge. Until that point, Hempel had never been introduced to the idea that scientific knowledge even by contemporary standards could be evaluated on its explanatory power as distinct from its descriptive adequacy. In the summer of 1939, Hempel got a job at City College and subsequently at Queens College New York in 1940. As a consequence, the philosophical debates in New York would be Hempel's direct intellectual context for his first eight years in the United States. During this time, Hempel participated in the Nagel-Hook circle, also known as the New York circle, which was an informal discussion group of New York philosophers organized by Nagel and Sidney Hook (Nollan and Hempel 2000, p. 19).

In 1941, Hempel was invited by the circle to discuss whether historical knowledge could be called science. This talk was eventually published in the *Journal of Philosophy* as the "Function of General Laws in History" (Hempel, 1942). In that paper, Hempel discusses the use of universal sentences to explain and predict historical events. In New York philosophy at the time, the scientific status of historiography and the status of historical laws was a major point of contention: J.H. Randall, Sidney Hook, Arthur Lovejoy and Philip Wiener had been discussing these issues in previous years (Dewulf, 2018, p. 401). Hempel's paper was a continuation of these discussions. Hempel highlighted that historical laws function as systematic representations of lawful relations among historical events. If a historical event is deduced from initial conditions and historical laws, such a deduction can be called an historical explanation or prediction. From this logical vantage point, Hempel assumed historiography was similar to other scientific disciplines (Hempel, 1942, p. 47). However, in the paper Hempel did not yet distinguish explanation from description as two distinct aims of science.

The question whether explanation is a distinct aim of science was part of Hempel's New York context. Philip Wiener, who was a lecturer at City College, had been familiarizing New York philosophers with the French debate between Emile Meyerson and Pierre Duhem on the explanatory aim of science.²¹ Duhem claimed that explanation was not the aim of physical theories and Meyerson had argued for exactly the opposite position, that explanation is the foremost objective of science. Wiener reviewed Meyerson's work and books on Duhem's philosophy for the *Journal of Philosophy* (Philip Paul Wiener, 1933, 1934, 1935, 1942). Eventually Wiener would also translate Duhem's *Aim and Structure of Physical Theories* to English in 1954. Wiener was a regular member of the Nagel-Hook circle which Hempel also frequented. Moreover, Nagel regularly listed Duhem's and Meyerson's works in the reading list for his

²¹ Within the logical empiricist network this debate also emerged during the second world war. Otto Neurath wrote both Carnap, Hempel and Feigl that an increased focus on the analysis of explanation moved the movement away from the empiricism of Mach and Duhem (Cat and Tuboly 2019, p. 622).

courses throughout the 1940s and 1950s.²² Nagel's philosophy of science course in the 1950s even began with the question "Do the sciences explain?", for which part of Duhem's and Meyerson's work was the point of reference.²³ Thus, Nagel's students were familiar with the intellectual challenge how to decide whether explanation is an aim of science distinct from description.

In 1941 Ernest Nagel also circulated a list for suggested topics for the Jones Prize, a graduate essay prize for Columbia students. Among the suggested topics, the following were listed: 'the structure of scientific explanation' and 'the logical structure of historical explanation'. In 1943 the topic for the prize was "types of explanation".²⁴ It is unclear who won the Jones Prizes during this period and what essay they wrote, but, at least, two Columbia graduates worked on these questions during the Second World War: Morton White and John Hospers. White's first paper, published in *Mind*, discussed historical explanation (White, 1943). White argued, similarly to Hempel, that historical events are explained if their occurrence can be deduced from true initial conditions and general laws. Whereas White only analyzed historical explanation in particular, John Hospers discussed explanations in general, not only in science, but also in natural language (Hospers, 1946). From an ordinary language point of view Hospers identified all explanations as answers to why-questions, and argued that any philosophical theory of explanation should address the various ways in which people actually answer such questions. In his paper Hospers came to no definite model or theory, but he left no doubt that scientists seek explanations. Since scientists, just like ordinary people ask and answer why-questions, scientists also offer explanations. However, Hospers did not defend the position that these explanations should be understood as an aim of scientific knowledge that is distinct from description. According to him, it could still be the case that scientists offer explanations by simply describing some state of affairs, and that their explanations can always be reduced to their capacity to describe the world (Hospers, 1946, p. 354).

Questions concerning explanation and its role in scientific knowledge were part of the philosophical context of Hempel during the Second World War: Nagel started his logic course with a discussion of Aristotle's *Posterior Analytics* and Wiener introduced New York philosophers during the 1930s to the debate between Duhem and Meyerson on the explanatory aim of science. The papers of White and Hospers are evidence that other New York philosophers, beside Hempel, were tackling questions concerning explanation. Most likely, this intellectual context was also Hempel's

²² "Reading List Philosophy of Science 1954", ENP, box 18, folder "Explanatory methods of empirical science"; "Reading List Philosophy 103 1952", ENP, box 18, "Teaching materials Logical theory (1952–53)".

²³ "Reading List Philosophy of Science 1952", ENP, box 24, folder "Teaching Materials Philosophy of science (New School, 1950)".

²⁴ "Teaching Material 1939: Aristotelian Theory of Science", ENP, box 25, folder "Teaching Material: miscellaneous".

intellectual motivation to work on a complete logical analysis of explanation as an aim of science. This was published in 1948 as “Studies in the Logic of Explanation”.²⁵

In their paper Hempel and Oppenheim give a set of examples of scientific explanations, like the explanation of the appearance of a broken oar in water. From this set, they abstract four conditions of adequacy for any scientific explanation. First, the explanandum must be a logical consequence of the explanans. Second, the explanans must contain some type of general laws required to infer the explanandum. Third, the explanans must have empirical content and, fourth, it has to be true (Hempel & Oppenheim, 1948, p. 137). These conditions of adequacy share several characteristics with Aristotle’s account of the demonstrative syllogism. First, both Aristotle’s and Hempel’s model state that explanations should be reconstructed as inferences. Second, both believe that some type of universal sentence is a necessary premise of any explanatory argument. In contrast to Aristotle, Hempel did not think that lawful sentences had to express the essential nature of the thing to be explained—the nature because of which the explanandum occurred. Just as Nagel had argued in his course, Hempel and Oppenheim also concluded that contemporary scientific knowledge does not aim to lay bare the causal structure of the world, or the essences because of which things occur.

Just as Aristotle, Hempel and Oppenheim set out to distinguish sound explanations from candidate-explanations by adding a material condition to the formal conditions of their model. They required that the lawlike sentences in the explanans had to be true. Aristotle similarly stipulated that all demonstrations must contain true premises (APo I, 2, 71b25). For Aristotle, this implied that the relation between subject and predicate expressed in the middle term was absolutely necessary. The relation had to be universally valid, and not contingently valid to a limited set of particular instances (APo I, 4, 73a25). For Hempel and Oppenheim, the truth-condition meant that high confirmation of lawlike sentences was not a sufficient condition for their use in sound explanations. They argued that a condition of high confirmation for explanatory laws would lead to cases where, at one point in time, some phenomenon was explained by a well-supported explanans, but would, at a later point in time, be disconfirmed by new evidence. In such cases, one would have to say that the old explanation was sound when the available evidence confirmed the explanans, but turned unsound when disconfirming evidence became at hand. This was an unsatisfactory result.

This does not appear to accord with sound common usage, which directs us to say that on the basis of the limited initial evidence, the truth of the explanans, and thus the soundness of the explanation, had been quite probable, but that the ampler evidence now available made it highly probable that the explanans was not true, and hence that the account in question was not—and had never been—a correct explanation. (Hempel & Oppenheim, 1948, p. 138)

As a consequence, all premises in the explanans should not only be confirmed by the available evidence. They also have to be true. Hempel was already confronted

²⁵ Paul Oppenheim is co-author of this paper. In the summer of 1946, Hempel wrote to Carnap that he was working on the manuscript of the paper during his holiday retreat with Oppenheim. However, the extent to which Oppenheim actually contributed to the content of the paper is unclear. Hempel to Carnap, 17 June 1946, Rudolf Carnap Papers, 84–19-25 ASP.

with this problem in 1942. Immediately after his publication of the “The Function of General Laws in History”, Hempel received a positive letter from Charles Stevenson, who was a regular visitor to the Nagel-Hook circle. After lengthy discussions with Nelson Goodman, Stevenson pressed Hempel on a problem: the crucial definition of laws in that paper. Hempel had defined a law as a hypothesis of universal form which is well confirmed by the available evidence (Hempel, 1942, p. 35). On Stevenson’s understanding, such a definition of law did not withstand scrutiny. “I think we usually expect ‘p is a law’ to be defined in a way that would imply ‘p is true’—and not merely, ‘p has some little confirmation’”.²⁶ For Stevenson, laws are true or false, independent of the confirmation that one has of them. Hempel’s definition of a law in 1942 did not correspond to this intuition. In his letter to Hempel, Stevenson already gives the argument in favor of a truth-condition which will reappear in the 1948 paper:

Should a general statement which was at one age well confirmed later become disconfirmed, we should not say ‘It was a law, but later ceased to be’, but rather, ‘It was thought to be a law, but we now have evidence for thinking that it wasn’t one.’

Stevenson’s philosophical response hinges on an assumed relation between the English language and philosophical concepts. Stevenson purely argues from the use of the concept ‘law’ in ordinary English sentences and never uses any example of scientific methodology in his reflections. Stevenson’s argument counters the core of Hempel’s starting point in 1942: laws are not systematizations of experience, their validity is independent of the experience that one has. In Hoppers’ paper on explanation, the truth-condition for explanatory laws was also defended: “the laws alleged must be true ones, else there is no true explanation” (Hoppers, 1946, p. 345). However, from an empiricist point of view, it would make perfect sense to say that certain laws cease to operate as laws relative to a new set of experiences, since these laws could no longer describe the new set. Duhem had explicitly defended that scientific laws are essentially provisional and relative to the accepted experimental information at any point in time. “Any physical law, being approximate, is at the mercy of the progress which, by increasing the precision of experiments, will make the degree of approximation of this law insufficient” (Duhem, 1906/1991, p. 174).

By adding the truth-condition to his model, Hempel sided with Stevenson’s and Hoppers’ concerns for the ordinary use of “law” in English. This also put Hempel at odds with Nagel’s ideas concerning the function of laws in explanations.

Ever since Nagel published *The Structure of Science*, his ideas on explanation have been consistently interpreted as similar to Hempel’s covering law model (Woodward, 2017; Salmon, 1989, p. 34). However, there are two important differences between their views. First, Nagel added no truth-condition for explanatory laws. Nagel remarked that “in point of fact we do not know whether the unrestrictedly universal premises assumed in the explanations of the empirical sciences are indeed true” (Nagel, 1961/1979, p. 43). Thus, he suggested to replace the truth-condition with a weaker one: “that the explanatory premises be compatible with established empirical facts and be in addition ‘adequately supported’ by evidence based on data

²⁶ Stevenson to Hempel, 3 February 1942, CH 38–3 ASP.

other than the observational data upon which the acceptance of the explicandum is based” (Nagel, 1961/1979, p. 43). Interestingly, Nagel explicitly contrasts his “empirical condition” for explanatory laws with Aristotle’s condition that the premises of explanatory arguments must be true and known to be true, referring to *Posterior Analytics* I, 2.²⁷

The second difference between Nagel and Hempel lay in their use of common sense intuitions concerning answers to why questions as valid evidence for a theory of scientific explanation. Nagel was only interested in capturing how experimental laws and theoretical principles function as the distinguishing characteristic of scientific knowledge in opposition to common sense knowledge. As a consequence, he never used ordinary answers to why questions as evidence for a theory of scientific explanation. Hempel, on the other hand, used common sense intuitions to discuss the nature of scientific explanation. His addition of the truth-condition is a good example of this. Ever since Hempel’s covering law model of explanation became influential, many philosophers have assumed that there should be a continuity between an analysis of scientific explanation and ordinary, every-day explanations (Woodward, 2017; Skow, 2016, pp. 524–525). For Nagel, however, an account of scientific explanation had the opposite function: it had to show how scientific knowledge distinguishes itself from the indeterminacy of ordinary language (Nagel, 1961/1979, p. 8). Both differences show how Nagel and Hempel’s work differed from a metaphilosophical point of view. Hempel was interested to present a philosophical model that provided a rational account of all possible examples of scientific explanation, including ordinary language examples of explanation. Nagel, on the other hand, wanted to provide an account of scientific knowledge in its opposition to common sense knowledge. The analysandum of their accounts was different, which resulted in interesting distinctions, e.g. concerning the truth-condition.

Hempel’s motivation to offer a logical analysis of explanation did not directly originate from his interest in Aristotelian philosophy of science. As far as I can ascertain, Hempel, in contrast to Nagel, never discussed Aristotle in his classroom or read through the *Posterior Analytics* himself. However, his intellectual context changed when he began to work in New York from 1939 onwards: thanks to the intellectual interest of Ernest Nagel for Aristotle’s *Posterior Analytics* and the subsequent work on explanation of Columbia graduate students like White and Hospers, Hempel began to reflect on questions concerning the structure of scientific explanation, and he molded various elements from this intellectual context into a new whole: explanation as a distinct aim of science and as a logical inference (from Nagel’s discussion of Aristotle), explanation as an answer to ordinary why-questions (from Hospers) and the truth-condition for laws in explanatory arguments (from Stevenson and Hospers). One element of this intellectual context was, however, abandoned by Hempel: the puzzle how to decide whether explanation is a distinct aim of scientific knowledge.

²⁷ In *Structure*, Nagel holds the position that if a theory is empirically adequate for a domain of inquiry, one can tentatively hold the theory to be true (1961/1979, p. 151). This does not imply that truth is a condition for explanatory theories. Although Nagel thinks instrumentalism and realism are “only a conflict over preferred modes of speech”, his refusal to understand truth as a condition for explanations distinguishes him from many contemporary realist-inspired theories of explanation.

In his classroom Nagel discussed the philosophers from the first half of the twentieth century, like Mach, Duhem and Meyerson, who had debated whether explanation was a distinct aim of science. Nagel's students were required to read their texts, and Nagel discussed these texts in his philosophy of science course, even in the 1950s. When Philip Wiener published an anthology to the philosophy of science in 1954, one part of it collected texts from Aristotle, Mach, Duhem, Meyerson and Schlick where they discussed exactly this philosophical puzzle (Wiener, 1953). Hempel never engaged this problem. From 1948 onwards, he took for granted that explanation was the foremost objective of scientific inquiry. He never gave any further reflection to this assumption. All responses to the 1948 paper also take the assumption for granted.

4.4 The Emergence of the Debate on Scientific Explanation

After the publication of Hempel and Oppenheim's paper, it takes a decade before the debate on scientific explanation actually starts. Salmon believed this silence was a sign that Hempel and Oppenheim's analysis of explanation was generally accepted within the philosophy of science community (Salmon, 1989, p. 33). I think that this silence shows the extent to which the topic of explanation was not yet of major significance within philosophy of science as a domain of inquiry during the first decade after the war. Until 1948 the topic had been confined to the philosophers of science in New York and it largely remained confined there for the next ten years.

An investigation of anthologies for philosophy of science of that period, shows that explanation was not yet a settled topic for philosophy of science in the 1950s. In the 1949 anthology *Readings in Philosophical Analysis*, edited by Herbert Feigl and Wilfrid Sellars, "The Function of General Laws in History" is taken up in the section "Problems of Description and Explanation in the Empirical Sciences", which also contained earlier discussions of a regularity account of causation by Schlick (Feigl & Sellars, 1949). In 1953, two anthologies in Philosophy of Science came out, one edited by Feigl and May Brodbeck, and the other was the aforementioned anthology of Wiener. In the Feigl-Brodbeck volume, Hempel & Oppenheim's, 1948 paper was added to the section "The Logic of Scientific Explanation and Theory Construction". The section opened with a passage from Duhem's *The Structure and Aim of Scientific Theories* and contained multiple essays that discussed the aims of theory construction. Hempel and Oppenheim's paper was the only paper directly analyzing the logic of explanation. In the preface Feigl and Brodbeck wrote that their concern had been systematic rather than historical. "Great names have therefore sometimes been sacrificed in the interests of relevance to contemporary issues and a modern idiom that does not in itself present further barriers to an already difficult subject" (Feigl & Brodbeck, 1953, p. v). The addition of the 1948 paper was a good example of this editorial policy to highlight novel and sometimes highly technical developments in the philosophy of science.

Wiener's anthology had an opposite aim. He did not intend to give an overview of the most recent advancements in the technical philosophy of science.

Philosophy of Science has an educational responsibility in the preparation of future engineers, physicians, lawyers, teachers, journalists, ministers, and public administrators as well as research workers with regard to our cultural problem of keeping apace with rapidly advancing sciences. (Wiener, 1953, p. vi)

Wiener decided to focus on basic concepts and problems, “rather than on the defense of any one school of thought”. He emphasized the importance of a historical perspective: “Historical perspectives broaden the scope of logical analyses”. Neither Hempel’s, 1942 nor his 1948 paper made it into Wiener’s anthology. However, by the end of the 1950s Feigl and Brodbeck’s reprinting of the 1948 paper had brought attention to Hempel and Oppenheim’s paper.

In 1957 Israel Scheffler reacted to Hempel and Oppenheim’s basic assumption “that explanation and prediction represent the central purpose of science and are epistemologically basic” (Scheffler, 1957, p. 293). Scheffler aimed to distinguish explanation and prediction from deductive-nomological inferences. To argue for this distinction, the example of an astronomer was introduced, who deduces a configuration of celestial bodies in the past given a current configuration of celestial bodies and a set of laws. Though such deduction fits the logical model of Hempel and Oppenheim, Scheffler believes that it cannot be described as explaining or accounting for the past configuration. Based on Scheffler’s intuition about this case, he required that, in order to have an explanation, the event described in the premises has to happen before the event in the conclusion. Scheffler maintained that explanation and prediction were temporally-laden notions, whereas the deductive-nomological model was temporally neutral. The reason for this distinction lay in the fact that “the interpretation of explanation is generally taken as a reflection of causal notions, and that the peculiar temporal asymmetry of explanation is identical to the temporal asymmetry of cause and effect” (Scheffler, 1957, p. 301). Hempel and Oppenheim’s model did not account for a causal order and thus Scheffler concluded that they had no reason to view science from the vantage point of notions like ‘explanation’: “It would seem a better reflection of the full generality of scientific reasoning if we view it as concerned with comprehensive nomological relations among events and abstract from causal explanation entirely” (Scheffler, 1957, p. 302).

Scheffler introduced a dilemma: either Hempel and Oppenheim’s assumption that science aims to explain the world, is correct, but then their model is broken, or their model is not broken, but then it shows how science aims to form comprehensive nomological relations, not how science explains. The basis of this dilemma lies in Scheffler’s intuitions about explanation which were introduced through the example of the calculating astronomer: inferring past events from current ones cannot be conceived as explaining. Like Aristotle’s theory of the demonstrative syllogism, Scheffler believed that explanations had to be asymmetrical. Sylvain Bromberger’s well-known flag-pole example that was introduced to Hempel at a conference (Bromberger, 1992, p. 8), would also exploit the same intuitive ambiguity: inferring the length of a flag-pole from the length of its shadow, could be considered a valid explanation of the length of the pole given Hempel’s model of explanation—a counter-intuitive result. Scheffler also discussed the truth-condition:

he agreed that explanations should be true and that they should be sound independently from the current state of the evidence. According to Scheffler, this norm does not apply to predictions. Whereas explanations should be true, predictions need not be (Scheffler, 1957, p. 298).

Another early critic was Michael Scriven. He believed that explanations could not simply be equated with the valid answers to why-questions. There were many kinds of questions, like what-questions or how-possibly questions whose answers could also be considered as explanations. Consequently, Scriven introduced an intuition about explanation that stands in opposition to Hempel and Oppenheim's opening statement:

[...] whatever an explanation actually does, in order to be called an explanation at all it must be capable of making clear something not previously clear, that is, of increasing or producing understanding of something. The difference between explaining and 'merely' informing, like the difference between explaining and describing, does not, I shall argue, consist in explaining being something 'more than' or even something intrinsically different from informing or describing, but in its being the appropriate piece of informing or describing, the appropriateness being a matter of its relation to a particular context. (Scriven, 1962, p. 173)

For Scriven, an explanation yields information that makes a phenomenon intelligible to an epistemic agent. As a result of the introduction of this alternative conception of explanations, Scriven was able to argue against the truth-condition for explanations. He gave the following counter-example: we can perfectly talk about two competing explanations for some phenomenon in contemporary physics, since both explanations are capable of offering us understanding, even though only one of them can be true (Scriven, 1962, p. 185). By realigning explanation to human understanding, Scriven reintroduced the relation between explanation and an epistemic agent, which the truth-condition was designed to obstruct. A similar starting point for the analysis of explanation can be found in John Yolton's criticism of Hempel and Oppenheim in 1959 in the *British Journal for the Philosophy of Science*. Yolton emphasized that explanations could not be understood independently from the understanding that they offer to epistemic agents: explanations have to make some phenomena intelligible, even though this intelligibility may require specific training (in physics for instance) (Yolton, 1959, p. 196). "Not deducibility, but intelligibility constitutes the basic feature of the logic of explanation" (Yolton, 1959, p. 207).

Norwood Hanson also argued along these lines in 1959 in the *Philosophical Review*: explanations, like Aristotle's cosmology, aim to make phenomena in the world intelligible, while predictive systems, like Ptolemy's astronomy, aim to predict the position of the heavenly bodies (Hanson, 1959, p. 350). For this distinction, Hanson did not rely on intuitions about what one calls 'explanation'. Instead, Hanson relied on the history of science, from Aristotle to his contemporary age. He argued that predictability and intelligibility had only been perfectly aligned in Newtonian physics, and consequently that Hempel's model only fitted an ideal situation that does not reflect the majority of current scientific theories.

For [Hempel] puts it that we never really have an explanation of a phenomenon P unless we could, simply by imagining a temporal shift, have predicted P by the same explanatory

techniques. For Hempel, therefore, explaining P is simply “predicting” P after it has already happened. But if this is meant to be an account of what is actually done in science (not to mention history), where are we to find examples of Hempel’s thesis in action? The answer is always the same: in Newtonian mechanics. (Hanson, 1959, p. 357)

Hanson’s historical argument was meant to show that Hempel’s equation of explanation to subsumption under a lawful generalization was based on a mistaken ideal of science: many actual scientific theories—Hanson gave the example of quantum mechanics - could explain phenomena, without having predictive capacity.

Hanson, Scheffler, Scriven and Yolton all criticized the introduced conditions of adequacy for scientific explanation that had been introduced by Hempel and Oppenheim. The effect of this criticism was two-fold. First, it focused part of the attention in philosophy of science on Hempel and Oppenheim’s paper, and as a result also on the concept “explanation” and on the examples and intuitions which Hempel and Oppenheim had introduced. “Studies in the Logic of Explanation” became an important point of reference, a paper that exemplified a particular image of science. Because several critics at the end of 1950s identified the 1948 paper as a reference point, the paper became the “received view” on a question that had not been a matter of intellectual concern for philosophers of science before the paper was written. The question what the structure of scientific explanation is, emerged as a standard question in philosophy of science through the debate that resulted from the paper. Second, the early critics quickly expanded the possible examples and intuitions relevant to the analysis of scientific explanation. Scheffler’s paper added the causal counter-example, Scriven’s criticism added the intuition of explanation as intelligibility, and Hanson added the statistics of quantum mechanics. At the beginning of the 1940s it was not important to call specific examples of science “explanations”, but by the end of the 1950s there were a whole collection of examples of “explanations in science” that had to be accounted for in the philosophical reasoning about science. None of the early critics questioned Hempel’s central assumption, that explanation is an aim of science. As a result, this assumption, which originated from Nagel’s coursework on Aristotle, silently shaped all further discussion.

Hempel and Oppenheim’s formal analysis of scientific explanation also received critical attention. The first paper that responded to Hempel and Oppenheim’s formal characterization of explanation was published in *Philosophy of Science* in 1955. It was written by Samuel Gluck, who had recently received his PhD at Columbia University under Ernest Nagel. Gluck indicated in his first note to the paper that Nagel had encouraged him to work on scientific explanation (Gluck, 1955, p. 34). Gluck accepted Hempel and Oppenheim’s general conditions of adequacy for a model of explanation, but attempted to formally characterize explanations that used statistical laws. Through the introduction of statistical laws the non-occurrence of the explanandum became consistent with the explanans. This was formally excluded by the original analysis of 1948. However, Gluck saw no issue: “*Practically speaking*, C&Tb [the conjunction of conditions and statistical laws] really suffice to explain E, an event which has already occurred” (Gluck, 1955, p. 36). Thus, Gluck’s short paper prepares the expansion of Hempel’s formal covering law model with statistical

laws.²⁸ Another six years passed before the next reaction to the formal conditions of the model. In 1961 Rolf Eberle, David Kaplan and Richard Montague proved that the formal conditions of the 1948 paper allowed the sentence ‘the Eiffel Tower is a good conductor of heat’ to be explained by ‘all mermaids are good conductors of heat’. They believed that this example diverged from the “customary notion of explainability” and, thus, served as a counterexample (Eberle et al., 1961, p. 419). Soon after the exposure of this formal problem, it was again taken up by Kaplan and Hempel’s own PhD student, Jaegwon Kim, who offered possible solutions (Kaplan, 1961; Kim, 1963). The early reactions to the formal aspects of the 1948 paper had the same two-fold effect: they referred to the 1948 paper as a pursuit-worthy analysis of science that discussed something which was at stake in philosophy of science, and they expanded or solidified the examples of “explanations in science” that could be accounted for in philosophy.

In 1960 a new set of anthologies for Philosophy of Science appeared. Now, Hempel and Oppenheim’s paper was taken up consistently. In Sidney Morgenbesser and Arthur Danto’s anthology, there was a section on laws and theory which contained an excerpt from Hempel & Oppenheim’s, 1948 paper and Scheffler’s criticism of that paper. In their introduction to the section they note that “the logic of explanation is very much under discussion these days” (Danto & Morgenbesser, 1960, p. 180). Edward Madden’s anthology *Introduction to Philosophy of Science* also contained the first part of the 1948 paper (Madden, 1960, p. 3). At the end of the 1960s “Studies in the Logic of Explanation” had become an undeniable classic. In the 1968 *Oxford Readings in the Philosophy of Science* the editor, Peter Nidditch, judged that “explanation is widely regarded as the primary explicandum for the philosophy of science, while the best known explications offered mold scientific explanations along deductive lines” (Nidditch, 1968, 3). Consequently, a summary of the covering law model written by Hempel in 1962, “Explanation in Science and in History”, was taken up in the readings. Baruch Brody’s anthology for philosophy of science from 1970 also contained Hempel & Oppenheim’s, 1948 paper (Brody, 1970). A count of papers discussing the covering law model in *Philosophy of Science* during the first two decades after its publication similarly shows that the topic of explanation solidified only during the 1960s. Between 1948 and 1958 only four papers discussed Hempel and Oppenheim’s paper, whereas 25 papers got written about explanation and the covering law model between 1959 and 1968. By the end of the 1960s explanation had grown into a central research topic within the early discipline of philosophy of science. Now, a whole generation of philosophers was introduced to philosophy of science partly through a discussion of scientific explanation. What is the covering law model, what does it account for, what are some counterexamples? All these questions had become standard.

²⁸ Hempel already explored these ideas in (Hempel and Oppenheim 1948, p. 141).

4.5 Conclusion

Scientific explanation has existed as a topic for the systematic reflection on science ever since Aristotle's *Posterior Analytics*. Aristotle identified the search for explanations as the main aim of scientific knowledge. When Ernest Nagel began teaching contemporary logic at Columbia University in the 1930s, this Aristotelian gloss on the aim of science was part of his course. Nagel did not want to reduce logic to the formal study of inferences, and included an investigation of the formal structure of science into his logics course. The *Posterior Analytics* was Nagel's preferred paradigm to introduce his students to a broader understanding of logic. Unlike German speaking logical empiricists, Nagel used the history of philosophy as a valid source for insight into the nature of science. For Nagel, a study of logic also implied a critical reflection on the source and validity of the norms proposed for knowledge by logic. This critical reflection required a historical insight into the reason why intellectuals in the past had defended certain norms for knowledge and why some older norms were subsequently abandoned. Because of Nagel's interest in Aristotle from this historico-critical point of view, the intellectual problem how to analyze the structure of scientific explanation became part of the philosophical context of New York during the Second World War.

Although Hempel has often been credited for bringing explanation at the center stage of research in post-war philosophy of science, it was Nagel's historico-critical approach to logic and philosophy of science that brought the topic to the attention of Hempel and, indirectly, to the discipline at large. This genealogical narrative about the re-emergence of scientific explanation shows that a historical approach to the philosophy of science is an enrichment to the discipline. The concepts and methods that philosophers of science use to approach science, or that scientists use to understand their own reasoning, are the result of intellectual decisions from the past. Consequently, a critical reflection of scientific reasoning should scrutinize these historical decisions. Nagel's Logic course and its important effect on philosophy of science as a field is a good example of this.

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