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A Genealogy of Scientific Explanation

*The Emergence of the Deductive-Nomological
Model at the Intersection of German Historical and
Scientific Philosophy*

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Preface



This dissertation was mainly written on a desk that occupies the space of the old philosophy library of Ghent University. The desk offers an excellent view over the central university library, the Book Tower and its magnificent courtyard. Unfortunately, throughout the process of writing the dissertation, the entire library complex was in the process of renovation. I never got to enjoy the sight of Belgium's most famous modernist building, since it was constantly covered by scaffolding.

The idea of storing books in a tower is unconventional. The weight of the books can easily destabilize the construction. Controlling temperature and humidity inside a tower is a difficult operation, and getting the books into and out of storage requires a reliable mechanical system. Henry van de Velde, the designer of the book tower, believed that all these problems could be solved by modern techniques. The stability of concrete and the power of elevators would overcome the concerns of librarians. By 1936, when construction of the tower finally started, van de Velde had transformed himself from a leading art nouveau designer to the spearhead of modernist architecture. Lines, squares, crosses and semi-circles make up the exterior design of the tower. Inside, an intricate play of rectangles constantly recurs within the design of the reading room and its furniture. Van de Velde had conceived the library as a modernist temple. Every ornamental shape was replaced by strict forms. Unfortunately, what was supposed to be his masterpiece, became a compromise between his modernist ideals and the practical circumstances of the construction. Because of the German occupation, the building was never properly finished. When the student numbers exploded in the 1960s, the last piece of van de Velde's original plan was even replaced by a pragmatic building to house the ever expanding faculty of arts and philosophy. This building, known as the Blandijn, has been the main locus of my philosophical training for the past nine years. Ever since its conception, it has been a symbol of the triumph of pragmatics over the sincere beauty of modern rational design.

By the end of the 20th century, the original rationale behind the book tower had already lost most of its appeal. Computers replaced the catalogue room that was originally conceived to house thousands of cards to request books. Large white scanners which heavily contrasted with the black and red marble of the reading room, were bolted to the floor. The elevators over time turned out to be unreliable mechanisms. Students had stopped using the library to consult manuscripts and books, but were instead occupying it with laptops and tablets. The innovative concrete of van de Velde's tower was rotting from the inside. The modernist dream was collapsing. There was only one solution: closing the entire complex, reconceiving its purpose, destroying those parts of the concrete that were falling apart and recasting it. This process was extremely laborious: every piece of the old concrete was minutely sandblasted to separate healthy from ill pieces. Only afterwards could the recasting of the modernist ideal begin. But, would the reconstruction be able to capture the original intent?

My dissertation was written with the agonizing sound of sandblasting in the background. Similar to the renovation of the Book Tower, the dissertation aims to partly destroy and

renovate another highlight of modernist culture: scientific philosophy as it was represented by logical empiricist philosophers. This philosophical project suffered a fate similar to that of the tower: Nazism prevented the broader implementation of its design and after the Second World War it lost its modernist appeals within the culture of the ever expanding post-war universities. I have no illusion that a renovation could ever result in the execution of the original ideals. Neither the tower nor scientific philosophy can ever be remade to achieve the purposes of their designers. In the present there is only the choice of which cultural constellations are valuable enough to be recast in a new struggle that will come to define what the role of the university and philosophy will be in our future society. I think scientific philosophy in all its various forms has this value, exactly because it essentially struggles with the central question how philosophy as a discourse can disrupt its own boundaries and connect with other forms of life outside and inside the university. After four years of professional philosophy, I believe this is the crucial struggle that philosophers are incessantly, though most of the time implicitly, engaged in. Two currently available avenues of approach to this struggle have, in my view, been entirely exhausted. First, there is the traditional approach of being a public philosopher, by publishing opinion pieces and editorials in newspapers or appearing in talk shows. In these roles the philosopher can only be understood as spokesman of a position that was already present before the intervention and that the philosopher was invited to represent. By taking up this role, the philosopher becomes a structural character in the story that the media replay every day. Second, there is the idea of innovating philosophical discourse by creating novel institutional spaces for a variety of interdisciplinary discourses. Though the creation of new institutional spaces creates novel outlets and possibly new kinds of discourses, it also continues to operate within the institutional status quo of scientists or philosophers who create work for the available (trans-, inter-, multi-) disciplinary outlets. In the 21st century, philosophers should look for different avenues of approach to the struggle of performing philosophy in continuity with life outside and inside the university. The history of scientific philosophy in the 20th century offers interesting perspectives on the difficulties inherent in such a struggle.

Although this dissertation thematises this struggle, it is the result of academic, self-centred philosophy and is written for an audience of professional philosophers. This was a conscious choice. Initially, I believed that a new perspective on the history of one of the central concepts in philosophy of science could create a new consciousness of what philosophers of science were in fact doing and how they might want to change it. During the process of writing, I gradually abandoned this juvenile hope. Now, I think the dissertation shows that an individual actor in a field does not possess the power to determine the direction of that field, even though the sum of the actions of all the actors does indeed set the direction. How we come to change ourselves and our environments is not transparent to us. Through writing the dissertation, its original purpose was destroyed and recasted. During my four years at my desk I have learned that reconstruction never ends. It is pointless

to conceive the Book Tower as a temple of modernism that can be preserved or definitively remade, just as it is pointless to conceive scientific philosophy as a philosophical project that can be revived and then executed. One can only partake in a process of reconstruction whose ends cannot be determined beforehand, but only emerge after the process has occurred. It is my hope that this dissertation can play its role in that process.

List of Abbreviations

References to archives sources are given in a footnote, and they have the following form: description of document, date (if possible), Box, folder, number of document (if possible), abbreviation of the archive. The consulted archives have been given the following abbreviation:

ASP = Archives of Scientific Philosophy, Special Collections, Hillman Library, University of Pittsburgh, Pittsburgh. Quoted by permission of the University of Pittsburgh. All rights reserved.

HR = Hans Reichenbach Papers

CH = Carl Hempel Papers

RC = Rudolf Carnap Papers

VCA = Vienna Circle Archives, Noord-Hollands Archief, Haarlem, The Netherlands. Quoted by permission of the Wiener Kreis Stichting, Amsterdam. All rights reserved.

POKP = Paul Oskar Kristeller Papers, Rare Book and Manuscript Library, Columbia University, New York. Quoted by permission of Columbia University. All rights reserved.

JHRP = John Herman Randall Papers, Rare Book and Manuscript Library, Columbia University, New York. Quoted by permission of Columbia University. All rights reserved.

ENP = Ernst Nagel Papers, Rare Book and Manuscript Library, Columbia University, New York. Quoted by permission of Columbia University. All rights reserved

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Introduction

Historical Philosophy

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.¹

In his 1911 critical review of Cassirer's *Das Erkenntnisproblem* Emile Meyerson summarizes the intellectual problem that runs through this dissertation: logic is not

¹ Il y a là une observation assez générale et que nous voudrions bien mettre en lumière, parce que la difficulté dont nous parlons, particulière à la philosophie des sciences, a, croyons-nous, troublé le regard de plus d'un chercheur. Nul homme ne se voit pensant - cela est, semble-t-il, incontestable; s'il n'en était pas ainsi, la logique ne serait pas une science qu'il nous faudrait acquérir, car nous saurions immédiatement et de science certaine comment nous sommes arrivés à nos conclusions. Nous l'ignorons, et ne pouvons parvenir à cette connaissance que peu à peu et par des voies fort détournées. Alors, des règles qui n'étaient que subconscientes deviennent conscientes. Mais nous pouvons nous tromper, attribuer à notre raison une règle fort différente de celle qu'elle a réellement suivie : le fait qu'on dispute sur la logique en est une preuve manifeste. - Le savant n'est pas, en cela, différent d'un autre homme. Pas plus que n'importe qui, il ne se perçoit pensant car, comme tout le monde, il suit des règles qui ne parviennent pas directement à sa conscience. (Meyerson 1911, 125–26)

transparent to its user.² Consequently, the philosopher of science, who aims to make transparent the valid norms of scientific reasoning, is faced with a serious problem: she cannot simply abstract the norms from the rules that scientists themselves believe they are using. Methodological reflections from scientists are not to be trusted, and neither are singular instances of a reasoning processes that one finds in scientific works, because such instances can always be the result of mistakes. Meyerson believes that the mark of a great scientist is not consistency with his own rules of thinking, but a certain “potent scientific instinct, a kind of prophecy that allows him to skip the various steps of an argument” (Meyerson 1911, 126). Thus, e.g., when Cassirer quotes Newton’s methodological reflections on his own reasoning, these do not necessarily reflect the reasoning that validates Newton’s conclusions.

Faced with this problem, the logician of science can attempt to produce norms that stand independently of actual scientific reasoning and scientific methodologies, but then she faces the extra challenge of arguing that these norms, though independent of actual scientific practice, are still relevant for our understanding of that practice. Or, she can attempt to discern norms through an analysis of historical tendencies within science. Meyerson agrees with Cassirer that the latter option is to be preferred: only in hindsight, from a historical perspective, can one discern why certain conclusions were justified. For Meyerson, discerning the logic of science is necessarily interlinked with a historical perspective on science. Cassirer’s book embodied this approach to the logic of science according to Meyerson: “it is at root a systematic book, a work dominated by a theory that the author seeks to support through a study of philosophical and scientific evolution in the modern period” (Meyerson 1911, 100).³ Although at several places in the review Meyerson lauds Cassirer’s immense work, he does not agree with its main thesis, that the concept of rule or mathematical function should necessarily replace the concept of essence or substance (Meyerson 1911, 121). Wherever Cassirer has marked a distinction between methodological ideas and scientific practice in Boyle, Newton or Descartes, he has, in Meyerson’s opinion, drawn the wrong conclusions. On Meyerson’s view, Boyle, Newton or Descartes, against their own methodological views, kept on looking for a substantial or

² I start my discussion from Meyerson’s review of Cassirer, because the review discusses, in an exemplary way, a central philosophical problem that runs throughout this dissertation, namely how to understand the relation between philosophy and history. Meyerson’s review openly reflects on the methodological question how to philosophically use history for philosophy, which is exactly the methodological question that I need to answer myself, before I can begin with my own historical work. Just as Meyerson, in his review, attempts to understand how Cassirer uses history, I need to uncover how I have used history in this dissertation. In the review, the same subject as in my dissertation is also at stake, namely the validity of scientific explanation as a meta-scientific concept.

³ C’est au fond un livre systématique, une oeuvre dominée par une théorie que l’auteur cherche précisément à étayer par l’étude de l’évolution philosophique et scientifique des temps modernes.

essential explanation (Meyerson 1911, 124). A positivist interpretation of scientific knowledge that would discard all investigations into substances, is according to Meyerson “entirely contrary to the actual tendency of the intellect, both for the individual thinking being as for the entire evolution of science” (Meyerson 1911, 128).⁴ The tendency that Cassirer claims to have uncovered in the ideas on science of the modern period was not the correct one. Since science should investigate, and has investigated, reality, Meyerson believes that science and metaphysics tend toward the same goal, to uncover the substance of the world.

If the logic of science is not transparent and if one requires a historical account of the tendency that has driven scientific thought in order to uncover its logic, then how should one proceed? How should an author proceed to choose and collate his historical texts? Meyerson seems to believe that the truth of the matter must arise of itself. A good reading of Boyle, Newton and Descartes will show how their concerns for the substance of reality, and not their own mangled methodological reflections, drove their inquiries to the truth: the unity of the logic of science will show itself through the history of actual scientific progress. In that sense, history of science, unlike logic of science, is transparent. Cassirer most likely agreed with Meyerson’s initial observation about the non-transparency of logic, but disagreed about the possibility that history can be made transparent through a complete historical investigation. In the introduction to *Das Erkenntnisproblem* Cassirer explicitly denies that the history of science can be unambiguously interpreted as a unity:

The concept of history of science already contains within itself every idea of the conservation of a general logical structure that returns in every succession of individual conceptual systems. Indeed, if the previous content of thinking were not linked with the content of its predecessor through some sort of identity, then we would have nothing to justify our combination of the scattered logical fragments that we have before us, into a sequence of events. Every historical sequence of development needs a ‘subject’, that forms its basis, represents itself in this sequence and is manifested by it. The mistake of the metaphysical philosophy of history does not lie in the fact that it demands such a subject, but in the fact it makes the subject into a thing, in the sense that it speaks of the self-development of an idea or the advancement of ‘world spirit’, or something like that. We must renounce any comparable factual bearer that grounds a historical movement; we must transform the metaphysical formula into a methodical one. Instead of a common substrate we only look for and postulate an ideal continuity in the singular phases of what happens; we only need

⁴ "mais parce qu'elle est entièrement contraire à la marche réelle de l'intellect, aussi bien chez le penseur individuel que dans l'ensemble de l'évolution de la science."

this ideal continuity to talk about a unity of the historical process.⁵ (Cassirer [1906] 2009, 13)

History is not transparent: it has no given, unitary subject, it cannot offer *the* logic of science that develops itself through various stages. Instead, the historical philosopher must look for a continuity within the individual occurrences of history. Consequently, Cassirer is not interested in finding out what *the* logic of science actually is. Rather, he intends to investigate what the new concept of knowledge is – a concept that was not uncovered, but created throughout the modern period by ideas working upon each other (Cassirer [1906] 2009, IX).

In so far as we conceive of the conditions for science as something that came about, we also acknowledge these conditions as creations of thought. And in so far as we understand these conditions in their historical relativity and in their historical conditionality, we also immediately create for ourselves a perspective on the unstoppable progress for the conditions of science and their ever renewing productivity.⁶ (Cassirer [1906] 2009, X)

Thus, Cassirer intends to show how specific conditions for scientific knowledge came about within a specific period of time, and by bringing to light this historical formation of the conditions for science Cassirer believes he can create a new perspective on what it means to produce scientific knowledge. Writing the history of the conditions for knowledge, is at the same time also a reflection on the limits of and possibilities for new ways of producing knowledge. Contrary to what Meyerson demands, the logic of science cannot be made transparent through history as a steady guiding light in all scientific inquiry. For Cassirer, the logic that guides science is exactly this continuous intellectual question/problem: what is science?

⁵ Der Begriff der Wissenschaftsgeschichte selbst birgt in sich bereits jenen Gedanken der Erhaltung einer allgemeinen logischen Struktur in aller Aufeinanderfolge besonderer Begriffssysteme. In der Tat: Wäre der frühere Inhalt des Denkens mit dem vorangehenden nicht durch irgendeine Identität verknüpft, so gäbe es nichts, was uns berechnete, die verstreuten logischen Bruchstücke, die wir alsdann vor uns hätten, zu einer Reihe des Geschehens zusammenzufassen. Jede historische Entwicklungsreihe bedarf eines »Subjekts«, das ihr zugrunde liegt und sich in ihr darstellt und äußert. Der Fehler der metaphysischen Geschichtsphilosophie liegt nicht darin, daß sie überhaupt ein solches Subjekt fordert, sondern darin, daß sie es verdinglicht, indem sie von einer Selbstentwicklung der »Idee«, einem Fortschritt des »Weltgeistes« und dergleichen spricht. Auf jeden derartigen sachlichen Träger, der hinter der geschichtlichen Bewegung stünde, müssen wir verzichten; die metaphysische Formel muß sich uns in eine methodische wandeln. Statt eines gemeinsamen Substrats suchen und fordern wir nur die gedankliche Kontinuität in den Einzelphasen des Geschehens; sie allein ist es, die wir brauchen, um von der Einheit des Prozesses zu sprechen.

⁶ Indem wir die Voraussetzungen der Wissenschaft als geworden betrachten, erkennen wir sie ebendamit wiederum als Schöpfungen des Denkens an; indem wir ihre historische Relativität und Bedingtheit durchschauen, eröffnen wir uns damit den Ausblick in ihren unaufhaltsamen Fortgang und ihre immer erneute Produktivität.

What is Cassirer's historical philosophy, given this background? It is not a detailed catalogue of the influences of ideas or thinkers on each other. Meyerson correctly noted that Cassirer's work is not an attempt at such a catalogue, given the fact that the various themes are, to Meyerson's surprise, not even discussed in their proper chronological order (Meyerson 1911, 102). Contrary to Meyerson's expectations, the work is also not a search for *the* logic of science as what drives all historical developments in the sciences. Instead, it is more like an engaged stance towards the intellectual problem what science is and consequently towards the contemporary limits and possibilities of science.

Fifty-five years after Meyerson, another French philosopher, Michel Foucault, also reviewed the work of Cassirer, on the occasion of the first French translation of *Die Philosophie der Aufklärung* (1932). Unlike Meyerson, Foucault had nothing but praise for the translated work and for Cassirer's oeuvre in general. He considered it a sign of the defensive French literary culture that it was not until thirty years after its original publication that Cassirer's great work was now translated. Foucault was especially interested in this work because it inaugurated a revolutionary historical-philosophical method:

Cassirer's work belongs to Enlightenment philosophy. More than anyone, Cassirer has found a way to make manifest the meaning of a return to the 18th century. Above all, Cassirer owes this to a method of analysis whose model has not yet lost its value for us.⁷ (Foucault [1966] 1966, 575)

Foucault contrasted Cassirer's method with the accepted method for the history of ideas in France. According to Foucault, a traditional French historian of ideas discusses the culture of a specific age, which is characterized by certain opinions, knowledge interests, desires or aspirations. Cassirer's method does something fundamentally different: on the one hand it brackets off the individual motivations of historical actors, the accidental biographical details and all kinds of irrelevant figures that populate an era, but on the other hand it also leaves the social and economic determinations aside. What remains to be uncovered through Cassirer's method, is an "inseparable field of discourses and ideas, concepts and words, expressions and affirmations that [Cassirer] intends to analyse in their own configuration" (Foucault [1966] 1966, 575).⁸ By removing histories of individuals and societies, Cassirer uncovers an autonomous space of theory [*espace autonome du <théorique>*]. According to Foucault, this was an entirely novel way of doing the history of ideas: "[Cassirer] discovers

⁷ Cassirer est du côté des « Lumières » et, mieux que personne, il a su rendre manifeste le sens du retour au xviii^e siècle. Grâce, avant tout, à une méthode d'analyse dont le modèle, pour nous, n'a pas encore perdu sa valeur.

⁸ Et ce qui se déploie alors devant lui, c'est toute une nappe indissociable de discours et de pensée, de concepts et de mots, d'énoncés et d'affirmations qu'il entreprend d'analyser dans sa configuration propre.

a history which had up until that time remained mute” (Foucault [1966] 1966, 576).⁹ This new history of ideas is identified as a specific transcendental project that aligns Cassirer’s entire oeuvre:

The *Problem of Knowledge* and the *Philosophy of Symbolic Forms* show exactly that thinking and discourse, or rather their inseparable unity far from offering the pure and simple expression of what we know, constitutes the place from which all knowledge becomes possible.¹⁰ (Foucault [1966] 1966, 576)

Cassirer’s historical analysis thus uncovers the specific configuration of ideas and words in a given era that make specific ways of knowing possible.¹¹ Foucault had himself attempted to give such a history in *Les Mots et les choses* which was published in the same year as the review of Cassirer. Consequently, it is not surprising that Foucault believes it is indispensable to familiarize oneself with Cassirer’s new type of history of ideas: it is, at the same time, an advertisement for his own project. He ends his review with the following imperative: “it is from [Cassirer’s method] that we must now start our work” (Foucault [1966] 1966, 577).¹²

For the remainder of his philosophical career, Foucault was mostly silent about the possible Neokantian origins of his own historical-philosophical project. In the last year of his life, however, Foucault wrote an encyclopedia article about himself under the pseudonym Maurice Florent. There, he described himself as a Kantian, much along the lines of his Cassirer-review.

⁹ « Sous ses yeux [il] se découvre une histoire jusque-là restée muette ». This is also the title of Foucault’s review « une histoire restée muette ». The title has a double meaning. First, it signifies the fact that Cassirer’s history of Enlightenment had remained unknown in France. Second, it refers to the fact that Cassirer had uncovered a new history of the Enlightenment through his method. Possibly, the title also refers to Kant’s text *Was ist Aufklärung?*, which opens with a plea to step out of a status of *immaturity* [Unmündigkeit], which can be translated as *muette* in French. Then, the writing of Cassirer’s history could be a move away from the status of immaturity, which is also Foucault’s interpretation of Cassirer’s method. Much like Meyerson before him, Foucault crafted his review of Cassirer with great care.

¹⁰ *Problème de la connaissance et Philosophie des formes symboliques* montrent justement que la pensée et le discours, ou plutôt leur indissociable unité, loin d’offrir la pure et simple manifestation de ce que nous savons, constituent le lieu d’où peut naître toute connaissance.

¹¹ That ideas and words form an inseparable discursive unity, also in Cassirer’s own work, can be seen from Cassirer’s discussion in *The Tragedy of Culture* [*Die Tragödie der Kultur*]. There, Cassirer extensively argues that every material symbol of culture in words or images makes the identification of objects possible, which, at the same time, also enables the creation of new cultural symbols that destroy the previous ones (Cassirer [1942] 2011, 123–24). The material representation of ideas in words thus creates a dialectic or a tragedy: culture is an activity that must always begin anew and that is never sure of its goal (Cassirer [1942] 2011, 113).

¹² Il était indispensable de la faire connaître, car c’est de là maintenant que, nous autres, nous devons partir.

If Foucault inscribes himself in the philosophical tradition, it is within the critical tradition of Kant, and one could call his project a historical critique of thinking.

If by thinking one understands the act that poses a subject and an object in their diverse, possible relationships, then a historical critique of thinking would be an analysis of the conditions in which certain subject-object relationships are formed or modified – these relationships would then be constitutive of possible ways of knowing.¹³ (Foucault 2001a, 1450)

This description of a historical, Kantian project in which Foucault situates himself fits Cassirer's philosophy perfectly. In the often neglected fifth study of *Zur Logik der Kulturwissenschaften*, called the *The Tragedy of Culture* [*Die Tragödie der Kultur*], Cassirer identifies his own project as a historical critique of all forms of thinking, including forms of art, literature, religion and science. Summarily, Cassirer describes his project as an investigation of the plasticity of possible subject-object relationships through time. A thinking mind can only express itself through discursive names and images. These discursive products enable the mind to think itself, and its relation to the world, but they also have a tendency to become reified when they are considered as absolutes (Cassirer [1942] 2011, 115–16). If this happens, the thinking mind requires a “renaissance”, a process in which reified cultural products are broken up and new possible configurations of the subject-object relationship are conceived. This process of cultural renaissance cannot be exhausted [unerschöpflich] (Cassirer [1942] 2011, 117). This process is the constant *Tragedy of Culture* in which cultural forms are destroyed and then reborn. “Culture is filled with the innermost violent tensions” (Cassirer [1942] 2011, 113).¹⁴ This theme of constant renaissance had already been the background of Cassirer's project in *Das Erkenntnisproblem*:

Every era has its own basic system of general concepts and presuppositions with which the era controls and collates the manifold of material which its experience and observation offers. The naive view and even the scientific approach, if it is not accompanied by a critical self-reflection, consider these products of the mind as rigid structures that are finished once and for all. The instruments of thought are then transformed into existing objects; the free postulates of the understanding are then

¹³ Si Foucault s'inscrit bien dans la tradition philosophique, c'est dans la tradition critique qui est celle de Kant et l'on pourrait nommer son entreprise Histoire critique de la pensée.

Si par pensée on entend l'acte qui pose, dans leurs diverses relations possibles, un sujet et un objet, une histoire critique de la pensée serait une analyse des conditions dans lesquelles sont formées ou modifiées certaines relations de sujet à objet, dans la mesure où celles-ci sont constitutives d'un savoir possible.

¹⁴ Die Kultur ist erfüllt von den stärksten inneren Gegensätzen.

conceived in the order of things that surround us and that we only have to passively accept.¹⁵ (Cassirer [1906] 2009, IX)

No creation of the mind can be eternal. That is why Cassirer believes that critical self-reflection is necessary: to avoid the illusion that certain postulates of the mind are set in stone and not open to change, while they are in fact constantly open to renewal.¹⁶ Cassirer did not regard critical self-reflection as an attitude of the mind towards itself that could be taken for granted. He considered it as a highly specific attitude towards thinking which had to come about in a specific era as a form of thinking itself. Cassirer thematized the coming-of-age of the attitude of critical self-reflection in the work that Foucault had discussed in his review, *Philosophie der Aufklärung*. Cassirer did not intend that work as “an epic of the course, development and fate of enlightenment philosophy” (Cassirer [1932] 2009, IX). The work was more intended to make visible the inner movement that completes itself during this period, and to show “the dramatic action of thought during the period of Enlightenment” (Cassirer [1932] 2009, IX). This dramatic action of thought is a specific understanding of what philosophy can and should do as a discourse that came about during the period of the Enlightenment. It excludes from philosophy the search for eternal truth or steady norms: philosophy is no longer conceived as a separate discourse of knowledge, but as an “all compassing medium in which natural knowledge and knowledge of law, or state, etc. construct, develop and ground themselves” (Cassirer [1932] 2009, XI).¹⁷ Thus, philosophy’s reversal from the search for truth to an attitude of critical reflection on thinking is the dramatic action that renews what it means to think during the period of the Enlightenment. For Cassirer, Kant’s text *Was ist Aufklärung?* epitomizes the dramatic innovation in thinking during the Enlightenment: the text summarizes what it means to have a critical perspective, but at the same time this means that its own Leitmotif “Sapere Aude – have the courage to use your own understanding” should also be applied to Kant’s text itself:

¹⁵ Jede Epoche besitzt ein Grundsystem letzter allgemeiner Begriffe und Voraussetzungen, kraft deren sie die Mannigfaltigkeit des Stoffes, den ihr Erfahrung und Beobachtung bieten, meistert und zur Einheit zusammenfügt. Der naiven Auffassung aber und selbst der wissenschaftlichen Betrachtung, soweit sie nicht durch kritische Selbstbesinnung geleitet ist, erscheinen diese Erzeugnisse des Geistes selbst als starre und ein für allemal fertige Gebilde. Die Instrumente des Denkens werden zu bestehenden Objekten umgewandelt; die freien Setzungen des Verstandes werden in der Art von Dingen angeschaut, die uns umgeben und die wir passiv hinzunehmen haben.

¹⁶ Reichenbach’s idea that the logic of science has the task to show which postulates of science are conventional, comes close to this conception of what a logic of science should do. Unlike Cassirer, Reichenbach does not believe that a historical perspective is necessary to perform this task. See 2.3.2. For an interesting transcendental perspective on Reichenbach, also see (Richardson 2006).

¹⁷ Die Philosophie bedeutet, gemäß dieser Grundanschauung, kein Sondergebiet von Erkenntnissen, die neben oder über den Sätzen der Naturerkenntnis, der Erkenntnis von Recht und Staat usf. stehen, sondern sie ist das allumfassende Medium, in dem diese sich bilden, sich entwickeln und sich begründen.

The slogan: *Sapere Aude*, which Kant called the “motto of the Enlightenment,” also holds for our own historical relation to that period. Instead of abusing that period or elegantly looking down on it, we must find the courage to measure ourselves against it, and internally confront ourselves with it.¹⁸ (Cassirer [1932] 2009, XVI)

Foucault understood this as the key to any reading of Cassirer’s view on Enlightenment. As Foucault pointed out in his review, Cassirer perfectly executed his program. The book is not only a discussion of the subject “Enlightenment philosophy as the coming-of-age of a critical attitude”; the subject is also Cassirer’s method of analysis. By showing how to perform a historical critique of Enlightenment thinking Cassirer renewed what it means to write a critical reflection on thinking, thereby applying Kant’s leitmotif to the Enlightenment itself. For Foucault, this new historical method of Cassirer’s was an important new form of critical philosophy. This becomes especially evident if one reads Foucault’s own text on Kant’s *Was ist Aufklärung?*

The hypothesis that I would like to advance is that this little text finds itself at some kind of critical hinge of reflection on history. It is Kant’s reflection on the actuality of his own enterprise. [...] To me, the novelty of this text is its reflection on ‘today’ as difference in history and as a motive for a particular philosophical task.¹⁹ (Foucault 2001b, 1387)

Reflecting upon one’s own ideas, or forms of thinking, implies separating them from what came before: no longer can they be justified by a previous tradition, e.g. through what a priest has said, or what a book has said. The particular philosophical *task* is the introduction of a historical distinction between what one is currently thinking and how this thinking came about through all sort of symbolizations from the past. Unsurprisingly, this is also Cassirer’s specific reading of Kant’s text: philosophy is discussed as a critical task of thought towards itself, whose result shows how certain forms of thinking from the past are not eternal, and can be changed. Foucault remains on the side of Cassirer throughout his entire discussion of Kant’s text, since he also summarizes the critical attitude as a specific historical-philosophical project:

One should certainly not conceive of the critical ontology of ourselves as a theory, or a doctrine, nor even as a permanent body of accumulating knowledge. One should

¹⁸ Das Wort: »Sapere aude!«, das Kant den »Wahlspruch der Aufklärung« genannt hat, gilt auch für unser eigenes historisches Verhältnis zu ihr. Wir müssen, statt sie zu schmähen oder vornehm auf sie herabzublicken, wieder den Mut finden, uns mit ihr zu messen und uns innerlich mit ihr auseinanderzusetzen.

¹⁹ L’hypothèse que je voudrais avancer, c’est que ce petit texte se trouve en quelque sort à la charnière critique et de la réflexion sur l’histoire. C’est une réflexion de Kant sur l’actualité de son entreprise. ... La réflexion sur « aujourd’hui » comme différence dans l’histoire et comme motif pour une tâche philosophique particulière me paraît être la nouveauté de ce texte.

conceive it as an attitude, an ethos, a philosophical life in which the critique of who we are is simultaneously both a historical analysis of the limits with which we are confronted, and an experiment to the possibility of breaching them.²⁰ (Foucault 2001b, 1396)

What is Cassirer's historical philosophy? When I earlier characterized it as an engaged stance towards the intellectual problem of what science is, and consequently towards the contemporary limits and possibilities of science, I was summarizing the historical-philosophical project that I have just presented as Foucault's reading of Cassirer. It is also the project that guided me in the conception of this dissertation, which should be understood as a historical critique of contemporary philosophy of science. I show how a specific configuration in the conception of science by philosophers came into being, in particular how philosophers of science started thinking about explanation as an undeniable aspect of scientific inquiry, and how this new subject of 'scientific explanation' was consequently used by philosophers to align all kinds of ideas and examples of 'science'. 'Scientific explanation' has come to partly constitute both what a philosopher identified in the sciences and what a philosopher wanted to analyze about the sciences.

As with any new configuration of thinking that comes about in a given era, scientific explanation as a form of reasoning in philosophy of science was extremely productive: it constituted a field of knowledge. All kinds of examples and models of science that were previously unconnected could now be aligned through the concept of 'scientific explanation'. This also enabled anglophone philosophy of science to become a discourse on its own, separated from other anglophone fields of philosophy. In this new autonomous field, 'scientific explanation' was something *at stake* for philosophers of science: any actor wanting to be part of this field had to know what had already been said about it, and had to have a view on it. Throughout this dissertation I will use 'stake' to denote an intellectual challenge that is *at stake* in a discursive field. I do not use the more standard terminology of 'problem', because 'problem' already suggests a specific content, whereas how to give content to a stake, is exactly part of what is at stake, e.g. one can discuss the relation between the historical and the natural sciences in a variety of ways: through a discussion of laws, concepts, evidence, etc. Stakes are inherently open: actors constantly struggle over them, intentionally or unintentionally – throughout the next chapters I will show such struggles over stakes surrounding historiography.

Scientific explanation as a stake aligned philosophical reasoning about science for anglophone philosophy of science after the Second World War. Such alignment is not only productive; it also poses the risk that the conception of 'science as explanation' becomes

²⁰ L'ontologie critique de nous-mêmes, il faut la considérer non certes comme une théorie, une doctrine, ni même un corps permanent, de savoir qui s'accumule ; il faut la concevoir comme une attitude, un ethos, une vie philosophique où la critique de ce que nous sommes est à la fois analyse historique des limites qui nous sont posées et épreuve de leur franchissement possible.

rigified or reified [als eine starre Gebilde], and that it obstructs new ways of conceiving science. Today, it has become difficult to conceive a philosophy of science without it, even though, as I show below, there used to be a philosophical discourse on science where scientific explanation did not play the same fundamental role as it does now. ‘Scientific explanation’ is now conceived “in the category of things that surround us and that we are to passively accept” (Cassirer [1906] 2009, IX).²¹ The writing of my history can be conceived as a counter-measure, or a stance of critical self-awareness [kritische Selbstbesinning]. As Cassirer put it, “The moment of form-constancy and the moment of the ‘modifiability’ of the form meets us everywhere” (Cassirer [1942] 2011, 126), including the highly specialized discourse of contemporary philosophy of science.

In what follows, Chapter 1 sets up the specific problem and the era that my historical-philosophical project intends to tackle. Chapter 2 contains a description of an era in philosophy of science in which scientific explanation played no role, whereas chapter 4 shows how the subject became constitutive for philosophical reasoning about science and thus formed a new era for philosophy of science. Chapter 3 discusses the transition from the first situation to the second. Before I continue with the actual dissertation, I want to clarify its title. Genealogy might be understood as a historical narrative that explains how the topic of scientific explanation was introduced in the philosophy of science. I believe this is an incorrect reading of what a genealogy, and also what a stance of critical self-awareness, should do. In order to make this point, I would like to use a distinction of Foucault’s.

In *Nietzsche, la généalogie, l’histoire* (1971) Foucault distinguishes between two separate genealogical enterprises. On the one hand, one can perform a genealogy as the search for origin [Ursprung]. If one investigates the origin of a certain concept, one looks back in time to find a concept “that was always already there”, that has an essential identity. One shows how, despite all the adventures and setbacks, a concept was eventually discovered and brought to light in the space of reasoning (Foucault [1971] 2001, 1006). The point of origin is also the point of the truth: the introduction of the concept is also the introduction of a truth that can no longer be denied (Foucault [1971] 2001, 1007). A good example of this type of genealogy would be Meyerson’s idea that the history of science, once made transparent, is capable of showing the one logic of scientific inquiry that revolves around explanations. As we will see in the next chapter, Wesley Salmon’s history of scientific explanation functions in the same way: it narrates where scientific explanation was uncovered as a topic in philosophy of science, and how this new insight, this truth about science, could no longer be ignored, and was consequently accepted by all other

²¹ Die freien Setzungen des Verstandes werden in der Art von Dingen angeschaut, die uns umgeben und die wir passiv hinzunehmen haben.

philosophers of science. This search for origin is, however, not genealogy proper, according to Foucault:

To do a genealogy [...] of knowledge will never imply to launch a quest to find its 'origin', disregarding all episodes of history as inaccessible.²² (Foucault [1971] 2001, 1008)

The quest for an origin neglects all the errors, coincidences or mischievous plans that came together to give rise to the so-called truth. Foucault identifies a genealogy more with a search for *Entstehung* (coming-into-being). This is the point of emergence, a singular principle of apparition. Genealogy, as an analysis of *Entstehung*, shows how a game or a struggle with different parties can give rise to a new constellation (Foucault [1971] 2001, 1011). No one figure, tendency, intention or force *can be held responsible* for an emergence, according to Foucault. A genealogy shows how any configuration is the contingent result of a constant struggle, in my case the constant struggle over what it means to do philosophy of science. "One should pick out the accidents, tiny deviations – or, the complete turnarounds –, the errors, the mistakes in assessment, the miscalculations that have all given rise to what exists and is valuable for us" (Foucault [1971] 2001, 1009).²³ Cassirer similarly denied the possibility of presenting the history of culture as a harmonious whole: "we must recognise that culture is no harmoniously unfolding whole but is filled with the most violent inner tensions" (Cassirer [1942] 2011, 113).²⁴ A genealogy investigates how struggles or tensions have given rise to certain configurations of ideas.

We aim to dissolve the complex whole of preconditions that our science uses to understand appearances, into the most important threads that have constructed these conditions through their historical emergence [Entstehung]. Through this enterprise we hope to find on the one hand a factual perspective on the intricately entangled conceptual web [Gewebe] and on the other hand an understanding of the internal relations and dependencies between the individual elements of this web.²⁵ (Cassirer [1906] 2009, 5)

²² Faire la généalogie [...] de la connaissance ne sera donc jamais partir à la quête de leur "origine", en négligeant comme inaccessibles tous les épisodes de l'histoire.

²³ C'est repérer les accidents, les infimes déviations – ou au contraire les retournements complets –, les erreurs, les fautes d'appréciation, les mauvais calculs qui ont donné naissance à ce qui existe et vaut pour nous.

²⁴ Man muß einsehen und zugestehen, daß sie [Kultur] kein harmonisch sich entfaltendes Ganze, sondern von den stärksten inneren Gegensätzen erfüllt ist.

²⁵ Den komplexen Inbegriff von Voraussetzungen, mit denen unsere Wissenschaft an die Deutung der Erscheinungen herantritt, suchen wir aufzulösen und die wichtigsten Fäden gesondert in ihrer historischen Entstehung und Herausbildung zu verfolgen. Auf diesem Wege dürfen wir hoffen, zugleich einen sachlichen

A genealogy thus investigates how a specific web of conditions for knowledge was woven through time from individual threads. The resulting web is always the effect of a struggle over something at stake. What is at stake in a struggle can get re-appropriated and re-articulated over time. Participants in a struggle do not share the same space of reason: they only share the struggle over what the boundaries of this space should be. Bringing this struggle to light, showing how in various ways certain threads got discarded, whereas others that were previously disconnected became entangled, showing how the stakes were constantly reconfigured this is the aim of a genealogy.

Presenting a genealogy of ‘scientific explanation’ is a stance that aims to create an effect: “history will be ‘effective’ to the degree that it introduces a discontinuity in ourselves” (Foucault [1971] 2001, 1015).²⁶ My genealogy, thus, aims to show that ‘scientific explanation’ is not a given in the space of reasoning of philosophers of science, but is the contingent result of a whole range of accidents, miscalculations and contrasting intentions. The effect that my genealogy is intended to create is exactly the kind of effect that the Cassirer-Foucault historical-philosophical methodology aims to proliferate. However, this does not imply that my history is not truthful or objective: it can only have its effect if anyone can investigate the evidence for every claim in it. Because every aspect of a genealogy is open to evidentiary criticism, it has the strength to show the contingency of its subject matter. If this were not the case, anyone would be free to disregard it as unfaithful to the historical sources.

Of course, one could argue that this kind of effective history is a type of historical explanation, because it shows how an intellectual configuration came about, or because it yields understanding of how we have come to reason about science, etc. I have no direct argument against this line of thought, only the question that has guided the writing of this dissertation. Why would one want to identify this effective history as an explanation? What do we learn about historiography after such an identification? Why should we value a comparison between this type of explanation and other types? Conceiving this dissertation as an exercise in explanation is exactly what one should become skeptical of after reading it.

Einblick in dieses vielverschlungene begriffliche Gewebe zu gewinnen und die inneren Beziehungen und Abhängigkeiten zwischen seinen einzelnen Gliedern verstehen zu lernen.

²⁶ L’histoire sera “effective” dans la mesure où elle introduira le discontinu dans notre être même.

Chapter 1

The Emergence of Scientific Explanation

"We want to make ourselves conscious of what we have always been doing" (Carnap 1935b, 37).¹ Rudolf Carnap set himself this task in 1935 at the first conference of Unified Science in Paris. His presentation "From Epistemology to Logic of Science" [Von der Erkenntnistheorie zur Wissenschaftslogik] was an attempt to create a historical self-understanding for scientific philosophers.² According to Carnap, scientific philosophy had up to that point mixed both psychological and logical investigations into science, and this had lead to many ambiguities and misunderstandings among adherents of scientific philosophy (Carnap 1935b, 36). A new self-understanding of what a scientific philosopher could say about science would avoid these impure mixtures. Carnap proposed to actively and self-consciously transform all epistemological questions into questions about the logical properties of scientific language. This new boundary for philosophical discourse was meant to remove any suspicion that a scientific philosopher could inquire about objects in the world – that activity should be left to scientists, not to philosophers (Carnap 1935b, 38). In another essay of the same year Carnap distinguished between four equally valid ways to study science: a psychological, sociological, historical and logical investigation. The first three perform a factual investigation of beliefs that scientists have, and of the institutional and social aspects of scientific inquiry. The logic of science only investigates

¹ "Wir wollen uns also jetzt nur etwas bewusst machen, was wir immer schon getan haben."

² The actors that I discuss in this dissertation use a variety of terms to denote their philosophical project: 'scientific philosophy', 'Unity of Science', 'logical empiricism', 'logical positivism'. What this exactly means differed for all the actors. Throughout this dissertation I use 'logical empiricism' to denote the network of actors that regularly wrote to each other, organized conferences together and jointly edited a journal, *Erkenntnis*. 'Logical empiricism' is good general term to loosely characterize the variety of philosophical positions that these actors held, since this term is what most actors gradually adopted to describe themselves throughout the 1930s (Uebel 2013). This network gradually disappears after the second World War and is replaced by a professional, academic network of philosophers in the United States. This network is 'philosophy of science' *as a discipline* that has its own journals, centers, handbooks, courses. I sometimes still use 'philosophy of science' as a term to denote the general inquiry of a philosopher into scientific knowledge – in that sense, Aristotle can also be called a philosopher of science.

statements asserted by scientists in abstraction from the psychological and social conditions of the assertions (Carnap 1935a, 30). Carnap's boundary work was a hazardous process: it remained unclear to what extent contemporary philosophers would understand Carnap's attempted shift.

Exactly how to understand a scientific assertion in a logical framework, in abstraction from the psychological or social conditions of science, was something at stake in the philosophy of science of the 1930s and 1940s. Most actors in the network of scientific philosophy had their own views on the validity and execution of such a distinction. Carnap was not the only philosopher trying to delineate the boundaries of the logic of science. In the 1930s and 1940s philosophy of science had little institutional anchors both in European and American universities. Handbooks, anthologies or other means to a curricular tradition in philosophy of science were still absent. Consequently, the boundaries that delimit what the logic of science is, were largely open: there were no clear exemplars of success, no standard topics and no shared methodologies. What a logic of science should do, became a crucial question which would determine what a philosopher could say about science and which kinds of arguments a philosopher should produce in support of her assertions. Roughly by 1960 these boundaries had rigidified, and at least certain topics and methods were firmly established. This dissertation describes some of the struggles concerning those boundaries among scientific philosophers, roughly between 1928 (the publication of Carnap's *Der logisch Aufbau der Welt*) and 1965 (the publication of Hempel's *Aspects of scientific explanation*). Specifically, it focuses on the method of logical reconstruction and its application to the topic of scientific explanation. This method and topic will prove to be of importance for the struggle concerning what a logic of science should do. Much like Carnap's address to scientific philosophers of the 1935 Paris conference, the research presented in this dissertation is an attempt to grow a new consciousness: the way philosophers have been trained to think about science is the result of a historical struggle that could have taken a different path. As such this dissertation is an invitation to unlearn a central, conceptual assumption in philosophy of science: we need not assume that explanation is a helpful concept in the articulation of science.

In contemporary philosophy of science scientific explanation is a well-established, central topic of inquiry. Every overview of the field contains at least one chapter on it and it is a standard component of course work in philosophy of science (Schurz 2013, 348; Bird 1998, 67; Ladyman 2001, 200). There is also a general consensus among philosophers that scientific explanation is a central concept in any understanding of science. The concept of explanation in contemporary philosophy of science has three main components. First, it is considered to be a central aim across all kinds of scientific inquiry: it drives this inquiry, and at least part of the end result of science should always be an explanation of the phenomenon under investigation. Second, explanation is considered in opposition to description. Scientific inquiry not only aims to describe the world (answering the question what is the case), it also aims to offer explanations of the phenomena that it investigates

(answering the question why something is the case). Consequently, as a third component, scientific explanation is often conceived as a specific answer to Why-questions. It is the goal of philosophers to offer a model or theory of explanation in science which covers these three aspects (Woodward 2017, sec. 1). Philosophers in general believe that there are some paradigm cases of scientific explanations available, and they attempt to form a philosophical model from these cases that should in some meaningful way capture the structure of scientific explanation in general. Currently, there is no general consensus that any offered philosophical model is adequate. However, this has not deterred philosophers from attempting to provide improvements to the available models, or meaningfully combine features of several models within a pluralist account of scientific explanation. Among the most prominent candidates are the covering law, unificationist, causal-mechanical, interventionist or mechanist accounts of scientific explanation.

This on-going philosophical reflection on scientific explanation can be considered a fabricated web. This web ties together all kinds of philosophical ideas, arguments and preconceptions in philosophy of science: philosophers constantly use the concept of explanation to think about science. Since this concept has become an uncontested aspect of any understanding of science, a philosopher can use it to define other aspects of science. E.g. the notion of a mechanism was introduced in contemporary philosophy of science as a central feature in one type of explanation in the sciences, namely explanations of how a phenomenon comes about or how some significant process works (Machamer, Darden, and Craver 2000, 2). Without a prevalent consensus that scientific inquiry has the aim to explain, such introductions would be more difficult. The concept of scientific explanation also allows the philosopher to distill a specific epistemic activity out of scientific texts: a scheme from a scientific handbook depicting the depolarization of a nerve cell can now be introduced as a case of scientific explanation which fits the mechanist model (Machamer, Darden, and Craver 2000, 17). Scientific explanation is a productive concept in philosophy of science: it installs a truth about what science is, and this truth aids the philosopher in producing further insights about science, its practice and its progress. As I will argue in this dissertation, this productive truth about science, namely that it explains, did not always play a role in the philosophical reasoning about science. Only during the last seventy years has scientific explanation been considered a central aim of science and, consequently a central topic for philosophy of science.

The writings of logical empiricist philosophers from the 1930s contain no discussion of explanation. The first decade of the journals *Erkenntnis* [1930-1938] and *Philosophy of Science* [1937-1947], the available journals for philosophy of science at that time, contain no account, model or theory of scientific explanation. There was no discursive articulation of anything like the modern concept ‘scientific explanation’ (as an aim of science, distinct from description and an answer to why-questions). When Rudolf Carnap formulated tasks for the logic of science [Aufgabe der Wissenschaftslogik] in 1934, scientific explanation was not listed among the possible topics. According to Carnap the logic of science is

concerned with science as an ordered structure of sentences [ein geordnetes Gefüge von Sätzen]. If one investigates this linguistic structure, one can investigate contradictions between theories, definitions of scientific concepts, the inferential relations among theories and the relation between laws and observational sentences (Carnap 1934a, 6). In Carnap's view, a logic of science was not concerned with how science explains phenomena or how science yields an understanding of the world. Similarly, Hans Reichenbach mentioned the nature of the synthetic a priori, the problem of induction and the role of conventions among the central topics for the logic of science in his 1938 book *Experience and Prediction* (Reichenbach [1938] 1961, 8–9). Scientific explanation was not discussed in Reichenbach's work on scientific philosophy. In the American context Dewey's late work *Logic: The Theory of Inquiry* shows that explanation was not an element in the pragmatic understanding of science. For Dewey the aim of science is the attainment of stable beliefs through operations of inquiry (Dewey 1938, 534–35).³ Explanation received no mention in this work. Thus, even in the pragmatist context of the United States explanation was not discussed as the central aim of science.

Consequently, the insight that explanation plays a fundamental role in any understanding of science is not natural or necessary for a philosophical inquiry of science, even though most philosophers of science currently conceive scientific explanation as an undeniable aspect of scientific inquiry and as a starting point in their analyses. This position about the role of explanation in science is, at least, contingent on some further reflections or arguments that first have to be accepted. The installation of the contemporary truth that science explains was the result of a historical development in the philosophical reflection on science in the second half of the twentieth century. This dissertation is devoted to a description of this development. It brings to light how separate threads in professional philosophy were woven together into the stable web of scientific explanation against a background of intellectual migration during the 1940s. Before I continue with a discussion of how this stable web emerged, it is important to discuss in more detail how the contemporary stability of this web works.

The web that supports philosophical reasoning about scientific explanation is very sturdy, but has received little critical scrutiny. The weaving of the web contains a puzzling loop. Any philosopher of science who gives an overview of developments in the research on scientific explanation, starts with a discussion of Carl Hempel's and Paul Oppenheim's article "Studies in the Logic of Explanation" from 1948. Wesley Salmon, who has written the most influential of such overviews, refers to this article as "epoch-making" (Salmon

³ In his 1965 *Aspects of Scientific Explanation* Hempel used an example from Dewey that he describes as a case of explanation (Hempel 1965, 335). The example concerns the emergence of bubbles from under a glass tumbler while doing the dishes (Dewey 1910, 70). Interestingly, Dewey discusses this example to show how reflection and experiment interact. Dewey simply does not talk about explanation as an aim of inquiry in this case. Also see 4.5.

1989, 3) and "the fountainhead from which the vast bulk of subsequent philosophical work on scientific explanation has flowed – directly or indirectly" (Salmon 1989, 8). According to Salmon the publication of this article is an event of great intellectual importance. As "one of the most significant philosophical achievements of the twentieth century" (Salmon 2000, 314) it marks the division between the prehistory and history of modern discussions of scientific explanation (Salmon 1989, 10). Supposedly, the effect of the paper is the installment of the unbroken consensus in philosophy of science, that explanation is a fundamental aim of science (Salmon 1989, 4). Salmon's narrative about the foundational effect of the 1948 article has become a standard view. It is taken over by many philosophers of science in their overviews of the field. (Koertge 1992, 86; Douglas 2009, 447; Woodward 2017; Schurz 2013, 348; Newton-Smith 2017, 127; Bird 1998, 67; Ladyman 2001, 200; Nancy Cartwright 2004, 4). Salmon's assessment of the importance of this paper has good grounds. Hempel and Oppenheim open their paper with a characterization of scientific explanation that still captures the concept of scientific explanation in contemporary philosophy of science. All three aforementioned components of the concept are there: it is an aim of science, distinct from description and an answer to why-questions.

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. (Hempel and Oppenheim 1948b, 135)

Yet, there is a puzzling circle in Salmon's standard story about the origins of research on scientific explanation. In the 1948 article Hempel and Oppenheim do not mark their enormous breakthrough themselves. They present no motivation for their introduction of explanation as a central aim of science. On the contrary, they believe that there is a "rather general agreement about [explanation as a] chief objective of science" (Hempel and Oppenheim 1948b, 135). However, this presupposition is squarely wrong – scientific explanation before their paper had not been the topic of any discussion in philosophy of science. Salmon himself often correctly notes that scientific philosophers and philosophically inclined scientists from the first half of the twentieth century explicitly denied that science is in any way concerned with explanation (Salmon 1989, 5, 2000, 313). No logical empiricist philosopher was ever concerned to articulate scientific explanation as an aim of science before Hempel and Oppenheim's article. It was simply not a part of the logical reconstruction of science. The examples of Rudolf Carnap and Hans Reichenbach are good cases: even though they are often considered to be the most important predecessors to contemporary philosophy of science, they never developed any account of scientific explanation (Salmon 2000, 312).

Here, the circle becomes evident: contemporary philosophers of science attribute the current consensus that science explains to Hempel and Oppenheim, while Hempel and

Oppenheim in turn attribute the idea to a general consensus which did not exist. The 1948 article did not contain any motivation or argument for the idea that explanation is an aim of science, but the article was extremely successful in creating a consensus on explanation as an aim of science, specifically by performing an analysis of explanation within a formalized language. Salmon speaks of an analysis "of unprecedented precision and clarity", one that enabled many philosophers to perform incremental improvements on the offered logical reconstruction (Salmon 1989, 3). By giving a logical model of explanation, Hempel and Oppenheim created the opportunity to discuss the formal merits of the model against the background of mostly intuitive examples. By setting clear boundaries for reasoning about the model, Hempel and Oppenheim's format of logical modeling ensured that any logically minded philosopher could contribute to the discussion. Salmon's own overview of the research on explanation is a historical narrative about the progress of this discussion: it begins with the views of Hempel and Oppenheim, discusses the various paths of criticism that were explored, and ends with an assessment of these paths. The introduction of such a historical narrative creates a loop. It excludes earlier views on science that are not in line with Hempel and Oppenheim's starting point. These views are considered by Salmon to be "prehistory" (Salmon 1989, 10). Since earlier discussions can rarely be conceived within the boundaries of the formal model, Salmon put them aside as vague.

Philosophers in roughly the first half of the twentieth century who wondered about scientific explanation may have had no clear idea of what it might be. 'Explanation' (especially without the qualifier 'scientific') seems so vague and ambiguous, with so many subjective overtones, that it is hard to see what sense can be made of it. Hempel and Oppenheim gave a clear model. (Salmon 2000, 314)

Salmon's narrative only reconstructs the valid avenues of research within the lines that were drawn by the 1948 paper. As a result, there is a circle. One performs research within the tradition of the 1948 paper, because one investigates scientific explanation, and one investigates this topic, because one situates oneself within this tradition.

In order to justify this loop philosophers of science have used two further genealogical narratives. Both serve as myths: the narratives justify Hempel's concern with explanation as an aim of science, but these narratives have rarely been squared against historical research on Hempel's ideas. The first myth situates Hempel and Oppenheim's paper in the tradition of Aristotle (Jeffrey 1969, 104; Salmon 1989, 3; Reck 2013, 312; Schurz 2013, 348). As is often noted, Aristotle, similarly to the 1948 paper, opposes knowing-that to knowing-why in his Posterior Analytics (APo 93a16). On Salmon's account Aristotle recognized that explanations are some form of deductive arguments, but Aristotle failed to explicate which kinds of arguments properly qualify as explanations. Consequently, Aristotle and "his successors" (who are never specified) did not meet with great success in specifying what constitutes a scientific explanation. In Salmon's narrative, this changes with the 1948 paper which marks the transition from prehistory to history, from what was still

vague and confused in the long tradition of Aristotle, to a consistent set of philosophical ideas in Hempel and Oppenheim that can be the subject of a coherent narrative. Even though this historical lineage between Aristotle and Hempel has often been drawn, no one has ever claimed that Hempel and Oppenheim's distinction between why-questions and what-questions share a common philosophical or scientific motivation with Aristotle's analysis of episteme. Nonetheless, the link with Aristotle has served the purpose of legitimating Hempel and Oppenheim's philosophical starting point by situating it in a 2400 year-old vaguely identified tradition.⁴

The second myth attempts to account for the absence of explanation in early logical empiricist philosophy. Supposedly, the earlier philosophers in this tradition had specific, context-bound grounds for their silence on the concept. The myth states that Hempel and Oppenheim in their 1948 paper could cast aside a restraint towards the concept "explanation", because their context had shifted from German metaphysical philosophy to American pragmatist and rationalist philosophy. Supposedly, logical-empiricists like Carnap, Reichenbach or Neurath had still situated explanation in the domain of metaphysics, because they operated in a German philosophical context that was dominated by post-Kantian idealist philosophy. In this context, it was a common philosophical idea that any explanation of nature requires a reference to empirically inaccessible agencies such as entelechies or vital forces. Consequently, logical empiricists shied away from this non-empirical notion of explanation. As the story goes, Hempel and Oppenheim show that "explanation" is not necessarily a metaphysical concept, related to the human understanding and beyond the sphere of empirical knowledge. By reconstructing the concept within a formal language, they show how it is possible to understand the explanatory aim of science within an empiricist framework. This supposedly liberated philosophers of science from their restraints towards the concept (Salmon 1989, 4; Douglas 2009, 447–48).⁵

This story has its origin in Carnap's handbook for philosophy of science. There, Carnap narrates about the opposition to why-questions in early logical empiricism.

In the nineteenth century, certain Germanic physicists, such as Gustav Kirchhoff and Ernst Mach, said that science should not ask "Why?" but "How?" They meant that science should not look for unknown metaphysical agents that are responsible for certain events, but should only describe such events in terms of laws. This prohibition

⁴ For an expanded, historical narrative that connects Aristotle's *Posterior Analytics* to debates in philosophy of science centered around explanation, see (Psillos 2012; McMullin 1992).

⁵ There is an alternative to this myth originating in (Nancy Cartwright 2004, 232), and taken over by (Woodward 2017). In this version logical empiricist philosophers eschewed metaphysics and as a consequence exchanged discussions over the metaphysical notion of causation with discussions over explanations. In this story Hempel translated talk of singular causation into talk of singular explanation (which only requires the reference to general laws capable of confirmation). Similar to Salmon's story, there is little historical evidence available in support of this story.

against asking "Why?" must be understood in its historical setting. The background was the German philosophical atmosphere of the time, which was dominated by idealism in the tradition of Fichte, Schelling, and Hegel. These men felt that a description of how the world behaves was not enough. They wanted a fuller understanding, which they believed could be obtained only by finding metaphysical causes that were behind phenomena and not accessible to scientific method. Physicists reacted to this point of view by saying: "Leave us alone with your why-questions. There is no answer beyond that given by the empirical laws." They objected to why-questions because they were usually metaphysical questions. Today the philosophical atmosphere has changed. In Germany there are a few philosophers still working in the idealist tradition, but in England and the United States it has practically disappeared. (Carnap [1966] 1995, 12)

This passage is historically suspect. Mach and Kirchhoff, both physicist who emphasized that physics aims to describe the world and not explain it, were not reacting to German philosophy. Instead, both accounted in their philosophy of physics for some of the internal developments of late nineteenth century physics. Moreover, as we will see below (2.2), many philosophers and scientists of the first half of the 20th century articulated scientific knowledge without the meta-concept of explanation, but not as a reaction against idealist philosophers. Thus, Carnap's own retrospection seems an inadequate characterization of the actual developments. Moreover, Carnap never discussed models of scientific explanation in his own work, and he never referred to explanation as a distinct aim of science. After Hempel's work on explanation Carnap would use the term "explanation" in his handbook on philosophy of science, but it is consistently understood as one of two related functions of scientific laws. Laws connect observational statements with one another: if these statements describe events from the past such connections can be labeled explanation, and if these statements are about future events, this relation is a prediction (Carnap [1966] 1995, 6–7). Such understanding of "explanation" does not amount to the starting point of Hempel and Oppenheim's 1948 paper which discusses explanation as an aim of science distinct from description. There are two initial reasons to believe that Carnap's retrospection should not be used as a legitimization of what happened within scientific philosophy of the twentieth century: Carnap's own use of the term "explanation" is not in congruence with Hempel and Oppenheim's starting point of 1948 and the worries that Carnap associates with the fear for why-questions in the German climate of philosophy, were never mentioned by Hempel or Oppenheim as their motivation to reintroduce the term "explanation".

The reference to an Aristotelian tradition and Carnap's story about the background of German idealism have never been properly investigated. Until now they have mainly served as myths to justify the sudden appearance of explanation as an aim of science in the paper of Hempel and Oppenheim. Once one recognizes the function of these stories as legitimating myths, three historical questions become pertinent.

1. How did philosophers reflect on the notion ‘explanation’ before the 1948 paper? What function, if any, did that notion play both within the logical-empiricist background in which Hempel's⁶ early ideas on science matured and within the American climate in which Hempel started his academic career after his migration to the United States in 1939?
2. How do Hempel's early ideas on explanation relate to the conditions in both contexts and how do these early ideas evolve into the 1948 paper?
3. How does Hempel's work come to create the consensus in later philosophy of science that science aims at explanations?

The first two questions will be discussed in chapters two and three respectively. The third question will be treated in greater detail in chapter four. All three questions are pertinent, because it is important to know how explanation as a crucial stake for the philosophical reflection on science came about. Its origin should not be supported by myths. If explanation really is a central aim of science, philosophers should know how this truth about science was only uncovered as late as 1948. In an ideal scenario the historical research would show how philosophers gradually became convinced that explanation is a central aim of science through the proliferation of good arguments. This would prove that widely agreed upon truths in philosophy, e.g. that science explains, are the results of reasoned reflection that reached a certain stability over time. In a worst case scenario the historical research would prove that certain agreed upon truths resulted from pure historical contingency, e.g. the assumption of one philosopher was suddenly taken over by every other philosopher without further discussion. The current standard view on the history of explanation that originated in Salmon's work contains elements of both scenario's. Salmon's narrative, on the one hand, meticulously reconstructs the arguments and counterarguments over models of scientific explanation that originated out of the 1948 paper. Salmon lists ten fundamental philosophical issues related to the paper and then shows how the debates over these issues were structured in the decades after 1948 (Salmon 1989, 23–24). Such a historical reconstruction exemplifies the ideal scenario. On the other hand, Salmon reverts to brute, sociological facts about philosophy of science when it comes to the consensus that explanation is a fundamental aim of science. "In recent debates there has been quite general agreement that science can tell us not only *what*, but also *why*" (Salmon 1989, 5). This is

⁶ I do not focus on Paul Oppenheim's ideas on explanation for two reasons. First, only Hempel continued to develop and defend the covering law model of scientific explanation after 1948. Second, it is hard to assess the extent to which Oppenheim actually contributed to the ideas about explanation. From Nicolas Rescher's personal account of Oppenheim's method, we know that Oppenheim mainly contributed to his co-authored papers by offering some general ideas and giving feedback on work (Rescher 1997, 342). This does not imply that Oppenheim was unimportant: materially, he was essential for Hempel's early career (see 2.4.2) ; and there are many indications that Hempel, until he moved to Yale in 1948, had weekly or even biweekly meetings with Oppenheim to talk about his work.

similar to a worst case scenario: Hempel and Oppenheim introduced an idea about science, claimed it was widely accepted - even though it was not - and in later years every philosopher of science agreed.

The worst case scenario can be found in the aforementioned loop mechanism in Salmon's narrative. The narrative suggests that the 1948 paper created a loop in the philosophical reflection on science and consequently reified scientific explanation as a concept in philosophy of science. The mechanism of reification goes as follows. The 1948 paper introduced an idea about scientific explanation as an aim of science and then set out to analyse it. This analysis created the possibility to critique the proposed analysans and as a consequence the various criticisms reified the analysandum. After several decades the initial starting point, the analysandum, had become a fixed, unalterable element of reflection on science in philosophy.

Salmon's discussion of the initial reception of the 1948 paper suggests that this mechanism actually occurred. In the first decade after its publication the 1948 paper received little attention in the literature. Salmon has called this absence of explanation in the literature "a strange temporal gap" (Salmon 2000, 314). He speculated that this silence implies a wide acceptance of Hempel and Oppenheim's account (Salmon 1989, 33). However, it might also indicate that scientific explanation remained an unrecognized topic in philosophy of science in the immediate years after 1948. Between 1957 and 1963 things change: fourteen papers are published that discuss the 1948 paper directly in a varied collection of journals, *Philosophy of Science* (Eberle, Kaplan, and Montague 1961; Kaplan 1961; Weingartner 1961a; Grünbaum 1962a, 1962b; Henson 1963; Kim 1963; Rescher 1963), the *British Journal for Philosophy of Science* (Scheffler 1957; Rescher 1958; Yolton 1959; Goudge 1958), *Synthese* (Rescher 1962) and *The Philosophical Review* (Hanson 1959). This results in three early debates on the analysis offered in the 1948 paper: a debate on the symmetry between explanation and prediction (Hanson 1959; Rescher 1958), on the formal conditions of the model (Eberle, Kaplan, and Montague 1961; Kaplan 1961; Kim 1963), and on the pragmatic conditions of explanation that do not fit in with the model (Yolton 1959; Scheffler 1957; Scriven 1962). However, none of these papers discuss Hempel and Oppenheim's starting point, namely that scientific explanation is a fundamental aim of science. They all accept the original analysandum 'explanation' as a valid, pursuitworthy topic for philosophy of science. In the 1960s the ball gets rolling: publications on models of scientific explanation become a regular element in philosophy of science journals, handbooks and anthologies. The procedure of analysis, operating with counter examples and language intuitions, reified the concept of scientific explanation. This still leaves open the question how reification was able to occur and why it took about ten years to initiate. Maybe Salmon is right and philosophers of science came to agree on the necessity of a concept like "scientific explanation" in their philosophical reflection on science during the first decade, and only later realized that Hempel and Oppenheim's offered analysis was insufficient. However, this should not be assumed. It requires actual historical

research into the intellectual, institutional and educational conditions of the philosophers involved in order to assess how the current consensus came about.

Chapter 2

At the Intersection of Historical and Scientific Philosophy: the Problem of History

Before one can begin to assess how Hempel's model of scientific explanation was taken up in philosophical reasoning at the end of the 1950s, it is important to understand how philosophical reasoning about science was ordered and what role the notion 'explanation' played in it before that time frame. Hempel and Oppenheim's reasoning in the 1948 paper did not emerge out of nowhere. It was the result of a specific configuration of several forms of reasoning that came together. Even though their paper is the first occurrence of the concept of explanation as an autonomous aim of science in logical empiricist writing, Hempel had already extensively used the term 'explanation' in the first paper he wrote after his definitive migration to the United States in February 1939.¹ In his 1942 paper, *The Function of General Laws in History*, Hempel mentioned 'explanation' 104 times. However, in that paper Hempel nowhere discussed the opposition between explanation and description as two distinct aims of science. Neither did he understand scientific explanation as an answer to why-questions. Both are crucial elements of the analysandum from the opening of the 1948 paper. Moreover, Hempel never stated in 1942 that he was analyzing or modeling scientific explanation in a specific discipline. Neither the title, the set-up nor the conclusion of Hempel's paper contained the term 'explanation'. Thus, the argument that Hempel presented in 1942 had a different focus than the 1948 paper.

As its title suggests, the 1942 paper is about the function of general laws in history. Hempel defended the position that "general laws have quite analogous functions in history and in the natural sciences. They formed an indispensable instrument of historical research" (Hempel 1942, 35). In the opening paragraph Hempel stated that his argument was intended

¹ Douglas and Koertge have already explicitly discussed the fact that Hempel's reflection on explanation starts in his 1942 paper on historiography (Douglas 2009, 448; Koertge 1992, 87–88). They did not investigate in detail how and why the term explanation exactly plays a role in Hempel's thinking on historiography. Though this chapter will partly answer this question, a complete investigation of the 1942 paper follows in Chapter 3.

to rebut "a rather widely held opinion that history, in contradistinction to the so-called physical sciences, is concerned with the description of particular events of the past rather than with the search for general laws which might govern those events" (Hempel 1942, 35). By showing how general laws have a function in historiography, Hempel concludes that such position should be discarded: "It is unwarranted and futile to attempt the demarcation of sharp boundary lines between the different fields of scientific research" (Hempel 1942, 48). Thus, Hempel explicitly argued for the unity of science against the position that there is an epistemological distinction between the natural sciences and history. According to this position, the natural sciences would order events in patterns and form general laws through general concepts, while historiography would only inquire into the uniqueness and singularity of events. This position entails an (epistemo)logical split between the historical and the natural sciences – from now on, we will call this thesis ZER, *Zerspaltung* [disruption] thesis.²

The origins of Hempel's ideas on explanation revolve around two intellectual problems. On the one hand, there is the question how to understand the relation between the natural sciences and the historical sciences – here, Hempel wanted to avoid ZER. On the other hand, there is the question how to understand scientific concepts and scientific laws. Hempel believed a proper understanding of laws might solve the first problem, how to understand the relation between historiography and the natural sciences. It is no surprise that these problems are mixed together in the 1942 paper. Both issues had already played an important role in German philosophy of the early twentieth century. In the rest of this chapter, I first give an overview of the most important philosophical arguments concerning ZER and scientific laws that form the background of the 1942 paper (see 2.1 & 2.2). Next, I discuss how ZER played a role in several conflicts among logical empiricist philosophers, both in their writings and in their correspondence. These discussions would eventually lead to Hempel's paper (see 2.3 & 2.4).

2.1 South-West Views on History

At the beginning of the 20th century German philosophy was faced with the task of incorporating the newly-found historical disciplines of the 19th century into philosophy. This was the conclusion of Wilhelm Windelband's 1904 reflection on the state of philosophy 100 years after Kant's death (Windelband 1904). Certainly not all philosophers

²I use *Zerspaltung*, because this term was used by Rudolf Carnap to denote any position that (epistemo)logically distinguishes between the natural and the historical sciences (Carnap 1931, 432).

in Germany agreed with Windelband's specific description of this problem, but many German philosophers accepted that there was something at stake for philosophy surrounding the historical sciences. There are two aspects to these stakes. First, there is the almost universally accepted idea around the turn of the century that historiography as a science needs to be accounted for in epistemology. This idea was aptly articulated by Windelband in his 1904 reflection: "The great, new fact of the existence of the historical sciences demands, as a first task, that critical philosophy expands the Kantian notion of knowledge" (Windelband 1904, 11). This novel task for epistemology had already been an important problem of 19th century German philosophy and became a central epistemological question for many German philosophers around the turn of the century.³ Windelband was not the only or even the most prominent philosopher to engage with the philosophical problems over historiography as a science. His 1904 text, however, understands these problems as central to the agenda of future philosophy in the 20th century. This renders it a good starting point to understand what was at stake in German academic philosophy at the turn of the century. A second aspect of the stakes is the task for philosophy to apply history to the analysis of knowledge itself. For Windelband it was of central importance to incorporate the history of human thinking into the analysis of knowledge. On Windelband's account, critical philosophy had to be updated given historical developments in the sciences of the 19th century. "Kant's understanding of 'science' is – historically understandable – restricted to the methodical identity of the theoretical inquiry into nature, which is determined by the Newtonian principle" (Windelband 1904, 10).⁴ Understanding how scientific knowledge is not stable, how it changes its own structure throughout its history, should be accounted for in epistemology.

According to Windelband, the outdated Newtonian principle that Kant had upheld states that science aims at the production of natural laws. These laws abstract from the facts of experience whatever remains the same throughout all of them. Science consequently produces classificatory concepts [Gattungsbegriffe] which order experience in kinds (Windelband 1904, 12). Even though this goal of science and the related logical structure of its concepts was appropriate to the scientific method of Kant's time, it could no longer, according to Windelband, be tolerated in 20th century philosophy of science as the sole conceptual structure of scientific reasoning. Historiography had joined the ranks of the sciences. Windelband called this event one of the "most significant appearances of 19th century mental life" (Windelband 1904, 10). For Windelband, the historical sciences are, contrary to the natural sciences, interested in the individual moments of the past. Therefore,

³ The most well-known example is perhaps Wilhelm Dilthey, who had already put an epistemology of the historical sciences on the philosophical agenda during the last quarter of the 19th century. But many other German philosophers had also engaged this question; for an overview of this tradition, see (Iggers 1983; Beiser 2012).

⁴ Kants Begriff der 'Wissenschaft' ist - historisch sehr begreiflich - eingeengt auf den methodischen Charakter der theoretischen Naturforschung, bestimmt durch das Newtonsche Prinzip.

the conceptual order of classification that abstracts from the individual properties of facts in experience cannot be understood as the conceptual order of historiography: unlike the natural sciences, it aims at the singular. Thus, a different kind of conceptual order is required to understand the historical sciences, an order that relates individual facts to each other without abstractions (Windelband 1904, 12–13). In his 1904 paper Windelband calls this alternative conceptual order a *frame* [Gestalt]. Similar to the natural laws these *frames* connect the historical events to each other: they create “an ordered cohesion that we can prepare out of the given based on general and necessary interests of our understanding” (Windelband 1904, 13).

In Windelband’s account, the dual stakes pertaining to historical knowledge cannot be separated. A historical account of the expanding boundaries of scientific inquiry (second stake), for him, necessitates expanding the epistemology of science with an alternative logical structure for historical concepts (first stake). This expansion was “best developed and formulated”, according to Windelband, by his former student Heinrich Rickert (Windelband 1904, 13). Rickert’s work *The Limits of Concept Formation in Natural Science* [Die Grenzen der naturwissenschaftlichen Begriffsbildung] further develops Windelband’s idea that the historical sciences require a conceptual structure distinct from the natural sciences.⁵ I summarize some of the core theses of Rickert’s work, because this will allow us to understand how logical empiricists’s concerns over history, especially those in Hempel’s 1942 paper, relate back to Rickert’s ideas.

Rickert’s work has a two-fold structure. First, Rickert argues that the conceptual order of the natural sciences has certain limits [Grenzen]. Second, Rickert argues that the historical sciences operate with a different conceptual order than the natural sciences: they objectify those aspects of reality that lie beyond the limits of natural scientific concepts. Taken together both conceptual orders (the historical and the natural) form the totality of possibilities to make reality accessible to conceptual knowledge. The following quote perfectly represents this argumentative structure:

We can now state that the limit of concept formation in the natural sciences is the beginning of the interest of historiography. In this way both types of science delimit each other logically and entail everything that empirical reality can offer to scientific aims.⁶ (Rickert 1929, 267)

⁵ This work was originally published as a completed volume in 1902. For my discussion, I use the latest version of the book that was published in 1927. Since the work has only partially been translated by Guy Oakes, most of the translations are my own. Wherever applicable I have used Oakes’ translation.

⁶ Hier dürfen wir nur sagen, dass dort, wo die Begriffsbildung der Naturwissenschaft ihre Grenze findet, meist das Interesse der Geschichte erst beginnt. So ergänzen die beiden Arten von Wissenschaften einander logisch und umfassen zugleich alles, was die empirisch Wirklichkeit an wissenschaftlichen Aufgaben stellt.

Rickert presents his work as an investigation in the logic of science. The investigation is entirely independent from empirical work in the sciences. It explicitly does not aim to show how scientists have to perform their work, even though Rickert believes this might be an additional beneficial result of his logical inquiry (Rickert 1929, 303). Rickert's logic is intended as a transcendental investigation: it lays bare how it is possible that science can investigate empirical reality.⁷ It investigates how concepts operate on reality as it is given to us in our intuition [Anschauung] and how these concepts produce knowledge of reality. Rickert's logical inquiry rests on two fundamental assumptions. First, scientific knowledge cannot be a depiction [Abbildung] of reality. We know physical reality only through its arrangements [Gestaltungen] in our intuition as they are extended in space and time (Rickert 1929, 32). This intuition, however, is an inestimable [Unübersehbar] manifold of arrangements in space and time. "To know every arrangement [in space and time] individually is an unsolvable task for the finite human mind" (Rickert 1929, 33). Rickert defines this unsolvable task as the first limit of our knowledge which he also calls the *extensive inestimability* [extensive Unübersehbarkeit] (Rickert 1929, 34).⁸ Second, Rickert assumes another inestimable aspect of the empirical manifold: "every singular body in our intuition, that one would grasp out of the inestimable fullness, would still offer us a manifold on its own, however simple we would choose it, and, upon closer inspection, we would find out that this manifold would appear ever larger the more we would investigate it" (Rickert 1929, 33).⁹ Rickert defines this as the *intensive inestimability* [intensive Unübersehbarkeit]. This assumes the possibility to identify singular arrangements in our intuition that have no relationship whatsoever with anything else and that form inestimable manifolds on their own (Rickert 1929, 33).

⁷ Consequently, Rickert does not believe that the difference between historiography and natural science is "one of method", as George Iggers has claimed (Iggers 1983, 153). Rickert was self-consciously purely interested in the philosophical question about the logical limits of concept formation. He barely discussed any actual historical work in his immense book on historiography. Beiser has also noted that Rickert's primary concern is with a purely formal distinction between the sciences, in the sense of a transcendental investigation (Beiser 2012, 401).

⁸ Rickert uses the term 'manifold' [Mannigfaltigkeit] to denote the infinite arrangements of the intuition. 'Inestimability' [Unübersehbarkeit] is used to denote a logical characteristic of the infinity of the manifold, namely the impossibility to imitate this infinity of arrangements in any kind of conceptual language. Concepts will only prove to have validity over the manifold, but cannot represent it. It is inestimable by conceptual standards. Beiser translates 'Unübersehbar' as infinite (Beiser 2012, 404). I have chosen not to follow this translation, because I believe a term should be reserved to denote the logical characteristic of the manifold, namely that it cannot be represented by concepts *because of its infinity*. Inestimability, as a somewhat understandable neologism, also preserves the relation to a subject who wants to make an estimate of the manifold, which is also contained in the original German.

⁹ Jeder einzelne anschauliche Körper nämlich, den wir aus der unübersehbaren Fülle herausgreifen, bietet uns, so einfach wir ihn auch wählen mögen, immer noch eine Mannigfaltigkeit dar, und wir werden, wenn wir uns an eine nähere Untersuchung machen, finden, dass diese Mannigfaltigkeit um so grösser zu werden scheint, je mehr wir uns in sie vertiefen. (Rickert 1929, 33)

From these two assumptions, it follows, according to Rickert, that scientific inquiry can have two distinct interests, into the extensive and intensive aspect of the intuitive manifold (Rickert 1929, 224).¹⁰ In order to overcome the inestimability of the intuitive manifold in both aspects and achieve objective knowledge of reality, it is required to introduce concepts that have a validity over the manifold (Rickert 1929, 215). Scientific concepts cannot depict empirical reality, since it is both extensively and intensively infinite. Consequently, concepts process and transform empirical reality in a way that is valid without depicting reality. Rickert defines concepts as “logical structures whose content encloses the valid knowledge of objects” (Rickert 1929, 297). A logic of valid scientific judgments investigates how the transformation of intuitive reality by concepts can have validity (Rickert 1929, 214). Rickert claims that there are only two possible transformations that produce such validity.

Empirical reality can also be brought under a logical perspective that differs from that of nature. Empirical reality becomes nature when we conceive it with reference to the general. It becomes history when we conceive it with reference to the distinction [Besondere] and the individual [Individuelle]. Every empirical science proceeds from immediately experienced reality in its concrete actuality and individuality, and every empirical science must single out what is essential from reality. In other words, it must destroy the immediacy of reality. (Rickert [1902] 1986, 54, 1929, 227)

In overcoming the extensive aspect of the intuitive manifold one can use generalizing concepts that abstract from the specific (space/time) properties of individual arrangements in the manifold. The intensive aspect can be overcome by using individuating concepts. Rickert believes that these two conceptual orders are mutually exclusive conceptual operations and together form the complete spectrum of conceptual understanding: “in the sciences, we can have no understanding of a third way to process the given” (Rickert 1929, 267).

For Rickert, the conceptual understanding in the natural sciences operates with concepts that abstract from and generalize over the infinite extension of objects in time and space. There are three stages in the formation of these concepts (Rickert 1929, 102). First, there are *elements of concepts* [Begriffselemente]: words that can be related to a multitude of individual arrangements in intuition. In the second stage, there are classifications [Klassifikationen]. These are ordered and simplified versions of *elements of concepts* that are strictly determined by the available empirical information about arrangements in the manifold. In a third stage, classifications are sought that show how elements of the manifold are related to one another in a strictly necessary way. Consequently, these necessary

¹⁰ Though Rickert talks of two different *interests*, this should not be interpreted as a pragmatic perspective on knowledge, as in (Beiser 2012, 400). The distinction between two *interests* is a direct consequence of Rickert’s transcendental perspective on the possibilities for conceptual transformation of the manifold.

classifications overcome the extensive inestimability, because they are unconditionally and generally valid over the entire extensive manifold (Rickert 1929, 57). Their validity is not limited to what can be abstracted from the available empirical data about objects in space and time, as an empiricist or positivist position would imply. The necessity of the classifications implies that they are not relative to a specific point of empirical inquiry. They are unconditionally valid. Thus, they have to overcome the randomness of conceptual abstraction from a finite set of arrangements (Rickert 1929, 61). According to Rickert classifications that are necessary, can be called natural laws. They are not abstractions from empirical experience, because “they have a validity over objects or events that have never been experienced, or can ever be experienced” (Rickert 1929, 63). The ultimate goal for the natural sciences is to produce a natural law that has this kind of validity in the highest possible degree. Such law would contain every possible necessary extensive relation between objects in reality. This ultimate law is “the endpoint of natural scientific knowledge from a purely logical point of view” (Rickert 1929, 62).

Rickert relates his distinction between classifications and natural laws to the distinction between scientific description and scientific explanation. He believes that there are two ways to think about the term “explanation” [Erklärung]. On the one hand, one can make some phenomenon understandable. This merely implies that one uses concepts to systematize objects or events that one has experienced. In this sense, explaining and describing are cognitively similar (Rickert 1929, 106). On the other hand, there is also the cognitive aim of subsuming an event under a natural law: this subsumption shows that an event was necessary and could not have been different. Rickert thinks this aim of science is an extra-empirical demand of science which is required to attain concepts with an unconditional necessity (Rickert 1929, 108).¹¹

For the historical sciences the aim is similar to the natural sciences. They have to produce concepts that have a universal validity, but their concepts have to overcome the intensive inestimability of the intuitive manifold. Historical concepts cannot generalize over and abstract from the specific properties of their objects. This would disrupt the specific historical interest into the individuality [Eigenart] of objects. Rickert’s favorite example to illustrate the historical interest into individuality is the Kohinoor diamond. One can understand this diamond as just another piece of carbon, as an exemplar of a general concept. However, if one were to carve the diamond in many pieces, it would no longer be the Kohinoor diamond, even though the pieces would still be carbon. Some objects or events are indivisible and unique (Rickert 1929, 315). An object can be considered unique, according to Rickert, once it is related to a value. Some of the relations between objects and values, make objects unique for an individual person or a specific group, e.g. the value of a

¹¹ To understand Rickert’s view on scientific explanation, consider the case of a flagpole. On Rickert’s account, one can explain the length of the flagpole given the length of its shadow, only if both are related to one another through an unconditionally valid law.

specific cat in the life of a person makes that cat unique for that person. According to Rickert, the relation between a value and an object [Wertbeziehung] is the specific conceptual structure for the historical sciences. This relation orders individual objects together under a value, but this order does not disrupt or abstract from the specific, individual nature of the objects (Rickert 1929, 279). E.g. the central value that an art collector attaches to his unique pieces of art, identifies the specificity of these pieces of art, but at the same time this value also unifies all the various pieces under the specific value of the art collector. Through the order that a value-relation introduces, it overcomes the intensive inestimability of the empirical manifold: one can now see how several individual objects or events are unique. E.g. one could investigate everything that a historical person, like an important king, did. But that would be an impossible task. However, not everything that a king did in his life, is relevant to determine what makes this king unique in the historical development of a society. Relating elements of the life of this king to a central value in the development of his society, allows the historian to select which aspects and properties of the king are relevant without abstracting from what makes this king unique (Rickert 1929, 295).

In general man has two perspectives on objects in the world: “[a man] is concerned with some objects only insofar as they are instances of general concepts. On the other hand, many objects will be important for the person who values precisely because of their uniqueness. For him, therefore, they are necessarily indivisible or unified individuals” (Rickert [1902] 1986, 86, 1929, 319). During the preliminary stages of natural scientific concept formation, these sciences only classify a finite set of objects in a conceptual scheme. However, the ideal goal of these sciences is the production of general concepts in the form of natural laws that are necessary and universally valid over all objects extended in space and time. The historical sciences similarly first relate objects and events from the past to certain values in a society of the past. However, in order to achieve objective, scientific historical knowledge, these value-relations have to acquire unconditional validity (Rickert 1929, 358). Consequently, the historical sciences, for Rickert, also ultimately produce universally valid value-relations that individuate all uniquely necessary objects. The historical sciences can only achieve this, if they have an insight into the universal values, independent of particular valuations that people have made in the past (Rickert 1929, 357). Rickert realizes that the assumption of universal values is not evident, and cannot be justified through empirical inquiries. However, he notes that skepticism concerning the possibility to produce unconditional, universal laws in the natural sciences has often been considered as “a rather superfluous epistemological speculation” (Rickert 1929, 359). Thus, Rickert believes the ideal of acquiring universal values is at least equally plausible as the ideal of natural laws,

both ideals being purely logical endpoints of the two possible conceptual transformations of the intuitive manifold.¹²

Rickert's meticulous argument for the logical distinction between the natural and the historical sciences revolves around the logical distinction that he draws between natural laws and value relations. While both conceptual structures ultimately aim to show how objects are necessarily related to one another, their logical structure is mutually exclusive. Natural laws abstract from the individual properties, while value-relations determine which properties are unique to objects. Rickert's logical distinction is one of the most advanced attempt to articulate a logical split between the natural and historical sciences. It is an exemplar of ZER, and as we will see below, it also makes up the unmentioned logical position against which Hempel argues in his 1942 contribution.

2.2 Laws and Explanation in Early Logical Empiricism

There still remains a second assumption from Hempel's paper to be investigated, namely the understanding of scientific laws and general concepts that also forms its background. In the logical empiricist circles around the young Carl Hempel, philosophers believed that explanation was not a useful concept in an analysis of science. Looking back on the history of the Unity of Science movement, Philip Frank identified three anti-explanatory philosophers as the intellectual predecessors of Viennese scientific philosophy: Mach, Poincaré and Duhem. On Frank's account Mach correctly equated explanation to description, against the idea that physics offers mechanistic explanations (Frank 1949, 6).¹³ He also quoted the following insight of Duhem as an inspiration to the circle: "A physical theory is not an explanation. It is a system of mathematical propositions, deduced from a

¹² In discussions of Rickert's position, it is often noted that Rickert's whole ideal to defend the objectivity of historical knowledge failed, since Rickert could not establish the universal and necessary status of the values behind historical inquiry (Beiser 2012, 441; Iggers 1983, 159). In light of Rickert's own discussion of natural science, this evaluation could also be turned around: it is a mistake to desire an empirical ground for universal principles. Rickert's anti-empiricism resulted in a philosophical commitment to necessary principles (laws or values) that could never result from mere empirical inquiries. This commitment was defended by Rickert's particular transcendental philosophy. One should not evaluate Rickert's transcendental investigations on the grounds of an empiricist ideal of justification.

¹³ Frank's historical narrative is not neutral in comparison to the philosophical position of all the members of the broad logical-empiricist movement: it was mainly written from the viewpoint of the first Vienna Circle around Hans Hahn, Otto Neurath and Frank himself (Uebel 2003). However, as will be shown below, Carnap and Schlick mainly agreed with the anti-explanatory view on science that Frank highlighted in his narrative. In Chapter 3.7 I will show that Neurath identified Frank as the only philosopher who had migrated to the United States and who properly understood the relevance of historical-pragmatic grounds for the anti-explanatory viewpoint.

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). Within this Mach-Duhem understanding of scientific knowledge, explanation is not taken as a goal of scientific inquiry that is distinct from description. Scientific theories are understood as systematized descriptions of our experience of the world, or of our observational facts. Theories, often expressed through mathematical laws, "replace experience" as an economy performed by the mind (Mach [1893] 1974, 586–87). If one believes that scientific theories can achieve insight beyond this economical systematization and take hold of the reality underlying the phenomena, then one has a wrong understanding of what a scientific theory is. Both Duhem and Mach took this to be a general lesson that they could draw from the history of science. On their views, an understanding of scientific theories and concepts could not be achieved through philosophical reasoning in abstraction from the actual historical developments of science. Only by carefully discussing how scientific theories and concepts became accepted throughout history, could one defend a specific logical understanding of science. Here, Windelband's second stake, how to use the history of knowledge in an analysis of knowledge, again plays a crucial role. Mach summarizes the historical development of dynamics in the following way:

We can determine the true value and significance of these principles and concepts [of mechanics] only by the investigation of their historical origin. In this it appears unmistakable at times, that accidental circumstances have given to the course of their development a peculiar direction, which under other conditions might have been very different. (Mach [1893] 1974, 308)

Consequently, the acceptance of principles and concepts is contingent on the specific historical development of scientific inquiries. Had the observations developed in a different sequence, the accepted theoretical hypotheses could have been very different. A historical point of view, for Mach, "brings new possibilities before us, by showing that which exists to be in great measure conventional and accidental" (Mach [1893] 1974, 316). Duhem also believed that a history of scientific theories is crucial to understand the scope and aim of these theories. His argument against scientific explanation as an aim of theories, hinges on his historical analysis of science.

If we want to prove that the search for explanations is a truly fruitful method in physics, it is not enough to show that a goodly number of theories has been created by thinkers who strove for such explanations; we have to prove that the search for explanations is indeed the Ariadne's thread which has led them through the confusion of experimental laws and has allowed them to draw the plan of this labyrinth. Now it is not only impossible to give this proof, but, as we shall see, even a superficial study of the history of physics provides abundant arguments to the contrary. (Duhem [1906] 1991, 32)

Duhem gives the example of Descartes' mathematical laws of the refraction of light. He believes that Descartes' laws were not the offspring of Descartes' explanation of light phenomena (Duhem [1906] 1991, 34). The explanations of light phenomena that have been offered throughout the history of optics, according to Duhem, did not matter for the advancement of the field: when scientists as Huygens, Newton or Laplace disputed over explanations of light phenomena, they did not acquire a better grasp on the available light phenomena. However, the representative part of the theory of light kept on growing by expanding the set of light phenomena that could be covered by the mathematical representations in the theory. Because the mathematical representations appear to be only a random classification of available phenomenon, Duhem distinguished between natural and unnatural classifications. A classification is natural, if it is capable of anticipating experimental laws that had not yet been observed (Duhem [1906] 1991, 30). Which mathematical representations are natural, can thus only become clear in hindsight, from a historical point of view (Coko 2015, 79). The explanatory part of a theory is considered by Duhem to be entirely obsolete.

What is lasting and fruitful in these is the logical work through which they have succeeded in classifying naturally a great number of laws by deducing them from a few principles; what is perishable and sterile is the labor undertaken to explain these principles in order to attach them to assumptions concerning the realities hiding underneath sensible appearances. (Duhem [1906] 1991, 38)

Advancements in science have always occurred whenever scientists quit speculating about the realities behind the phenomena of their investigation. Philip Frank would later in his career discuss this Mach-Duhem historical dialectic of scientific theories¹⁴. According to Frank the explanatory speculations of many physicists did not prove robust against newly discovered experimental facts. Consequently, a physical law should always be understood in relation to the set of experimental facts at a specific point in time.

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", the anti-explanatory tradition takes this aspect of their theory to be the philosophical, also called metaphysical, baggage of the theory, which is obsolete, since the explanations serve certain philosophical concerns about the ultimate reality of nature and are not related to the systematization of experience. Rickert's specification that scientific explanations should only contain natural

¹⁴ Alan Richardson has also pointed out that Frank's late philosophy of science should be understood from within a Machian setting (Richardson 2012, 12).

laws that are unconditionally valid, even for those objects that can never be observed, is a good example of the introduction of a philosophical norm onto scientific inquiry that the anti-explanatory tradition rejected. For the Mach-Duhem tradition, such a norm goes beyond the boundaries that are given by the historical developments of science.

Within the anti-explanatory tradition, “explanation” [Erklärung] as a term can still refer to an aspect of scientific theories, but only pertaining to how theories systematize experience through mathematical laws. Philosophers in the logical empiricist movement broadly conceived also use the term “explanation” in that sense. Moritz Schlick maintained that “the explanation of nature means a description of nature by means of laws” (Schlick 1953, 473). This exhausts all cognitive goals of science: “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 similarly 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). Relating explanation to the search for causes or to a disclosure of the true inner essence of the world, is according to von Mises a mistake. Science is only “a description of relations, of interconnections between phenomena” (Von Mises 1968, 138). Rudolf Carnap also discussed the term “explanation” in this sense: it is the deduction of a singular, scientific sentence from other singular, scientific sentences with the help of a law (Carnap 1931, 463). Thus, for Carnap, explanations are only connections between singular, protocol statements through laws: the laws systematize scientific sentences that have a foundation in protocol statements. Consequently, it was seemingly common among early logical empiricists to assume that explanation was not an aim of science distinct from description. The term ‘explanation’ was consistently used to denote the systematization of experience/protocol statements through laws. In contrast to Mach and Duhem, logical empiricists did not even extensively argue for their views on explanation. It played no important role in their analysis of science, that is until Hempel’s 1948 paper.

This anti-explanatory tradition is also the background of Hempel’s 1942 paper. Hempel’s challenge in the paper is to defend the unity of science by incorporating the epistemology of history within an understanding of science that can be applied to the natural sciences as well. To that end, Hempel operates with a notion of scientific laws that is in line with the anti-explanatory understanding of science that one finds in Mach or Duhem: scientific concepts allow the formulation of general laws that enable the systematization of experience. General concepts which can cover multiple individual events are developed in the sciences. As Mach put it: “description is only possible of events that constantly recur, or of events that are made up of component parts that constantly recur” (Mach [1893] 1974, 6). There can be no descriptive account of an individual event that is unrelated to other elements of experience. From this it follows that any subject-matter of science can only be accounted for through general concepts that unite various individual events under them. Hempel’s argument in the 1942 paper relies on this assumption in §1 and §2: a universal

hypothesis or law is defined as a regularity between *kinds* of events, and whatever history as a science does, is concerned with *kinds* of events (Hempel 1942, 35–36). In section 2.1 Hempel assumes that "the main function of general laws in the natural sciences is to connect events in patterns which are usually referred to as explanation and prediction". Such an 'explanation' of the occurrence of an event is usually thought of as an indication of the causes. But, on Hempel's account, indicating causes should be understood solely as the representation of a regular connection between kinds of events through universal hypotheses.¹⁵ Thus, the anti-explanatory framework remains the background for Hempel's reasoning: the sciences only systematize events in lawful patterns. This is also in line with an earlier definition of the term 'explanation' in a 1936 book of Hempel that was co-authored by Paul Oppenheim, *The concept of Type in Light of the New Logic* [*Der Typusbegriff in Lichte der neuen Logik*]. There, Hempel and Oppenheim had introduced the term explanation [Erklärung] as "the specification of laws that connect empirical data in a determined way" (Hempel and Oppenheim 1936, 1).¹⁶ Such an understanding of the term 'explanation' is again indicative of the anti-explanatory framework of the Mach-Duhem view: science only represents experience through laws. It is an economy of the mind that systematically classifies experience. In that sense, Hempel's 1942 paper remained entirely within the accepted view on explanation that was common among logical empiricist philosophers.

2.3 Reactions to ZER in Logical Empiricism Before 1942

So far, I have argued that Hempel's 1942 paper defends the view that general laws perform functions similar to those in the historical sciences, against the view defended by Windelband and Rickert that the historical sciences cannot operate with general concepts or laws. I have also shown that Hempel's defense relies on a specific understanding of what a general law is, namely a systematization of experience. This is in line with the anti-explanatory view on scientific theories that was widely accepted by logical-empiricist

¹⁵ Both in the 1942 and 1948 paper Hempel could be interpreted as defending a regularity-account of causation, since Hempel replaces all references to causation with references to general laws that express regularities between kinds of events. This interpretation was already taken up in (Nancy Cartwright 2004). However, neither the 1942 nor the 1948 paper discuss causal relations. For the anti-explanatory tradition, the opposition between description and explanation as distinct aims of science was the central problem. Even though causality was often related to this problem, there is no easy interpretation of the arguments of all these philosophers as simply regularity theories of causation.

¹⁶ "die sog. Erklärung, genauer gesagt für die Aufstellung von Gesetzen, die empirische Daten bestimmter Art miteinander verknüpfen."

philosophers. This view was initially developed by Ernst Mach and Pierre Duhem, who defended it, not through a philosophical argument, but through a historical investigation of the development of scientific theories. Hempel's 1942 paper was, however, not the first logical-empiricist paper to counter the ZER argument. In fact, many logical-empiricist philosophers were engaged with ZER. Below, I focus first on Rudolf Carnap's response to ZER between the publication of his *Aufbau* and his later work in the 1930s (see 2.3.1), and I compare Carnap's views to Otto Neurath's vehement attack on ZER (see 2.3.2). Both Carnap's and Neurath's views will prove to be different from Hempel's eventual position in a significant way. Second, I show how a discussion of ZER was also central in the correspondence between various philosophers of the logical-empiricist network, and that Hempel's initial interest for ZER came about within these discussions (see 2.4).

2.3.1 Carnap and the Geisteswissenschaften

In *Der logische Aufbau der Welt*, Carnap explicitly aims to position himself within the contemporary debates about the epistemic status of the historical sciences.¹⁷ Carnap's concern for the Geisteswissenschaften in the *Aufbau* is related to the aim of the book, which is to show how a limited set of basic concepts and a logical theory of relations can be used to constitute all the concepts of the different sciences within one "constitutional system". Carnap intends to show that despite the differences in objects, methods and concepts, the various branches of the sciences can be united in "a unified system of concepts to overcome the separation of unified science into unrelated special sciences" (Carnap [1928] 1998, sec. 2).¹⁸ Given this aim, Carnap incorporates not only the natural sciences, but also psychology and what he calls the "Geisteswissenschaften" in his discussion.¹⁹ These sciences study

¹⁷ Translations of Carnap's *Aufbau* are taken from (Carnap [1928] 2003).

¹⁸ Recently, Richard Creath has argued that Carnap's arguments for the Unity of Science should be interpreted as arguments against ZER, what Creath called the Dyadic Tradition of Windelband and Rickert (Creath 2017). This chapter develops such interpretation to a greater extent and shows how Carnap's arguments against ZER changed in the 1930s.

¹⁹ 'Cultural sciences' will be used as a term that refers to a wide set of disciplines containing historiography, linguistics, political science, anthropology, literary studies, art studies and archeology. Carnap consistently uses Dilthey's term "Geisteswissenschaften" to refer to these disciplines. Neo-Kantians like Rickert and Cassirer, use the term "Kulturwissenschaften" (Makkreel 2010). For none of these authors was there a clear-cut difference between social sciences and humanities. So I will not use this contemporary terminology. Where I will speak of these sciences in abstraction from their epistemological analysis, I will use the term "cultural sciences", which is not a common English term itself. I will not use the term as a translation for Carnap's "Geisteswissenschaften", because it would abstract from the very reason why Carnap used this term, and not the term "Kulturwissenschaften". Carnap calls the objects of these sciences the "geistige Gegenstände". I chose to translate "geistige Gegenstand" as "cultural object", because mental objects in English refer to a psychological phenomenon, which is precisely not what Carnap means with "geistige Gegenstand". Carnap himself realizes that

cultural [kulturellen], historical and sociological objects (Carnap [1928] 1998, sec. 23). Carnap gives a wide range of examples of these objects: courtesy as a social custom (sec. 24), expressionism as an art form (sec. 31), a state as a political organization (sec. 4, 30, 151), religion as a group custom (sec. 55) and the Trojan War as a historical event (sec. 175). These kinds of objects are discussed as possible objects of scientific knowledge in a considerable number of sections (sec. 12, 23, 24, 55, 56, 150, 151). Carnap introduces the concern for these objects of science in sec. 12 of the *Aufbau*.

Recently (in connection with ideas of Dilthey, Windelband, Rickert), a "logic of individuality" has repeatedly been demanded; what is desired here is a method which allows a conceptual comprehension of, and does justice to, the peculiarity of individual entities, and which does not attempt to grasp this peculiarity through inclusion in narrower and narrower classes. Such a method would be of great importance for individual psychology and for all cultural sciences, especially history. (Cf., for example, Freyer [Obj. Geist] 108) I merely wish to mention in passing that the concept of structure as it occurs in the theory of relations would form a suitable basis for such a method. The method would have to be developed through adaptation of the tools of relation theory to the specific area in question. Cf. also Cassirer's theory of relational concepts [Substanzbegr.] esp. 299, and the application of the theory of relations (but not yet to cultural objects) in Carnap [Logistik] Part 11.²⁰ (Carnap [1928] 2003, sec. 12)

In this passage, Carnap seems to claim two things. First, Rickert's demand for a logic that focuses on an individuating conceptual understanding, is a valid demand. Second, the structural understanding of scientific concepts that Carnap himself presents in the *Aufbau* is capable of fulfilling Rickert's demands. These claims seem incompatible: Carnap's structuralist notion of concepts is meant to be applicable across the sciences, and should not endorse Rickert's logical theory of the cultural sciences, which separates historical and natural scientific concept formation. Does Carnap imply that a separate *logic* of individuality is really necessary to understand the historical sciences? By understanding the apparent contradiction entailed by this passage, one understands the position about the cultural sciences that Carnap holds.

To that end, it is crucial to understand how Carnap thinks about the particular historical contribution of philosophers like Dilthey, Windelband and Rickert to the scientific status of

his terminology could confuse his readers (Carnap [1928] 1998, sec. 23). Carnap probably stuck to the Diltheyian terminology, because he was most familiar with Dilthey's work.

²⁰ This passage has already often been quoted, even though it is only part of Carnap's reference to the literature. The passage has especially been used to show how Carnap's constitutional theory of the *Aufbau* is related to a Neokantian philosophical project (Friedman 2000b; Richardson 1998, 38–39). I mainly discuss the passage to show how Carnap relates his own work to philosophers who had argued for ZER.

the historical sciences. Carnap often lauds these philosophers for their particular historical importance.

The philosophy of the nineteenth century did not pay sufficient attention to the fact that the cultural objects form an autonomous type.²¹ The reason for this is that epistemological and logical investigations tended to confine their attention predominately to physics and psychology as paradigmatic subject matter areas. Only the more recent philosophy of history (since Dilthey) has called attention to the methodological and object-theoretical peculiarity [Eigenart] of the area of the Geisteswissenschaften. (Carnap [1928] 2003, sec. 23)

In the meantime [since the 19th century], other objects (especially the cultural objects, the biological objects, and the values) have been recognized as independent, even though the equality of their status with that of the physical and the psychological objects is at the moment still debated. (Carnap [1928] 2003, sec.162)

Carnap agrees with Windelband's assessment that scientific developments in the 19th century have shown that historical and cultural subject matters can be a part of the scientific inquiry, and, consequently, that these subject matters should also be discussed in the epistemological and logical investigation of science. These subject matters have a certain peculiarity [Eigenart]. They cannot simply be reduced to physics or psychology, but should be recognized as "autonomous".

In those sections of the *Aufbau* that discuss the constitution of the Geisteswissenschaften, the autonomy and peculiarity of the cultural object-spheres is repeatedly emphasized. In sec. 56 Carnap states that cultural objects "are not composed out of psychological states", rather they belong to a completely different object sphere within the constitutional system. This is repeated in sec. 151: "the cultural objects are of a completely different object level than the psychological or physical". Propositions containing cultural objects cannot be meaningfully [mit Sinn] transformed into propositions containing other kinds of objects (Carnap [1928] 1998, sec. 23). Thanks to philosophers like Dilthey, Windelband and Rickert the autonomy and peculiarity of these object levels can finally be recognized. These philosophers have had this particular historical importance. However, this did not imply according to Carnap that Rickert's *logical* theory of individualizing concepts should be taken over as well. On the contrary, Carnap believed that a functionalist notion of a concept can be used to dissolve Rickert's worries about the interest into individuality which had engendered much unnecessary philosophical controversy over the Geisteswissenschaften. Carnap's resulting position thus merges the logical unity of all scientific concepts with

²¹ One could also argue that 19th century German philosophy was heavily focused on historiography (Beiser 2012, chap. 1). Carnap seems to believe that predominantly Dilthey's work of the late 19th century only brought cultural objects under the attention of philosophers.

autonomy at the level of disciplines, including the various Geisteswissenschaften. In the *Aufbau*, and in the remainder of his career Carnap would consistently deny that a logic specific to the Geisteswissenschaften could be given: contrary to Rickert's explicit belief, there was nothing interesting to say about cultural concepts specifically from a *logical* point of view. At the same time, Carnap also consistently upheld the idea that the cultural sciences should be incorporated in the Unity of Science, and consequently that their concepts required some level of attention, though only from a *practical* point of view. Below I first focus on Carnap's position that the autonomy of the cultural objects can be maintained within the overall logical framework of the *Aufbau*. Second, I show how Carnap incorporates cultural concepts in the preliminary constitutional system that he proposes, and how he reasons about this incorporation as a practical, non-logical enterprise.

2.3.1.1 Constitution Theory and Logic of the Geisteswissenschaften

As we have seen above, Carnap agrees with Rickert in section 12 that concepts in the cultural sciences should not be analyzed as generic classes. As a reference to a similar position, he points to a specific passage in Hans Freyer's *Theorie des objektiven Geistes*. Freyer was an influential interwar sociologist inspired by Dilthey's works. He held positions in Kiel and Leipzig, and he became a representative of right-wing socialist reform and a supporter of the national socialist movement. Carnap personally knew Freyer from the Dilthey school around Herman Nohl in Jena, and he was certainly acquainted with Freyer's work, as his specific reference in sec. 12 testifies (Damböck 2012, 75–76; Tuboly 2018; Damböck 2017, 181–83). In the paragraph that Carnap refers to, called *Towards a logic of individual unities* [Zur Logik individueller Einheiten], Freyer laments the lack of a non-Aristotelian logical understanding of the concepts of the Geisteswissenschaften: “In German idealism, romanticism and in contemporary German philosophy one can find many attempts at this new logic, but the actual Aristotelian act has not ended yet. Its demise is, however, necessary” (Freyer 1923, 108). Such a request for a new logic was grist to the mill for Carnap, who was on the forefront of the development of new symbolic logic himself, and specifically of its application to the analysis of science (Damböck 2017, 189).

As quoted above, Carnap believed Rickert's and Freyer's problem - namely to logically account for the uniqueness of an object - could be solved through the introduction of “the concept of structure as it occurs in the theory of relations”. Subsequently, he refers to a specific passage in Cassirer's *Substance and Function* (henceforth *S&F*): the passage appears in a chapter where Cassirer criticizes Rickert's theory of the concept in the natural sciences.²² Cassirer argues that Rickert's notion of “concept” in the natural sciences is incapable of “grasping the particular as particular”, since this concept is understood as an abstraction aimed at uniting only what is common in reality (Cassirer [1910] 2004, 222).

²² This was also noted in (Creath 2017, 10).

Since Rickert understands the universality of a concept as abstraction, the particular is thus lost once subsumed under the concept. Cassirer, however, wants to understand concepts in the natural sciences as a definite law of relations that unites the various individuals in a functional relation, and Cassirer argues that an individual object can only be recognized as an individual if it has a place within the structure of relations (Cassirer [1910] 2004, 225). This leads Cassirer to reject Rickert's logical problem of individuality. The passage in *S&F* to which Carnap referred, also contained a page-long footnote reflecting on the nature of the purely individual historical concept and the problem of individuality. It is the only passage in *S&F* where Cassirer makes claims about the concept formation in the cultural sciences.

An essential task of the historical concept is the insertion of the individual into an inclusive systematic connection, such as has constantly established itself more distinctly as the real goal of the scientific construction of concepts. This "insertion" can occur under different points of view and according to different motives; nevertheless it has common logical features, which can be defined and isolated as the essence of "the concept". (Cassirer [1910] 2004, 228)

Cassirer's point in this long footnote is a critique of any strong conceptual differentiation between the natural and the cultural sciences, directed against the proposals of Windelband or Rickert. In contrast to his later work Cassirer still believes that one logical analysis of the scientific concept, namely what he calls the functional concept, can incorporate both types of sciences. Logically, Cassirer believes, concepts from both sciences are similar, even though there may be different 'motives'.²³ Carnap understands his project in the *Aufbau* as a way to spell out such a theory of the functional concept with the aid of the modern logic of relations. For Carnap, this should also include an analysis of concepts of the *Geisteswissenschaften*. Similar to Cassirer's position in *S&F*, Carnap argues that one logical analysis of the scientific concept could incorporate both the natural sciences and the cultural sciences. Specifically, Carnap believes that his use of purely structural definite descriptions of objects in the constitutional system of the *Aufbau* would allow him to determine the individual within a structured whole of relations. He can thus dissolve Rickert's and Freyer's quest for a *logic* of individuality specific to the cultural science: such a logic would serve no purpose.

In order to defend that the *Geisteswissenschaften* still form an autonomous field, even though there is no logical structure particular to them, Carnap discusses "the epistemic value" [Erkenntniswert] of a level of a constitutional system. The notion of epistemic value is introduced in sec. 50 to characterize a fundamental feature of any constitutional level in any possible constitutional system. However, Carnap only mentions the relevance of this

²³ In *S&F* Cassirer never explained what he meant by this. For a thorough discussion of Cassirer's early criticism on Windelband and Rickert, and the later developments of his views, see (Birkeland and Nilsen 2002).

notion when he talks about cultural objects. Quite clearly, he recognizes that the problem of autonomy is most urgent for the cultural sciences; Carnap accepts the Windelbandian or Diltheyian insight that the historical sciences should be considered as an element of the sciences equal to physics, even though this was still unnecessarily under debate in Carnap's days.

A constitutional system is supposed to constitute various concepts from a limited set of ground concepts (Carnap [1928] 1998, sec. 1). In the *Aufbau*, Carnap introduces a constitutional theory that should be applicable to any constitutional system. The important notion of epistemic value that Carnap uses to uphold the autonomy of the *Geisteswissenschaften* is an aspect of this theory. Using the theory, Carnap also proposes a specific constitutional system that should be capable of yielding all scientific concepts and that resembles the constitution of the world by a traditional epistemic subject. This system has elementary experiences as ground objects and one ground relation that holds over these objects (recollection of similarity). Carnap does not exclude the possibility of other systems, e.g. with a physical basis (Carnap [1928] 1998, sec. 62), or even a cultural basis (Carnap [1928] 1998, sec. 56). In the end, Carnap's proposed system only has a secondary importance. It is mainly intended to capture the potential strength of constitution theory.

The constitution theory of the *Aufbau* analyzes every sentence as a propositional function stripped of all non-logical constants. Certain names of objects [Gegenstände] can be used to complete the propositional function, yielding true or false propositions. "Object" [Gegenstand] is thus used in the *Aufbau* in a wide sense for any possible argument of a propositional function (Carnap [1928] 1998, sec. 1 & 5). Those objects that can be used to complete the same type of propositional function are "sphererelated objects" and a class of all objects which are sphererelated to each other is called an "objectsphere" [Gegenstandssphäre] (Carnap [1928] 1998, sec. 29). The objectspheres form the "levels" [Arten] of the constitutional system and are related to each other through constitutional definitions (Carnap [1928] 1998, sec. 41). These definitions state how propositional functions containing an object of a specific level can be transformed into propositional functions containing other, already constituted objects within the system, while preserving the truth value of the relevant propositions.

Central to this idea of constitution is the notion of the "quasi-object". Every sign of an object of one of the levels refers to a "quasi-object". The quasi-object is, on the one hand, an object for the propositional functions of its own sphere. On the other hand the same quasi-object is a class or a relation that has validity over the objects on the lower level that are used in its own constitution (Carnap [1928] 1998, sec. 42). Every object of the system can be both a concept constituted out of lower-level objects and itself an object constituting higher-level objects (with the exception of the ground objects). Therefore, all the objects of the system other than the ground objects are called "quasi-objects" (Carnap [1928] 1998, sec. 27). They are only relations between or classes of other quasi-objects. The only objects within the logical constitution system are the ground objects (elementary experiences):

these objects are not themselves constituted. Therefore the objects of science have only those objects as their true logical reference (“logische Bedeutung”) (Carnap [1928] 1998, sec. 41). However, these objects do not epistemically validate the objective nature of scientific concepts. The elementary experiences are purely subjective and prevent an intersubjective system (Carnap [1928] 1998, sec. 66). Only the structure of relations which is posited over them yields objective content.²⁴

This has a peculiar result: science does not talk about the ground objects. “In its practical procedure science creates propositions mainly in the form of propositions about the constitutive structure, not about the ground objects. And these structures belong to different constitutional levels, which belong to different spheres.” (Carnap [1928] 1998, sec. 41) One cannot replace a quasi-object in a propositional function with a quasi-object from a different level of the system. One can only transform the propositional function, but then one is no longer talking about the previous objects. This allows Carnap to give his hierarchy of quasi-objects a specific epistemological meaning. Because of this feature of the quasi-object, science can be conceived of as a unified multiplicity of autonomous object spheres. Science is a structure of various autonomous object spheres layered over each other. The object spheres are constituted out of a single class of objects, namely those objects that can have a position as argument within a specific type of propositional functions (Carnap [1928] 1998, sec. 41). Carnap introduces this idea very early in the *Aufbau* through the example of the “state”, a political concept belonging to the higher levels of the cultural domain:

The object state, for example, will have to be constructed in this constitutional system out of psychological processes, but it should by no means be thought of as a sum of psychological processes. We shall distinguish between a whole and a logical complex. The whole is composed of its elements; they are its parts. An independent logical complex does not have this relation to its elements, but rather, it is characterized by the fact that all statements about it can be transformed into statements about its elements. (Carnap [1928] 2003, sec. 4)

Thus, the complex object ‘state’ is not made up of psychological processes (it’s not a whole), but any statement about this object can be transformed into statements about psychological processes. Even though this characteristic is common to all the constitutional levels, Carnap refers to the difference between the compound whole and the logical complex almost exclusively in the context of the *Geisteswissenschaften*. In sec. 23, where he first introduces the *Geisteswissenschaften*, he states that “the cultural objects are not composed of psychological (much less the physical) objects, but they are of a completely different object-type”. In sec. 56 he almost verbatim repeats the same thing. In sec. 151 Carnap wants

²⁴ In general I follow Michael Friedman and Alan Richardson’s anti-foundationalist reading of Carnap’s philosophical project behind constitution theory and its relation to neokantian philosophy (Richardson 1998; Friedman 1999a, 1999b, 2000b).

to “emphatically emphasize that the cultural objects are not psychologized”, because they are constituted through a relation over certain psychological objects of the system. “The cultural objects belong to a higher sphere within the system.”

Although constitution does not imply the reduction of objects into other objects, it does entail the possibility of transformation of every sentence containing cultural objects to a sentence containing physical or psychological objects. Throughout the transformation of sentences something has to be preserved. This is the extension (truth value) and Carnap calls the truth value “the logical value” (Carnap [1928] 1998, sec. 32;50). The assigned truth values for the sentences before and after transformation will remain the same. Because the object spheres are autonomous, something also has to be lost through transformation. This is the epistemic value [Erkenntniswert] of a sentence. “This is the representational meaning of a sentence or its worth for knowledge” (Carnap [1928] 1998, sec. 50). Through constitutional transformation of a propositional function the epistemic value of a sentence can be lost (Carnap [1928] 1998, sec. 50). “The constitutional method only concerns the logical value, not the epistemic value; it is purely logical, not psychological” (Carnap [1928] 1998, sec. 50).

In the section on identity Carnap refers back to the difference between the two values of a sentence: even though “the birthday of Sir A” and “22 March 1832” have the same reference or logical value, they do not share the same sense or epistemic value. While the constitutional system guarantees that the logical value remains stable throughout every transformation of scientific sentences, the descriptions determined by the constitutional definitions “play an important role for scientific knowledge” (Carnap [1928] 1998, sec. 159). What does this “importance” mean? Scientific questions are meaningful because “the signs in an answer are different from the signs in its question” (Carnap [1928] 1998, sec. 159). Tellingly, the best illustration of the importance of constitutional definitions that introduce new signs and higher object levels can be found in a section on the autonomy of the cultural sphere; the domain for which there already existed sophisticated and controversial debates on its relation to concepts of the natural sciences or psychology.

In sec. 56 Carnap uses the difference between the two values of a sentence when he discusses the possible directions of constitutional rules for the cultural objects. “The meaning [Sinn] of the sentences about the cultural objects cannot be rendered within sentences about psychological objects (this is sometimes the case, but not always).” If the cultural complex of 'greeting' were composed out of psychological thoughts, then everything which can be said of 'greeting' could be said of thoughts. But this is not the case. 'Greeting' as a cultural object is characteristic of a larger social group of people, the psychological thoughts cannot be a characteristic of such a group, since they are by definition individual. It is the logical complex of greeting that enables one to speak over and beyond the merely psychological occurrences, even though the complex is constituted by certain psychological objects of the constitutional system. This constitution merely entails “the possibility of transformation in constitutional meaning, being the possibility of

a transformational rule, through which the logical value remains unchanged, but not the epistemic value” (Carnap [1928] 1998, sec. 56).

'Greeting' as an object of the cultural sphere can only be used as an argument in propositional functions of that sphere, but not in functions of lower levels. Nor can any of the objects from lower spheres be used as arguments in propositional functions about the cultural domain. Only the logical value of the sentences is maintained in transformation. The hierarchy of types within the constitutional system guarantees that the objects of every sphere can only be used as arguments of the propositional function of that sphere. This, however, entails that every scientific discipline is limited to gathering knowledge about the objects of its own sphere. Whatever knowledge it gains will, in the constitutional system, be expressed in propositions about the right sphere. The constitutional definitions entail the possibility of the transformation of sentences, which in turn entails the incorporation of the objects in a logical, intersubjective system. This constitutional transformation does not guarantee the epistemic justification of the higher concepts; they only guarantee that the concepts are embedded in a unified structure. Greeting as a social custom is not composed out of psychological events; it is only constitutionally defined through these events.

Carnap uses the difference between composition and constitution, and its related difference between the logical and epistemic value of sentences, to emphasize that cultural objects themselves are not reduced to psychological or physical objects. Only the sentences are transformed, which almost always entails a loss of epistemic value. Even though such remarks would also be possible for the psychological or physical objects vis-à-vis the elementary experiences, he specifically makes the remarks in the context of the *Geisteswissenschaften*. This is a clear sign that Carnap thinks the autonomy of the *Geisteswissenschaften* is an important aspect of his constitution theory. This again shows that Carnap has accepted the historical insight of Dilthey, Windelband and Rickert, that the cultural sciences form a scientific domain whose inquiry cannot be reduced to psychology or physics, but unlike these predecessors, Carnap did not believe there was a separate logic of the *Geisteswissenschaften*. Carnap denied any possible logical criterion to divide the sciences.

2.3.1.2 Practical Concerns about the *Geisteswissenschaften*

From the viewpoint of constitutional theory, Carnap cannot say much about the objects in the cultural sciences other than that they will form spheres in a constitutional system. However, since the *Aufbau* also initiates an investigation into a possible constitutional system that can incorporate all the sciences, Carnap gives an outline of what he takes to be a credible constitution of cultural objects in such a system. Their constitution is not performed in logical-symbolic form. Carnap focuses solely on the fundamental possibility of such a logical constitution (Carnap [1928] 1998, sec. 139). Carnap assumes that he can use the already available psychological and physical objects from lower levels of the constitutional system in order to constitute the cultural objects. For the transformation of

propositions containing cultural objects into propositions containing already constituted psychological objects, Carnap postulates a relation of manifestation [Manifestationsbeziehung]. This is the relation between a cultural object and the psychological process in which the cultural appears or manifests itself (Carnap [1928] 1998, sec. 24). Carnap uses the example of greeting twice as an illustration for this relation: the cultural custom of taking your hat off when you see someone you know, can be constituted using those psychological processes that 'manifest' that custom (Carnap [1928] 1998, sec. 24, 150). Certain psychological dispositions manifest a cultural object, like a custom, while others do not. A relation of manifestation stipulates which dispositions, volitions, etc. manifest the cultural.

Carnap also offers a second route of constitution of the cultural domain, namely the relation of documentation [Dokumentationsbeziehung]. This is the relation between a cultural object (e.g. an art movement) and its document, being an enduring, physical object in which the cultural life is petrified (e.g. the physical aspects of a painting) (Carnap [1928] 1998, sec. 24). Documents are the material witnesses [dingliche Zeugen] of the cultural. The documents of an art style for example can be paintings or sculptures. The documents of the railwaysystem can be its infrastructure and written timetables. However, an object can only become a document with the aid of a manifestation.

However, the documentation of a cultural object necessarily takes place with the aid of a manifestation. For, if a physical object is to be formed or transformed in such a way that it becomes a document, a bearer of expression [Ausdrucksträger] for the cultural object, then this requires an act of creation or transformation on the part of one or several individuals, and thus psychological occurrences in which the cultural object comes alive; these psychological occurrences are the manifestations of the cultural object. (Carnap [1928] 2003, sec. 55)

Here Carnap implies that a relation between a physical and a cultural object can only be stipulated when there are recognizable actors that use the object as if it manifests something cultural. The relation of documentation needs to show how the cultural comes alive through the merely physical based on certain mental states attributable to actors, which in turn, manifest the cultural. The physical domain does not, in itself, determine the cultural, but an actor thinks and acts through the physical so that it becomes alive. The *Aufbau* contains no further explanation of this principle. In the end, Carnap's remarks are only hints at a possible constitution. Still, it shows that Carnap was sensitive to the difficulties of transforming a physical object into something culturally meaningful.

The central notion of manifestation in Carnap's proposal stemmed from a dominant tradition of thinking about the cultural [geistige] in 19th century German philosophy. Manifestation is a relation between an expression [Ausdruck] and the cultural thing [eines Geistiges] that it manifests or expresses. The idea of a document as bearer of the expression of something cultural has its origins in Hegel's philosophy of the objective spirit: certain documents are the material patterns of human interaction in which the spirit [Geist] objectifies itself. This vocabulary of an objectification of the Geist is explicitly taken over

by Dilthey in his epistemology of the Geisteswissenschaften, but without its metaphysical, Hegelian aspects (Dilthey [1910] 1927, 7:148–50). Dilthey describes the objectifications as “manifestations of life” [Manifestationen des Lebens]. They are the realizations of the cultural in the empirical world. Every gesture, form of courtesy or work of art is related to a common structure that binds them, namely the cultural structure (Dilthey [1910] 1927, 7:146).

In a letter to Wilhelm and Elisabeth Flitner forty years after the publication of the *Aufbau* Carnap wrote that he never read anything by Dilthey, as far as he remembered (Gabriel 2004, 16–17). However, he does mention Dilthey's *Einleitung in die Geisteswissenschaften* in the bibliography of the *Aufbau*. If Carnap did not get the notion of manifestation from Dilthey himself directly, one might expect he got it from Diltheyian inspired philosophers like Herman Nohl or Hans Freyer, whom he knew personally. Manifestation is, however, not discussed in Freyer's *Theorie des objektiven Geistes*. Whether or not he actually read Dilthey, the first version of the *Aufbau* was written in an intellectual climate in which Dilthey was widely discussed (Damböck 2012, 76). Consequently, it is not strange that Carnap heavily relies on the Dilthey tradition in his discussion of the Geisteswissenschaften.

This influence from Dilthey can also be seen in Carnap's ideas about possible new constitutional relations for cultural objects. The range of possible cultural objects in the cultural domain of the *Aufbau* is extensive: engineering, economy, law, politics, language, art, science, religion, etc. (Carnap [1928] 1998, sec. 151). In order to cope with the huge amount of possible cultural objects in the constitutional system, Carnap makes a distinction between primary and secondary cultural objects. Whereas the primary objects are constituted through the available physical and psychological levels, using only relations of documentation or manifestation, the secondary objects use other cultural objects for their constitutional rules. (Carnap [1928] 1998, sec. 150). It would be hard to constitute all the different objects of the cultural domain directly through manifestations or documentations. Carnap, therefore, divides the task for the constitution of the cultural domain into two separate programs. The logic [Logik] of the Geisteswissenschaften, on the one hand, has to investigate which objects of the different fields can be constituted as primary or secondary. The investigation, on the other hand, how and which psychological objects manifest the primary cultural objects is the task of the phenomenology [Phänomenologie] of the Geisteswissenschaften (Carnap [1928] 1998, sec. 150).

Carnap's call for a phenomenology of the Geisteswissenschaften is in line with one dominant strand of the contemporary theory of his time. Dilthey had already argued that the difference between the natural sciences and the Geisteswissenschaften should be understood phenomenologically because each type of science starts from a different kind of experience. While knowledge of nature should be grounded in sense perception, the knowledge of the socio-historical is grounded in lived experience [Erlebnis] (Beiser 2012, 328). A later manifestation of the same idea is present in the second study of Cassirer's *Zur Logik der Kulturwissenschaften* (Cassirer [1942] 2011). There, Cassirer argues that the true

difference between the two forms of science can only be understood by a phenomenology of perception [Phänomonologie der Wahrnehmung] which yields two different branches of perception: Dingwahrnehmung, the perception of objects in space and time, that is, the world of things or, Ausdruckswahrnehmung, the perception of physical objects as expressions of a person (Cassirer [1942] 2011, 42). Constituting the physical as a bearer of expression is also exactly what a relation of documentation is supposed to do in Carnap's *Aufbau*.

It is important to stress that Cassirer's or Dilthey's specific use of 'phenomenology' is different from Carnap's. In the end Carnap refers to a program for a constitution of cultural objects as manifestations within the boundaries of his constitutional theory. No philosophical investigation of two strands of perception occurs in that program – as Dilthey or Cassirer would want it. All three would, however, give a similar epistemological evaluation of knowledge of the cultural: it is not reducible to the physical level. Only after the relation of manifestation logically constitutes the cultural object, is it possible to recognize certain psychological objects as manifestations of a cultural phenomenon. E.g. the psychological experience of watching an opera or watching a musical may be very similar, while they have a clear different cultural meaning. They manifest something else, which is only so given the theoretical import from the constitutive definitions of the cultural level. The psychological objects in a constitutional system have no cultural content.

The *Aufbau* does not undertake a systematic discussion of a specific methodology of the Geisteswissenschaften. Instead, the work aims to give a logical reconstruction of the conceptual structure of science, rather than of its experimental practice or empirical inquiry. *Verstehen*, which was typically understood at that time as a central method for the Geisteswissenschaften, is, however, mentioned on the side by Carnap. *Verstehen*, introduced by Dilthey, was considered a procedure for understanding the meaning of actions, texts or objects from the past. In the *Aufbau* Carnap links the procedure of *Verstehen* to the constitutional definition for the cultural objects.²⁵ Carnap first mentions the method in sec. 49 of the *Aufbau*.

In many cases, especially in the Geisteswissenschaften, when we are concerned, for example, with the stylistic character of a work of art, etc., the indicators [Kennzeichnungen] are given either very vaguely or not at all. In such a case the decision as to whether a certain state of affairs obtains is not made on the basis of rational criteria but by empathy. Such empathy decisions are justly considered scientific decisions. The justification for this rests upon the fact that either it is already possible, even though very complicated in the individual case, to produce indicators whose application does not require empathy or else that the task of finding such

²⁵ This goes against the dominant view on early logical empiricists' position on *Verstehen*. According to this view logical empiricists took the method of *Verstehen* merely as a heuristic method: an imaginative process of intuition that generates some claim for the historian. The rational justification of this claim, however, remains unrelated to the heuristic (Uebel 2010, 293–96).

indicators has been recognized as a scientific task and is considered as solvable in principle. (Carnap [1928] 2003, sec. 49)

The method of empathy (later equated with *Verstehen* (Carnap [1928] 1998, sec. 55)) is scientific, because it should always be possible to make the criteria explicit when, for example, the stylistic characters of a work of art obtain. The indicators [Kennzeichnungen] are the constitutional definitions of the cultural objects. These definitions state which physical states or psychological objects document or manifest cultural content. So while the initial recognition of a painting as an expressionist painting can be based on intuition, one should in principle always be able to rationally reconstruct this intuitive recognition. Finding a path for the constitution of the object based on the relation of manifestation or documentation, is the discursive aspect of *Verstehen*.

It is occasionally claimed that it is possible to recognize cultural objects without having to take a detour via psychological processes in which they manifest themselves or via physical documentation. But so far, such methods are not known to science and have not yet been applied. The Geisteswissenschaften recognize their objects [...] through "empathy" or *Verstehen*. But this intuitive procedure, without exception, begins with manifestations and documentations. Furthermore, it is not merely the case that intuitive understanding, or empathy, is occasioned by the recognition of the mediating psychological or physical objects, but its content is completely determined through the character of the mediating objects.

EXAMPLE. The awareness [Erfassung] of the aesthetic content of a work of art, for example a marble statue, is indeed not identical with the recognition of the sensible characteristics of the piece of marble, its shape, size, color, and material. But this awareness is not something outside of the perception, since for it no content other than the content of perception is given; more precisely: this awareness is uniquely determined through what is perceived by the senses. Thus, there exists a unique functional relation between the physical properties of the piece of marble and the aesthetic content of the work of art which is represented in this piece of marble. (Carnap [1928] 2003, sec. 55)

Grasping [Erfassung] the marble sculpture as an aesthetic art object is not independent from the constitutional definition of the art object. The constitutional definition stipulates which physical and psychological objects are manifestations of an art object. The act of *Verstehen* is similar, because it determines which physical and psychological objects manifest an aesthetic content. The implicit intuitive method thus always relies on the possibility to make the relation explicit between a cultural object and its physical or psychological expression.

Within the framework of the *Aufbau* the method of *Verstehen* is a methodological aspect of what Carnap calls the 'first' task of science: the construction of a constitutional system. This task has priority in the logical sense: it gives a full logical determination of the objects of scientific investigation (Carnap [1928] 1998, sec. 179). The necessity of this logical

investigation, however, should in no way keep science from engaging with higher level objects that have not yet been fully constituted, such as cultural objects, “if at least science does not want to abstain from those important fields which are meaningful for their practical application” (Carnap [1928] 1998, sec. 179). In the real scientific process scientists are justified in using a merely intuitive constitution of their object, as long as they also have the task of giving a full logical characterization. Carnap's call for a phenomenology of the *Geisteswissenschaften* is specifically directed towards this last task.²⁶

Again, we see a convergence between Dilthey's and Carnap's position. Carnap uses ideas from Dilthey to articulate possible constitutional rules for cultural objects within the boundaries of the constitutional theory that he had set out. This articulation, however, is only an indication to researchers in the cultural sciences. It proves to them that Carnap's constitutional theory does not exclude their subject matter from science. How to actually constitute specific cultural objects and relate these constructions to one another and to physical or psychological objects, cannot be answered by the philosopher – this is an open question for researchers. Carnap has no philosophical position about these sciences in particular: manifestation and documentation are merely indications of how one could think about the constitution of cultural objects. Carnap had used terminology and ideas from the Diltheyian framework that was known to him at the time to produce these indications.

2.3.2 Neurath's criticism of 'Geisteswissenschaften'

That the relations of manifestation and documentation are merely hints can also be seen from Carnap's discussion of the cultural sciences during the rest of his career. After the publication of the *Aufbau*, Carnap was addressed by Neurath about his account of cultural objects in the *Aufbau*. In Carnap's diary entry of 19 December 1929 he reports the following:

²⁶ At first, one might suspect that this integration of the intuitive *Verstehen* in the non-intuitive constitutional framework runs counter to Dilthey's original conception of *Verstehen*. In his *Aufbau der geschichtlichen Welt in die Geisteswissenschaften* Dilthey states that *Verstehen* is based on the relation between what expresses and that which it expresses. And this relation cannot be represented by “formulas of logical powers” [Formeln logischer Leistungen] (Dilthey [1910] 1927, 7:218). He does not, however, exclude the possibility of a logic of the *Geisteswissenschaften* [Logik der Geisteswissenschaften]. Such a logic should investigate rules that assess the possibility of general principles concerning the relation between a physical or psychological expression and the cultural that it expresses. This is, of course, exactly what Carnap's relation of manifestation is supposed to do. According to Dilthey, this logic would yield the method of *Verstehen* as a form of induction. This induction would not generate a law, but a structure that takes the individual as part of a meaningful whole (Dilthey [1910] 1927, 7:220). Thus, a non-intuitive account of *Verstehen* is also present in Dilthey's own work (Beiser 2012, 351).

With Feigl to Neurath. Neurath rants at my discussion of the “Geisteswissenschaften” in the *Aufbau*. It is too idealistic for him; he had points of attack: Dilthey was mentioned: “custom”, “state”, “manifestation”. Back in the house at one o’clock.²⁷

Neurath was not pleased that Carnap had used the theoretical terminology of Dilthey for his incorporation of the cultural sciences in the constitutional system. Carnap’s terminology, like the confusing German word “Geist” and the suspiciously metaphysical “Manifestation”, could easily be replaced with terminology that stemmed from an empiricist tradition of ideas. Since Carnap had no logical ideas particular to the cultural sciences, it was easy for him to comply with Neurath’s remarks. Carnap completely discarded Dilthey’s terminology after the *Aufbau*. In his 1930 paper “Die Alte und die Neue Logik” Carnap abandoned the use of the term “Logik der Geisteswissenschaften” and “Phänomenologie der Geisteswissenschaften” to describe specific tasks within the formation of a constitutional system. Instead he openly attacks the use of this terminology:

From the point of view of the new logic „Geisteswissenschaftliche Philosophie“ proves itself to be, not directly false, but actually logically untenable and therefore meaningless.²⁸ (Carnap 1930, 13)

One year later, in his paper “Die physikalische Sprache als Universalsprache der Wissenschaft”, Carnap aims to counter any possible philosophical distinction between the natural sciences and the Geisteswissenschaften, based on a distinction between their objects of study, their methods or the sources of their knowledge. The only division between the sciences that Carnap accepts is a practical division of labour (Carnap 1931, 432). In the *Aufbau* Carnap was still inclined to give the task of constituting cultural objects a name of its own, like ‘phenomenology of the cultural sciences’. This terminological integration has disappeared. “Geist”, a word that featured heavily in the *Aufbau*, is now considered as dangerous terminology that cannot be integrated into the physicalist language:

The sciences mentioned (“Geisteswissenschaft or “cultural sciences”) often in their present form contain pseudo concepts [Scheinbegriffe], viz. such as have no correct definition, and whose employment is based on no empirical criteria; such words stand in no inferential relation to the protocol language and are therefore formally incorrect.

²⁷ Mit Feigl zu Neurath. Neurath schimpft über meine Darstellung der „Geisteswissenschaften“ im „Aufbau“. Ist ihm zu idealistisch; hat Angriffspunkte: Dilthey wird genannt: „Sitte“, „Staat“, „Manifestation“. 1 Uhr zu Hause. Diary entry of December 19, 1929, RC 025-73-03 ASP. I accessed the diaries via the website of Christian Damböck’s project, “Early Carnap in Context: Three Case Studies and the Diaries,” at https://homepage.univie.ac.at/christian.damboeck/carnap_diaries_2015-2018/index.html. This was already noted by (Uebel 2007a, 137).

²⁸ „Geisteswissenschaftliche Philosophie“ erweist sich vor dem unerbittlichen Urteil der neuen Logik nicht etwa nur als inhaltlich falsch, sondern als logisch unhaltbar, daher sinnlos.

Examples: ‘objective spirit’ [objektiver Geist], ‘the meaning of History’ [Sinn der Geschichte], etc. (Carnap [1931] 2011, 72–73)

Similarly, Verstehen is now only understood as a harmful intuitive procedure that is unrelated to the constitution of cultural objects (Carnap 1931, 434). Instead of Dilthey’s terminology, Carnap uses Neurath’s terminology to incorporate these sciences.

By (empirical) sociology is intended the aggregate of the sciences in these regions in a form free from such metaphysical contaminations. It is clear that Sociology in this form deals only with situations, events, behaviour of individuals or groups (human beings or other animals, action and reaction on environmental events, etc. (Carnap [1931] 2011, 73)

Incorporating sociological concepts into the physicalist conception of science, however, remains for Carnap an important task in the logic of science (Carnap 1934a, 17, 1934b, 253). When in 1938, Carnap wrote an article on the “Logical Unity of Science” in English, he used the term “social sciences and the so-called humanities”. The specific idea of a translation of sentences containing objects of those fields into sentences containing only physical and psychological objects, an idea that had already been central in the *Aufbau*, was rephrased by Carnap in the following terms:

The conditions for the application of any term can be formulated in terms of psychology, biology, and physics, including the thing-language. Many terms can even be defined on that basis, and the rest is certainly reducible to it. (Carnap [1938] 1991, 402)

Ten years after the publication of the *Aufbau* nothing remains of Carnap’s incorporation of terminology from Dilthey, Rickert or Windelband. He would never again talk about overcoming the problem of individuality through relational logic, defending the autonomy of the Geisteswissenschaften, incorporating the method of Verstehen or incorporating a phenomenology of the Geisteswissenschaften into constitutional theory. Carnap only maintained that contemporary concepts in the social sciences could be incorporated in a physicalist language in some form or another. Whatever form this might be, could only be determined by the researchers themselves, not by philosophers. Throughout his career, however, Carnap upheld the historical insight which he had ascribed to Dilthey, Windelband and Rickert, namely that social, historical or cultural objects are subject matters of the sciences. This motivates Carnap’s consistent incorporation of the “Geisteswissenschaften” and later “the social sciences” into the program of a logic of science: to determine how social terms can be reduced to terms in a physicalist language was part of the program. Thus, on the one hand, Windelband’s first stake, to epistemically and logically account for the historical sciences, is consistently taken over by Carnap. Unlike most earlier German attempts to account for this stake, Carnap denied that there is anything philosophically interesting to say specifically about these sciences. The only meaningful

logical question about the social sciences concerns the bare possibility to translate sentences containing social terms to sentences containing physicalist terms, and Carnap always took such possibility for granted.

On the other hand, Carnap does not incorporate the second stake, to present a historical view on knowledge, into philosophical concerns. Carnap considered a history of knowledge irrelevant to the logical understanding of science. There are two distinct viewpoints on science according to Carnap, namely as a human activity and as a body of ordered knowledge. “The investigations of scientific activity may be called history, psychology, sociology, and methodology of science. The subject matter of such studies is science as a body of actions carried out by certain persons under certain activities” (Carnap [1938] 1991, 393). Opposite to this viewpoint, the logic of science aims “to analyze the statements of scientists, study their kinds and relations, and analyze terms as components of those statements, and theories as ordered systems of those statements” (Carnap [1938] 1991, 393). The logical analysis of science only studies the syntactic and semantic aspects of scientific language in general. How contemporary scientists have come to use particular protocol languages or theoretical languages, cannot be answered by the logic of science. In a reply to criticism of Edgar Zilsel, Carnap fleshes this distinction out in greater detail. Ascertaining how certain protocol languages emerge and how groups of scientists accept them, is not part of pure, but of *descriptive* semantics (Carnap 1932, 182). These kinds of questions belong to a different viewpoint on science, namely science as a historical and social activity. Consequently, for Carnap, the historical development of scientific language within a social community is quintessential: without such development there would be no science. Every scientific statement is the result of a historical process. He explicitly states that

There is no other way to distinguish ‘our’ science than to distinguish it as the science of our culture, i.e. the science that was constructed after a particular scientific method and with certain particular hypothetical starting points that originated from a particular historical development, and that are now controlled by protocol sentences of scientists of our particular culture.²⁹ (Carnap 1932, 180)

According to Carnap it is only a contingent, historical fact of the matter that there is a social system like science in our contemporary culture that tends towards ever increasing coöperation between the different scientists who increasingly use the same protocol language in their discussions (Carnap 1932, 180). Nevertheless, however important these

²⁹ Es gibt keine andere Auszeichnung für "unsere" Wissenschaft, als die historische Auszeichnung, dass sie die Wissenschaft unseres Kulturkreises ist; genauer: die Wissenschaft, die mit den und den hypothetischen Ansätzen, die dort und dort in der geschichtlichen Entwicklung aufgetreten sind, nach den und den wissenschaftlichen Methoden aufgebaut und an den Protokollsätzen der Wissenschaftler unseres Kulturkreises nachgeprüft wird.

historical developments were for actual scientific results, the logical analysis of scientific language in general could be done independently from them. This independence motivates why Carnap was not much concerned with the specific constitution of sociological terms: this was only a concern for the actors in sociology, who would increasingly incorporate their sociological terms in a physicalist protocol language given their social environment in scientific institutions.³⁰ From a logical point of view, none of the conceptual or terminological debates in the social sciences mattered according to Carnap. Thus, Carnap accepts a historical perspective on knowledge as legitimate, but, contrary to Windelband's conception of the second stake, separates it from a logical analysis of knowledge.

After 1928 Carnap never again took up any positive theoretical position about cultural/sociological concepts. Within the Unity of Science movement, this task was taken up by Otto Neurath, who had done historical, economic and sociological research himself. In Neurath's book *Empirische Soziologie* of 1931, Neurath developed a theoretical view on the social sciences from a Unity of Science perspective. Contrary to Carnap's indifference, Neurath believed that the epistemological problems about historiography and the social sciences were crucial for the success of the movement. At the end of his book, he noted that "in Europe and the USA the logical-empiricist movement to which this publication belongs, is mainly represented by men who on questions of 'history' or 'political economy', take no great trouble to be scientifically precise" (Neurath 1973, 414). These men "direct their analysis at science as such; and what is true of physics in the narrower sense also holds mutatis mutandis for sociology. But in any case there is a series of special problems that awaits separate treatment" (Neurath 1973, 415). Neurath distinguished two tasks for logical-empiricist philosophy. On the one hand it should develop a basic view on concepts in science that is free of any metaphysics – this was the kind of work that Carnap performed excellently. On the other hand it should critically fertilize scientific practice, which was not Carnap's strong point. While the first task could be done by merely focusing on the physical sciences, the second, according to Neurath, also required a discussion of history, economy and sociology (Neurath 1931, 142). Neurath hoped that "some of the younger representatives of this movement will use the new knowledge on sociology" (Neurath 1973, 415).

Neurath believed that his own discussion of sociology would be a good starting point for these young representatives. His work discussed how sociology should be empirically understood in continuity with the other sciences, while still maintaining certain peculiarities of its own. To that end, Neurath first introduced a general position on the aim of scientific theories. "Every scientific sentence is a sentence about a lawful ordering of empirical facts"

³⁰ Of course, as a matter of fact, the social scientists did not do this in the course of the 20th century, and it still is open to debate whether natural scientists actually ever achieved the kind of social unity that Carnap ascribes to them in his reply to Zilsel. Carnap's historically founded confidence in the convergence of a single protocol language, a first sight, seems historically naive.

(Neurath 1931, 2). In this sense, Neurath's understanding of scientific theories is in line with the Mach-Duhem tradition. Any social event or behavior that can be determined as a space-time structure through observation, is a social fact, and one can find many regularities between social facts. Sociology mainly focuses on the customary behavior in specific groups, the expansion of these customs to other groups and the conditions that enable and promote such expansion (Neurath 1931, 114). The sociologist uncovers layers of regular behavior in groups on the earth, just as the geologist uncovers the several layers of compounds on the face of the earth. However, this position did not imply that the systematic connections between facts in the social realm are entirely similar to the lawful connections between facts in the physical sciences, since the lawful systematization of social events does not always allow for stable predictions (Neurath 1931, 16).

Contrary to most regularities in the natural sciences, the regularities between social facts are unstable according to Neurath. There are two reasons for the instability of these regularities.³¹ The first reason lies in the contingent fact that human, social behavior is more unstable than the behavior of animals or natural motion. According to Neurath, each individual of a social system can react differently to new events in his social environment: some individuals will perform abnormal behavior compared to the others in the group, and this abnormal behavior can then expand among groups and between groups (Neurath 1931, 130). This in turn results in a change of the social conditions of other customary behavior. "What is budding today we often recognise only at a later stage when we have already taken part in the subsequent development" (Neurath 1973, 404). Contrary to the parts of complex machines, individuals of a society can change their regular behavior. "But technology is more fortunate, there are many machines of the same kind, and each machine must keep to its own behaviour" (Neurath 1973, 363). In a social system there are numerous elements that influence the regularity of behavior, and all of these elements can change the operation of other elements. Because of technical developments, certain temporally stable social relations can also change dramatically through time, e.g. the population levels that can be supported by agriculture in a specific society, can drastically change due to technological advancements (Neurath 1931, 110). Thus, unlike machine parts, the regularities displayed by human behavior in certain social systems are not robust under changing conditions. This limits the projection of social regularities into the future and consequently the validity of sociological predictions.

A second limit lies in the fact that a sociological statement is itself an element of the social system. Sociological prediction can influence social regularities. By predicting certain social events in the future, the sociologist possibly changes the social relations between individuals. Certain individuals can alter their behavior based on these predictions:

³¹ For a discussion of Neurath's arguments for unpredictability, but from the viewpoint of his 1944 book *Foundations of the Social Sciences*, see (Reisch 2001, 204–6).

predicting a market bubble can change the behavior of traders on a stock exchange, which then exactly prevents the predicted bubble. Unlike predictions in astronomy, sociological predictions are themselves elements in the system of regularities that they use to make the predictions (Neurath 1931, 131). Limited predictions, conditional on the fact that certain social regularities within a system do not change, can still be made. Yet, both the feedback loops of predictions in social systems and the unpredictability of new behavior in populations prevents the possibility of any global predictions by sociologists. Given this limit on the validity of social regularities, the most optimal sociological theory can only yield a system of sentences that contains no contradictions with observations of past social behavior (Neurath 1931, 132). Precise factual predictions cannot be desired of a sociological theory. For Neurath, this is an important distinction between sociology and some parts of the natural sciences.

The distinction between sociology and the natural sciences is, however, not an epistemological one: there is no epistemic principle that necessitates it. The difference is related to the interwoven complexity of social systems and the limits of an experimental practice to capture its complexity. The social sciences cannot set up experiments over their populations. Instead they rely on so-called natural experiments, the vast changes in social relations throughout the history of the world (Neurath 1931, 67). This, however, is only a practical limit on the amount of control that one has over the systems under investigation. Similar to astronomy the sociologist puzzles with the available facts of the past, and attempts to discern patterns. Unlike astronomy, the sociologist cannot use a failed prediction as a control over the discerned patterns, since the conditions on which social systems operate constantly change. Thus, Neurath wanted to distinguish sociology from the natural sciences, purely based on the material characteristics that are different for social systems and the difference in empirical inquiry which resulted from this.

As a consequence, Neurath refused to limit the investigations in the social sciences based on some abstract principles, against Windelband's or Rickert's insistence that the historical sciences were concerned only with unique individual objects or events. According to him the investigation of a relation between sun spots and periodical crises of societies in the past could possibly yield some systematic results. There is no principled way to determine what kind of phenomena in the social realm can be related to other types phenomena. Neurath's central mantra is the following "Systematic definitions [of a domain] cannot determine in advance what will yield more scientific progress. A geologist uses 'laws' of all kinds, he does not limit his field to the earth, or only to non-living things" (Neurath 1973, 365). As Neurath points out, geologists study fossils and meteorite stones. Consequently, sociologists, economists and historians should not limit the direction of their research questions in advance for some abstract reasons. Any principled distinction between sociology and the natural sciences concerning the direction or method of investigation is, according to Neurath, obsolete. Especially the proposal to focus on those objects that can be "understood", is set aside.

Empathy, understanding and the like may help the research worker, but they enter the totality of scientific statements as little as does a good cup of coffee which also furthers a scholar in his work. If someone maintains that he can only predict historical processes ‘empathetically’, we would have to ask him on what he based his empathy. (Neurath 1973, 357)

There is no “understanding sociology” [Verstehende Soziologie] with an epistemic character distinct from the natural sciences that supposedly focuses on those objects that can be related to a value or a human intention. Neurath singles out the attempts by Dilthey, Windelband and Rickert to find the epistemic or logical principles that make the social sciences distinct from the natural sciences, and Neurath denies these attempts any scientific validity – they were all based on philosophical reasons in abstraction from scientific practice in sociology or historiography. According to Neurath these philosophers approached scientific, sociological inquiry from without, through certain non-empirical assumptions about the limits of social inquiry. Neurath specifically focused on Dilthey, who in his eyes should be held responsible for all later “metaphysical” attempts at an epistemic distinction between the natural and the social sciences. Because of Dilthey’s work, “even those people who tend to be empiricist, bow before the term ‘Geisteswissenschaften’ and praise Dilthey’s significant achievement” (Neurath 1973, 356). This was exactly how Carnap had previously discussed Dilthey in his *Aufbau*, by taking over Dilthey’s terminology and emphasizing his historical importance. However, on Neurath’s understanding, Dilthey’s work was the starting point of all metaphysical approaches to the social sciences.

Carnap’s *Aufbau* is, nonetheless, mentioned positively for a different reason. Carnap had given an account of sociological/cultural concepts that made them epistemically indistinguishable from natural scientific concepts: in the *Aufbau* all concepts are embedded through constitutional definitions in a constitutional system. A “logic of the geisteswissenschaften” only investigates how cultural concepts could be defined through previously constituted physical or psychological objects in the system. This constitution was merely a practical task for the investigator of cultural phenomena. When Neurath discussed Carnap’s *Aufbau* in his own work, it is this aspect of the *Aufbau* that interested him: Carnap had shown how a constitutional system containing all scientific objects is possible with a materialist basis consisting of physical objects in space-time (Neurath 1931, 59). Carnap’s relation of documentation was a materialist constitution of cultural objects, since it stipulated what specific utterances in a group of people or specific gestures or objects were the documentation of a cultural object, like a religion. Similarly, Neurath believed that the conversion from Catholicism to Calvinism should be understood as an empirical matter ascertaining “that men who have a certain mode of life and use certain words in cults (and otherwise) begin at a certain point in time to exhibit a different behaviour and perhaps to use different words” (Neurath 1973, 358). Relating cultural objects, like religions or economic systems, to specific events in space-time coördinates, was the central task for the social sciences.

A good conceptual system is a prerequisite for a good sociological theory. Preferably such a conceptual system connects social concepts to other scientific concepts. As an example that attempts to form such a unified conceptual system, Neurath also refers to Carnap's *Aufbau* (Neurath 1931, 110). Unlike Carnap's idea about documentation and manifestation, Neurath proposes to structure all social concepts in three categories: the terrain of life, the order of life and the living standard. The terrain of life contains anything that influences the relations between people: forests, rivers, tools, bacteria, climate, etc (Neurath 1931, 116). The order of life is the set of all customary behavior in a specific group, while the living standards contain everything that determines the quality of life for individuals in a society (Neurath 1931, 125). Neurath believes that all concepts in these categories can be factually observed by researchers in their inquiries, and consequently that they are the perfect conceptual tools to initiate a materialist sociology that focused on social regularities.

This task of systematizing factual observations in the social domain is still not easy. The concepts that one uses to observe social facts are often not uniform. Neurath gives the examples of missionaries who used different conceptual systems to describe the behavior of tribes, which were often mixed with theological concepts (Neurath 1931, 111). Neurath's own preliminary proposal would certainly not do the job. Similarly to Carnap's position, Neurath believed that the construction of a good conceptual system is not a philosopher's task, because it cannot be separated from the factual observations. The construction of concepts and the making of observations would influence one another (Neurath 1931, 112). To make observations about social facts, one already needs a conceptual understanding of social structures, and one needs to attune the conceptual structure to the observations made. Similar to Duhem's and Mach's understanding of scientific concepts, concepts for Neurath are necessarily dynamic, their applicability and sustainability is relative to the set of observations that have been made. Certain concepts will enable new kinds of observations and new observations will make certain older concepts obsolete and require the introduction of new ones. In this dynamic, historical progression of the social sciences, it is central that all metaphysical concepts are progressively removed, just like in physics (Neurath 1931, 110). Neurath himself showed that this progression was already underway by writing a partial history of sociological theory in the first part of his book (Chapter 2-5), arguing that it progressed towards a physicalist conception.

Thus, by 1931 both Neurath and Carnap agreed that a Rickert-like project was impossible: there was no epistemic or conceptual distinction between the natural and historical sciences to lay bare. Their reasons were, however, different. According to Carnap, cultural concepts could be constructed within the same logical framework as any other domain of scientific concepts. As long as these concepts operated in the bounds of constitution theory, or what Carnap would later call a well-formed syntax, any proposal was fine. Logically, there was nothing interesting to say about the cultural sciences. Neurath, on the other hand, believed that it was necessary to investigate the particular

research practices of the social sciences to understand what makes them different from other practices of science, especially concerning the validity of predictions. Thus, while Carnap became increasingly disinterested in the social sciences, Neurath wanted to further scrutinize them. Neurath believed that the Unity of Science movement with its scientific world conception necessarily had to contribute towards the progression of a physicalist conception of the historical and social sciences, and Neurath actively attempted to show through the history of the social sciences, that such a physicalist conception was growing. Thus contrary to Carnap, Neurath also believed that a historical perspective on conceptual developments in the social sciences was a point of interest for the Unity of Science Movement.

2.4 Conflicts over History in the Logical Empiricist Network

In the 1930s many actors in the Unity of Science network had disagreements about the role of history in philosophy, and over philosophy of history. These disagreements show that Hempel's 1942 paper did not come out of nowhere: as a young proponent of the Unity of Science movement Hempel was actively involved in the disagreements about history within the network. Below, I highlight this struggle over history by following the interactions concerning history that two main figures in the network had. First, I focus on Hans Reichenbach's struggles with historically oriented philosophy within his practice as editor of *Erkenntnis* and as leading figure of the Berlin society for Empirical Philosophy (Gesellschaft für empirische Philosophie). In the second part, I deal with similar attempts of Otto Neurath as an organizer of the Unity of Science conferences and editor of the *Encyclopedia of Unified Science*.

2.4.1 Reichenbach's Institutional Conflicts with Historical Philosophy

The first interaction that I would like to highlight starts in April 1930, when Hans Reichenbach, as chief editor of the new journal *Erkenntnis*,³² sent out a letter to Edgar Zilsel

³² For more information on the origins of *Erkenntnis* as a journal, edited by Reichenbach and Carnap, see (Hegselmann and Siegwart 1991; Stadler 2015, 56–57).

asking Zilsel to contribute a manuscript to the new journal.³³ Even though Zilsel had no university position, he was an active member of the Schlick circle and Reichenbach clearly believed that Zilsel would be a good candidate for a contribution to the first volume of *Erkenntnis*. Zilsel replied that he was currently working on a book about the application of a physicalist method to historical and social events. Consequently, Zilsel would prefer to send a manuscript that was related to this issue, specifically on "The rise of science: a sociological problem". Zilsel presented this paper as oriented to the natural sciences in two different ways. According to him, it should be of great interest to Reichenbach. First, the paper would "consider historical events as natural events and seek to connect them through statistical laws".³⁴ Second, it would "treat the rise of the exact sciences and show how the so-called Geisteswissenschaften as they are performed today, are remains of a prescientific time".³⁵ The paper would perform this historical research "by approaching the presentation of its material as it is performed in physics journals".³⁶ Consequently, Zilsel's paper would participate in both aspects of the stakes surrounding historiography: it would show how historiography is epistemologically on a par with physics, and it would also present a historical genealogy of this specific epistemology by relating it to the origins of the natural sciences.³⁷

Reichenbach was very happy with the proposal: it fit well with the intention of the journal to perform philosophy in continuity with the sciences. Reichenbach also wrote back that he did not want the journal to focus solely on the natural sciences. When the journal first appeared after this initial correspondence with Zilsel, Reichenbach introduced his editorial intentions in an introductory text where he specifically stated that contributions like the one proposed by Zilsel were welcome.

As long as the natural sciences contribute the most to knowledge in philosophy, as they have done up until now, they will remain the chief focus of the journal. However, philosophy could be fertilized, as it appears to us, in a similar way by the Geisteswissenschaften, which we would only separate from the sciences in terms of

³³ Reichenbach to Zilsel, 29 April 1930, HR 013-38-32 ASP.

³⁴ "erstens weil sie geschichtliche Vorgänge selbst als Naturvorgänge betrachten und durch statische Gesetzmässigkeiten miteinander zu verknüpfen sucht [...]", Zilsel to Reichenbach, 2 May 1930, HR 013-38-31 ASP.

³⁵ "zweitens weil sie die Entstehung der exakten Wissenschaften behandelt, die sog. Geisteswissenschaften dagegen, wie sie heute betrieben werden, als Reste aus einer vorwissenschaftlichen Zeit auffasst", Ibid.

³⁶ "Auch die Darstellungsart wird sich jener annähern, die etwa in physikalischen Zeitschriften üblich ist." Ibid.

³⁷ Stadler has already noted that of all the core members of the Vienna Circle, Edgar Zilsel and Otto Neurath were interested most in integrating the social sciences into the scientific world conception of the circle (Stadler 2015, 10). This concrete episode shows how Zilsel performed this interest in practice. For Neurath's attempt, see 2.4.2.

a division of labor. We hope to present such philosophy of the Geisteswissenschaften in this journal as well.³⁸ (Reichenbach 1930, 1–2)

Because Reichenbach wanted to have a manuscript within four weeks, Zilsel sent a different text, *History and Biology*, a chapter from the book that he was working on. That manuscript should fulfill Reichenbach's wish “to have a contribution from philosophy of history and sociology”.³⁹ After five months, however, Reichenbach rejected Zilsel's paper, because it was too long.⁴⁰ A week after this initial rejection Reichenbach reconsidered his decision and announced that the paper would fit in a special edition of the journal on biology. In order to make the text shorter he advised Zilsel to remove the examples that were “sprinkled into the text”: “a philosophical journal is only concerned with the principal ideas”.⁴¹ Zilsel refused to comply with Reichenbach's advice, because his examples were not an accidental feature of the paper. Zilsel's motivation highlights how Zilsel understood the position of his own writing within contemporary philosophy of history:

I do not consider your proposal to cut in my manuscript *History and Biology* as expedient.

...

These days there is a large amount of work in philosophy of history that uses a metaphysical strategy of argumentation. Next to this, there is not a small amount of programmatic proposals about history oriented towards the natural sciences. These, however, show that the researchers are not familiar with historical facts. Consequently, these proposals appear dilettantish to experts. If my work is to have scientific value, then it has to show how one could apply a natural scientific method to history in a non-dilettantish, fruitful way.

...

If I were to remove all examples, then only a formal program remains, that would most likely appear sympathetic to readers with a pure interest in the natural sciences.

³⁸ Solange die Naturwissenschaften wie bisher den weitaus grössten Teil an Erkenntnissen in die Philosophie hineinragen, solange werden sie deshalb den Schwerpunkt der Zeitschrift bestimmen; aber an sich scheint uns ein Befruchtung der Philosophie durch die Geisteswissenschaften, die wir überhaupt nur in arbeitstechnischem Sinne von wissenschaften abtrennen möchten, in gleicher Weise möglich, und wir hoffen, von solcher Philosophie der Geisteswissenschaften ebenfalls Zeugnisse bringen zu können.

³⁹ “In den Fall dass es Ihnen angenehm ist, schon in den nächsten Wochen einen geschichtsphilosophisch-soziologischen Aufsatz mit Sicherheit zu erhalten [...]” Zilsel to Reichenbach, 8 May 1930, HR 013-38-29 ASP

⁴⁰ Reichenbach to Zilsel, 4 October 1930, HR 013-38-25 ASP.

⁴¹ “[...] für unsere philosophische Zeitschrift kommt es ja nur auf die prinzipiellen Gedanken an.” Reichenbach to Zilsel, 16 October 1930, HR 013-38-23 ASP.

Such a contribution would, however, lack any scientific fruitfulness and remain unconvincing to any expert.⁴²

Just as Neurath, Zilsel did not want any abstract statements about the historical sciences in general that were unrelated to the actual practice of these sciences. His examples were crucial to achieve this aim, and he refused to cut them out. After this motivated refusal the paper lost any chance of publication in *Erkenntnis*. Reichenbach considered the final rejection of the paper grievous, and he apologized for the late decision.⁴³ As a result, a contribution both on the historical origins of modern science and on the nature of historical knowledge would never get published in *Erkenntnis*. Even though Zilsel's philosophical program was aimed at exactly the kind of contribution from the Geisteswissenschaften that Reichenbach wanted, Reichenbach did not reserve more space for Zilsel's manuscript. As a result, Reichenbach missed an opportunity to show that a natural scientific view on historiography could move beyond programmatic statements, which was exactly the status quo in philosophy of history that Zilsel wanted to break.⁴⁴ Zilsel's manuscript had the potential to articulate the relation between the scientific philosophy of logical empiricism on the one hand and the practice of history on the other hand. As Zilsel realized, such full articulation was necessary to convince historians who would never be won over by merely programmatic statements. This missed opportunity for articulation would come back to haunt Reichenbach within a couple of months.

In May 1931 Reichenbach decided to translate his philosophical views on the relation between philosophy and science to the political sphere. His goal was to promote scientific philosophy within the German educational system and he prepared an official memorandum from his *Society for Empirical Philosophy* to the German ministry of education, requesting more professorships in natural philosophy. In the first draft of the memorandum Reichenbach claimed that “the natural sciences, including the mathematical-physical sciences and the biological, have generated crucial philosophical problems and answers

⁴² “Ihr Kürzungsvorschlag zu meinem Ms. Geschichte u. Biologie erscheint mir nicht zweckmässig. ... Es gibt heute eine Große Zahl "geschichtsphilosophischer" Arbeiten, die metaphysische Redensarten aneinanderreihen; daneben gibt es nicht selten naturwissenschaftlich gerichtete programmatische Äußerungen zur Geschichte, die aber zeigen, dass dem Verfasser die konkreten historischen Tatsachen unbekannt sind, und die daher jeden Sachkenner dilettantisch anmuten. Wenn meine Arbeit wissenschaftlichen Wert besitzt, so könnte das nur dem Umstand entspringen, dass sie zeigt, wie man naturwissenschaftliche Methoden nicht-dilettantisch und fruchtbar auf die Geschichte anwendet. ... Wollte ich die Beispiele weglassen, so bliebe wieder nur ein Formales Programm übrig, das vielleicht manchen rein naturwissenschaftlich interessierten Leser sympathisch anmuten mag, aber wissenschaftlich ganz unfruchtbar ist und keinen Sachkenner überzeugen wird.” Zilsel to Reichenbach, 18 October 1930, HR 013-18-22 ASP.

⁴³ Reichenbach to Zilsel, 20 October 1930, HR 013-18-21 ASP.

⁴⁴ Zilsel would later also criticize the Vienna Circle for their lack of actual empirical research, and Neurath's lack of concrete examples to support his ideas on the social sciences (Uebel 2007b, 255).

during the last decades”.⁴⁵ These philosophical advances had, however, not reached academic education: “Philosophy oriented to the natural sciences is only extremely weakly represented in German higher education so far and philosophy chairs are mainly preserved for representatives of a historical and cultural scientific method”.⁴⁶ Remedying this situation would benefit both students from the natural sciences and students of philosophy. Therefore, through the memorandum the *Society for empirical Philosophy* requested new philosophy chairs for natural philosophy and, if there would not be enough financial means, it requested that representatives from natural philosophy be involved in upcoming appointments for existing philosophy chairs.

In order to give his plea more authority, Reichenbach sent out an initial draft to many prominent German scientists for support (e.g. Hilbert, Einstein, Planck, Haber). Most of them gave their approval to the petition. Only one German philosopher received the draft, Ernst Cassirer.⁴⁷ Reichenbach believed that Cassirer was a supporter of his cause: “I know that you, just as we, see fault in the existing one-sided occupation of philosophical chairs”.⁴⁸ Even though Reichenbach mainly sought support from prominent scientists, he also considered it important to find philosophers who were willing to support him – and “they are hard to find, because many philosophers follow an opposite tendency”.⁴⁹

In 1931 Cassirer, the former rector of Hamburg University (1929-1930), was one of the exponents of German historical philosophy. Cassirer's *Das Erkenntnisproblem* was perhaps the best known historical account of contemporary epistemology and Cassirer constantly used results from the cultural sciences in his *Philosophie der symbolische Formen*. Cassirer's advice to Reichenbach is, consequently, not surprising. He wrote back in the most

⁴⁵ “Aus den Naturwissenschaften heraus, und zwar aus den mathematisch-physikalischen ebenso wie aus den biologischen Wissenschaften, sind in den letzten Jahrzehnten entscheidende philosophische Problemstellungen und Problemlösungen erwachsen.” Reichenbach to Minister (draft), undated, HR 025-11-20 ASP.

⁴⁶ “Die naturwissenschaftlich gerichtete Philosophie innerhalb der Deutschen Hochschulen ist bisher nur ausserordentlich schwach vertreten und die philosophischen Lehrstühle werden fast durchweg Vertretern der geisteswissenschaftlich-historischen Richtung in der Philosophie vorbehalten.” Reichenbach to Minister (draft), undated, HR 025-11-18 ASP.

⁴⁷ For a discussion of the philosophical relationship between Reichenbach's scientific philosophy and Cassirer's historical views on knowledge, see (Heis 2013).

⁴⁸ “Ich weiss, dass Sie, ebenso wie wir, in der bisher bestehenden Einseitigkeit bei der Besetzung philosophischer Lehrstühle einen Fehler sehen.” Reichenbach to Cassirer, 5 June 1931, HR 025-11-04 ASP. In previous years Reichenbach had corresponded regularly with Cassirer about a possible position for scientifically oriented philosophers. After a visit to Cassirer in 1925 he remarked that “es Gott sei dank auch andere Menschen gibt, die über die Fragen nachdenken, welche mich interessieren. Sie können sich wohl gar nicht vorstellen, wie erfrischend ein solcher Eindruck ist.” Reichenbach to Cassirer, 22 January 1925, HR 016-03-16 ASP.

⁴⁹ “[...] aber scheint es mir notwendig zu sein, gerade auch Vertreter der Philosophie für die Unterschrift zu gewinnen. Das ist schwierig, weil ja die meisten Vertreter der Philosophie die gegenteilige Tendenz verfolgen.” Reichenbach to Cassirer, 5 June 1931, HR 25-11-04 ASP.

careful words that he was principally in agreement, but that Reichenbach should adapt his phrasing of the issue in order to have a better effect.⁵⁰

If you want to have the support of philosophers, then you should – in my view – avoid any suspicion that what you want, is a competition with philosophy that is oriented to the cultural sciences and cultural history. It all comes down to presenting an understandable case to the Ministry, that proper chairs for Natural Philosophy are an unconditional necessity today, and that this requires the appointment of researchers who have this area of expertise and master the methods of the contemporary natural sciences.⁵¹

In order to get Cassirer's signature, Reichenbach added the following specific clause to later drafts of his petition.

It should be explicitly noted that this request does not imply a specific opinion about philosophy that is oriented towards history and the cultural sciences. It is merely a request for fairness and scientific approach, when one requests to discontinue the existing one-sidedness in the occupation of philosophical chairs.⁵²

He wrote to Cassirer that this clause would not be welcomed by some scientists who had already given their signature, especially David Hilbert.⁵³ "You cannot imagine how extensive the bitter judgments are in natural scientific circles towards the current ruling trend in philosophy; it is actually only your name that people discount from this judgment."⁵⁴ For Cassirer, there was no conflict at an intellectual level between philosophy

⁵⁰ "Ich bin daher auch gern bereit, ihn zu unterzeichnen, möchte aber, ehe ich diese tue, an dem Wortlaut noch einige Änderungen vorgenommen wissen, die, wie ich glaube, seinen praktischen Wirksamkeit nun hinderlich sein könnten." Cassirer to Reichenbach, 11 June 1931, HR 25-11-02 ASP.

⁵¹ "Gerade wenn Sie auf Unterstützung von Vertretern der Philosophie rechnen, sollte meines Erachtens der Anschein unbedingt vermeiden werden, als handle es sich in dem, was Sie anstreben, um irgend eine "Konkurrentzunternehmer" gegen die geisteswissenschaftliche und geistesgeschichtlich eingestellte Philosophie. Es kommt alles darauf an, den Ministerium begreiflich zu machen, dass eigene Lehrstühle für "Naturphilosophie" heute eine unbedingte Notwendigkeit sind, und dass hierfür nur Forscher in Frage kommen, die das Tatsachengebiet und die Methodik der heutigen Naturwissenschaft selbständig beherrschen." Ibid.

⁵² "Es sei ausdrücklich betont, dass mit dieser Forderung keine Stellungnahme zu der geisteswissenschaftlich-historischen Philosophie an sich ausgesprochen sein soll; es ist lediglich eine Forderung der Gerechtigkeit und Wissenschaftlichkeit, wenn wir darum bitten, dass die bestehende Einseitigkeit in der Besetzung der philosophischen Lehrstühle aufhören soll." Reichenbach to Minister (draft), June 1931, HR 025-11-33 ASP.

⁵³ "Es ist mir garnicht leicht, diesen Standpunkt gegenüber den Naturwissenschaftlern durchzusetzen, die vielfach eine schärfere Form der Eingabe wünschen; so hat Herr Hilbert meine erste Fassung ursprünglich noch verschärfen wollen, und ich werde ihn für die jetzige mildere Fassung erst neu gewinnen müssen." Reichenbach to Cassirer, 15 June 1931, HR 025-11-03 ASP.

⁵⁴ "Ich glaube, Sie können sich garnicht vorstellen, wie grob vielfach in naturwissenschaftlichen Kreisen die Erbitterung gegen die herrschende Richtung in der Philosophie ist; es ist eigentlich nur Ihr Name, den man von diesem Urteil ausnimmt." Ibid.

oriented to the natural sciences and philosophy oriented to the cultural sciences. Whatever Reichenbach's actual views were on the theoretical relation between scientific philosophy and the cultural sciences, the fact that he did write Cassirer's clause into his petition shows that his interest with the petition was solely on the practical level of creating more institutional space for philosophy oriented to the natural science. However, as a result of the clause, Reichenbach's text seemed to highlight not only a mere institutional distinction, but also a conceptual distinction between two types of philosophy and two types of science which merit equal institutional acknowledgment. In turn, the hint at such distinction brought Reichenbach in conflict with his associates from Vienna.

Around the time of the exchange with Cassirer, Reichenbach also wrote to Carnap about "an action that he [Reichenbach] is preparing in order to promote chairs for our movement".⁵⁵ Carnap discussed Reichenbach's proposal with Schlick, Neurath and Hahn, and he wrote back about their judgment. According to Carnap's report, the philosophers of Vienna "are essentially of one mind".⁵⁶ Schlick did not believe in the possibility of success, and thought that it would be better to use personal influence to change the situation. Moreover, Schlick believed that the way the proposal was phrased could generate false ideas, as if natural philosophy were somehow separate from philosophy of history, while there was obviously only one philosophy. "We should abstain from the impression that we are simply 'natural philosophers' who leave the others to their business as they want to conduct it."⁵⁷ Similarly, Hahn thought chances of success were very low, while the formulation of the memorandum had a tendency against the unity of science. Neurath fully agreed with Schlick. Finally, Carnap first described himself as neutral, but to end of the letter he too refused to give his support. Carnap reminded Reichenbach that the Viennese philosophers disliked Reichenbach's separation of "Natural philosophy" – and requested once more a clarification of whether Reichenbach actually believed such separation to be theoretically necessary or only practically required.⁵⁸

⁵⁵ "In den nächsten Tagen will ich Ihnen noch genaueres über eine Aktion schreiben, die wir hier gegenwärtig vorbereiten, um für unsere Richtung Lehrstühle zu gewinnen." Reichenbach to Carnap, 10 June 1931, HR 013-41-53 ASP.

⁵⁶ "Wir sind in Wien über diese Fragen wesentlich einer Meinung." Carnap to Reichenbach, 11 July 1931, HR 013-41-52 ASP.

⁵⁷ "Falls sie in dem geplanten Sinne gemacht würde, so würden auch falsche Vorstellungen erweckt, als gäbe es Naturphilosophie und Geschichtsphilosophie getrennt, während es doch selbstverständlich nur die Philosophie (in gewissem Sinne) gibt. Auf keinen Fall dürfen wir es so darstellen, als seien wir nur "Naturphilosophen" und auf dem übrigen Gebiet dürften die andern ihre Sache in beliebiger Weise betreiben." Ibid.

⁵⁸ "Unsere Ablehnung einer Abtrennung der "Naturphilosophie" ist Ihnen bekannt. Wollen Sie diese Abtrennung in der Denkschrift nur aus taktischen Gründen vornehmen oder sind Sie hier auch schon in der prinzipiellen theoretische Frage anderer Ansicht als wir?" Ibid.

One year before this rejection of the petition, Reichenbach, Carnap and Schlick had already heavily discussed a similar issue, and their disagreement had great consequences.⁵⁹ Initially all three would be editors of the new journal *Erkenntnis*. However, when Reichenbach in April 1930 sent a draft of his introductory editorial to the others, Carnap and Schlick were very displeased to read that Reichenbach made concessions to traditional philosophy.⁶⁰ In particular, certain passages seemed to suggest that Reichenbach still considered the contents of the new journal as part of philosophy in opposition to other domains of knowledge. This would imply that a separate domain of philosophical knowledge was possible, which, according to Carnap, contradicted the intellectual revolution of the Vienna circle.⁶¹ Reichenbach responded with amazement over this strong disagreement from Vienna, and (perhaps as a joke) added that the Viennese philosophers should not make the same mistakes as traditional philosophy.

Our program should be a program of cooperation, and not a program of a specific philosophical movement that is misunderstood because of specialized terminology.⁶²

This reproach by Reichenbach was not received well in Vienna. Carnap wrote back that Schlick withdrew from his editorial responsibilities for *Erkenntnis*. Even though Carnap would remain on board as an editor, he himself noted that Reichenbach's formulations implied that Reichenbach merely wanted to improve contemporary philosophy, whereas the Viennese philosophers believed that philosophy was at a decisive turning point. For Carnap, the journal did not articulate this revolutionary attitude enough.⁶³

In July 1931 the same discussion and the same arguments reappeared in their dispute over Reichenbach's petition, which after the inclusion of Cassirer's clause implied that there were two equally valid, but separate strands in contemporary philosophy that merited equal academic attention. The Viennese philosophers could not agree with such an implication. In August 1931 Reichenbach received a discussion note from Neurath to be published in

⁵⁹ Thomas Uebel has already pointed out that the terminological Vienna-Berlin dispute over the term “Naturphilosophie” is linked to the struggle over the editorship of *Erkenntnis* and Reichenbach's Memorandum. (Uebel 2013, 88).

⁶⁰ “Sie machen der traditionellen Philosophie hier in einem Grade Zugeständnisse, der mich nach Ihren bisherigen Stellungnahmen sehr verwundert hat.” Carnap to Reichenbach, 29 April 1930, HR 013-41-66 ASP.

⁶¹ For this intellectual opposition of the Vienna circle against an autonomous discipline of philosophy, also see (Stadler 2011, 2015, 42; Uebel 2008, 90).

⁶² “Unser Programm muss ein Arbeitsprogramm sein, und nicht das durch eine spezialisierte Terminologie missverständliche Programm einer bestimmten philosophischen Richtung.” Reichenbach to Carnap, 6 May 1930, HR 013-41-65 ASP.

⁶³ “Wir alle hier sind der Meinung, dass die Philosophie sich gegenwärtig an einem entscheidenden Wendepunkt befindet; dass es sich nicht darum handelt, die bisherige Philosophie in etwas verbesserter, vorsichtigerer Form fortzusetzen. Wir fürchten jetzt (ich selbst habe mich am längsten gegen diesen Gedanken gesträubt), dass dieser entscheidende Punkt in der Haltung der Zeitschrift nicht zum Ausdruck kommen wird.” Ibid.

Erkenntnis, Remarks on Reichenbach's book: *Goals and Directions of contemporary Natural philosophy*. Neurath remarked that Reichenbach maintained a distinction between natural philosophy as counterpart to cultural philosophy or cultural scientific philosophy, which also implied a parallelism between the cultural sciences and natural sciences.⁶⁴ However, according to Neurath, Reichenbach also believed that there was an opposition between the natural sciences and literature, which implied that only literature was in opposition to the natural sciences. Neurath demanded a clarification: "Is there only literature beside natural science, or also cultural science and cultural philosophy, whose encroachment one should fend off?"⁶⁵ Neurath's note was a request to clarify how Reichenbach's use of the term "natural philosophy" was related to demarcations in scientific knowledge and philosophy, like cultural philosophy and cultural science.

Reichenbach was furious after reading Neurath's note and refused to publish it. He even threatened to resign as editor of *Erkenntnis* if Neurath would press on him to publish it.⁶⁶ Carnap convened with Hahn and Neurath, after which Neurath decided to withdraw the discussion note. He would reformulate his questions into a positive contribution.⁶⁷ Reichenbach's concessions to Cassirer in the petition had reopened his discussion with the Vienna philosophers from the year before. The discussion concerned terminology – exactly the kind of philosophical discussion that scientific philosophy was supposed to escape. Underneath the terminological dispute, however, lay the epistemological questions that Windelband articulated in 1904: what is historical knowledge, does it require an epistemology distinct from the natural sciences, and can one write a history of knowledge itself? Whatever happened to Reichenbach's petition, after all the set-backs and disputes, is unclear. It certainly was not published in *Erkenntnis*, as Reichenbach had promised his fellow petitioners. It probably faded into the background as a failed attempt at political action.

Two days before Reichenbach wrote his letter to Carnap threatening to resign from *Erkenntnis*, he expressed his frustration in a letter to Sidney Hook, a philosophy professor at the New York University:

⁶⁴ "Weiter scheint Reichenbach unter "Naturphilosophie" nicht eine Philosophie des Empirismus schlechthin verstehen zu wollen, sondern ein Gegenstück zur Kulturphilosophie (geisteswissenschaftliche Philosophie). An anderer Stelle führt er den Parallelismus: Kulturphilosophie-Naturphilosophie, Kulturwissenschaften-Naturwissenschaften näher aus." Neurath to Reichenbach, Zuschrift an die Herausgeber, undated, HR 013-41-50 ASP.

⁶⁵ "Gibt es neben Naturwissenschaften nur die Literatur, oder neben dieser noch Kulturwissenschaften und Kulturphilosophie, der übergriffe man abwehren müsse?" Ibid.

⁶⁶ Reichenbach to Carnap, 22 August 1931, HR 013-41-49 ASP.

⁶⁷ Carnap to Reichenbach, 9 September 1931, HR 013-41-48 ASP.

For some time now I am constructing a plan, of which I am currently writing to you. I would like to gain closer contact with American philosophy. I have the feeling that the way our circle performs natural philosophy in Germany, could find more comprehension in America than in Germany, where we constantly have to struggle against the dominance of historically oriented school philosophy.⁶⁸

This letter from Reichenbach to Sidney Hook was the start of Reichenbach's attempt to migrate to the United States which concluded in Reichenbach's appointment at UCLA in 1938.⁶⁹ It began after Reichenbach's repeated confrontation with the problem of history in German philosophy between 1930 and 1931: Schlick, Carnap, Neurath and Cassirer all reminded him that no matter how indifferent Reichenbach was to historiography, he still needed to take up some kind of position in his writing. Through Zilsel's paper Reichenbach had received one possible route to articulate the relation between the natural sciences, historiography and history of science. Even though Zilsel had a clear idea of what a unification of these problems should imply, and also how it could be presented in a convincing way to participants of the debates surrounding historiography, Reichenbach did not consider it important enough to merit the required extra pages.

In the introduction of Reichenbach's *Experience & Prediction*, one can learn why Reichenbach believed that a historical perspective on knowledge was not very interesting from a philosophical point of view. Similarly to Carnap, Reichenbach believed that the sociological perspective on scientific knowledge can be distinguished from the logical or epistemological perspective. "Epistemology considers a logical substitute rather than real thought-processes. For this logical substitute the term rational reconstruction has been introduced" (Reichenbach [1938] 1961, 5). The reconstruction, supposedly, enables us to understand what we have always been thinking. Reichenbach compares it to the full demonstration of a mathematical theorem (Reichenbach [1938] 1961, 6). It replaces actual thinking with justifiable operations. To describe this epistemological point of view, Reichenbach introduced the term "context of justification", in opposition to the "context of discovery".⁷⁰ Within this context of justification, one can both reconstruct scientific reasoning and critically analyse it. The critical analysis investigates whether the

⁶⁸ "Seit einiger Zeit geht mir ein Plan durch den Kopf, von dem ich Ihnen heute schreiben möchte. Ich würde nämlich sehr gern mit der amerikanischen Philosophie mehr Kontakt gewinnen, denn ich habe das Gefühl, dass für die Art, wie unser Kreis jetzt in Deutschland Naturphilosophie treibt, in Amerika vielleicht mehr Verständnis da ist als in Deutschland selbst, wo wir immer gegen die übermacht einer ganz überwiegend historisch eingestellten Schulphilosophie zu kämpfen haben." Reichenbach to Hook, 20 August 1931, HR 014-51-28 ASP.

⁶⁹ For an overview of Reichenbach's period in Istanbul between his position in Berlin and his appointment at UCLA, see (Irzik 2011).

⁷⁰ Don Howard has shown convincingly that the distinction was introduced by Reichenbach primarily to counter Neurath's conventionalist holism that would allow social and political values to influence theory choice (Howard 2006).

reconstructed justifications are in fact valid. For Reichenbach, the critical analysis is the logic of science proper (Reichenbach [1938] 1961, 8). The most important task for this logic of science is the determination of the conventional elements in scientific theories, e.g. it was an important success for the logical investigation of scientific theories to discover that Euclidian geometry is in fact a conventional choice within mathematical physics, even though it was previously conceived as a necessity.⁷¹ Whereas the sociology of science can determine which choices have been made in the practice of science, only the logic of science can determine which elements of a theory are in fact open to choice, and the logic of science can determine this independently from the sociology and historiography of science (Reichenbach [1938] 1961, 11). Similarly to Carnap, Reichenbach thus emphasized the autonomy of the logic of science, which can investigate science in abstraction from its historical development. However, unlike Carnap, Reichenbach was largely unconcerned with debates concerning the epistemological relation between the social and the natural sciences. Outside of his intentions in the editorial of *Erkenntnis* to publish on the Geisteswissenschaften as well, Reichenbach said very little on the topic.

2.4.2 Neurath's Attempts to focus the Movement on History

As Neurath had pointed out at the end of his *Empirische Soziologie* in 1931, the writings of logical empiricist philosophers did not contain a rich articulation of the intellectual stakes surrounding historical knowledge. In the years after the disagreement between Reichenbach and the Viennese philosophers, this lack of articulation became a central element in the correspondence between Otto Neurath and other actors in the network of logical empiricism. Several interactions surrounding Otto Neurath during his years in Holland (1933-1940) are good indicators of the various disagreements in the movement over the historical and social sciences.⁷² Below, I first focus on Neurath's attempt to convince Carl Hempel to discuss the historical sciences and to use a historicizing method for the analysis of science. Next, I show that Hempel was not an isolated case, but that Neurath actively attempted to proliferate discussions over these issues.

⁷¹ For a contextualization of Reichenbach's conventionalist philosophical project and the discovery-justification distinction within a German debate over the position of the a priori in either reason or the will, see (Richardson 2006).

⁷² In the Vienna Circle meetings Otto Neurath had regularly insisted on the integration of history of science and sociology of science (Stadler 2015, 45; Carnap 1963, 22). In the text below, I investigate how Neurath advocated this agenda after the Viennese period. For an overview of the entire scope of Neurath's activities after his migration to Holland, see (Sandner 2014, 234–96).

2.4.2.1 Neurath's Initial Plea with Hempel

In 1935 Otto Neurath and Carl Hempel started corresponding heavily over issues concerning the historical sciences.⁷³ At the time, Hempel was working in Brussels with Paul Oppenheim on a book about the concept of type in the sciences. In January 1935 he sent a manuscript of the work to Neurath for feedback. In his response Neurath disagreed with Hempel and Oppenheim's use of the distinction between the natural sciences and the cultural sciences [Kulturwissenschaften]. Even though he realized that the distinction was only used for practical purposes, he wrote that some readers would reify Hempel and Oppenheim's use of a distinction between two types of science while such a distinction was unnecessary. He mocked that "there are authors who use 'nomothetic' and 'idiographic' as true Windelbandits and who dream of differential quotients, and speak of set theory, as if they have been fed such distinctions as mother milk".⁷⁴ Using Windelband's terminology, even if it were only for a practical distinction between the sciences, was, according to Neurath, a bad idea. Neurath advised not to talk about science by starting from certain rigid, top-down distinctions, e.g. between the natural and the cultural sciences. Authors who start their discussions of science from such abstract points of view often go astray:

When one wants to show the meaning [of one's analysis] for empirical science, it is more important according to me to point out how [a scientific] author performs his work, than to point to an author who already starts from classifying and methodologizing points of view, who presents desiderata that mostly are not the right ones, because such an author is not skillful enough to analyze himself.⁷⁵

Consequently, Neurath advised Hempel not to use Windelband's distinction between nomothetic and idiographic science: "the distinction between natural science/generalising and cultural science/individualising is not an opportune distinction, even if one would merely consider [the distinction] as a more or less."⁷⁶ These ideas of Neurath were also

⁷³ Based on the correspondence that is preserved in the Neurath Nachlass, the correspondence between Hempel and Neurath only began after Hempel's initial migration to Brussels in 1934. Hempel was one of the most regular correspondents of Neurath between 1934 and 1939: they wrote up to three times a week to each other.

⁷⁴ "Da gibst Autoren, die "nomothetisch" und "Ideographisch" sagen, wie richtigen Windelbanditen und dabei von Differentialquotienten träumen, von Mengenlehre reden, als ob sie mit der Muttermilch eingezogen hätten [...]" Neurath to Hempel, 2 February 1935, Nr. 244 VCA.

⁷⁵ "Wenn man die Bedeutung für die empirische Wissenschaft zeigen will, entspricht es meiner Art mehr einen Autor an der Arbeit zu zeigen und weniger einen, der selbst schon klassifizierend, methodologisierend auftritt, Desiderata kund gibt, die meist gar nicht die richtigen sind weil er zu unbeholfen ist, um sich selbst zu analysieren." Neurath to Hempel, 2 February 1935, Nr. 244 VCA.

⁷⁶ "So wie ich ja auch Naturwissenschaftlich-Generalisierend und Geisteswissenschaftlich-individualisierend nicht für glückliche Zweiteilung halte, selbst wenn man das nur als ein mehr und minder bezeichnet." Neurath to Hempel, 2 February 1935, Nr. 244 VCA.

active when he wrote his discussion note to Reichenbach: making a distinction or hinting at a distinction without analyzing how scientists (in this case historians) actually reason in their domain, is a misguided philosophical method.⁷⁷ Neurath said that Oppenheim and Hempel had "written a cookbook with logic" at various points in the draft.⁷⁸ On Neurath's account, if one cannot show how the subtle, logical distinctions are connected to the actual research of scientists, it is often not clear how the logician's analysis could contribute anything.⁷⁹ His own advice in the letter is to avoid Rickert's terminology entirely: "I myself either write much more subtle (...), or much more coarse and 'ballunghaft'. When I write subtle, I avoid the discussion natural sciences –cultural sciences."⁸⁰ After summarizing Rickert's position, he warned that people might start to understand Hempel and Oppenheim's position on the same lines as the metaphysics of Rickert which imposes abstract norms on historical practice. "And when experts take up the subtleties and they represent Rickert through the method Oppenheim-Hempel, everyone will be cursed with metaphysics."⁸¹ Even though Neurath clearly does not agree with the kind of logical distinctions that Rickert or Windelband make, he does not want Hempel to engage in a critical dialogue with this tradition either.

I do not want to encourage you to start a debate with Rickert. It was an excruciating task for me to plow through approx. 650 pages, especially given that the book has no index and has a blasphemous length. ... I wish it on no one to read something like that – but one should be cautious with an attack, because the backlash could be unpleasant.⁸²

It is thus better to analyze scientific works and in general avoid abstract Rickertian terminology like "cultural sciences" [Kulturwissenschaften].

A second defect that Neurath noticed in Hempel's manuscript was the lack of a historicizing approach by the authors. "It is important to proceed historicizing, i.e. to

⁷⁷ This aspect of Neurath's naturalism, the rejection of a philosophical epistemology that could justify science from without, is a well-known feature of Neurath's work (N. Cartwright et al. 1996, 95).

⁷⁸ "Dass SIE BEIDE in Gefahr kämen ein Kochbuch mit Logistiek zu schreiben, ist mir auch nicht mal entfernt in dem Sinn gekommen." Neurath to Hempel, 2 February 1935, Nr. 244 VCA.

⁷⁹ "So was züchst man, wenn man nicht selbst andeutend zeigt, wie die subtilen Probleme irgendwie an die konkrete Forschung anzukuppeln sind." Ibid.

⁸⁰ "Ich Selb schreibe manchmal mehr subtil (...), manchmal mehr grob und 'ballunghaft'. Wenn ich subtil schreibe, meide ich meist die Diskussion 'Naturw.' - 'Geisteswiss.'" Ibid.

⁸¹ "Und wenn nun Kenner auf die Subtilitäten eingehen und Rickert nach Methode Oppenheim - Hempel darstellen, wirds bei aller Metaphysik verflucht." Ibid.

⁸² "Ich will nicht anregen, sich um solcher Debatte willen mit den Rickert auseinanderzusetzen. Es ist eine für mich qualvolle Arbeit gewesen, meiner Zeit dieses ca. 650 Seiten durchzupflügen, zumal das Buch ohne Index ist und von einer lästerlichen Breite. ... Ich wünsch das niemandem, derlei zu lesen - dann aber muss man mit der Attacke vorsichtig sein, weil der Gegenschlag unangenehm werden kann." Ibid.

determine an author as the representative of a tendency; to this end one should draw out the complete situation of science alongside its origin.”⁸³ Neurath said that “many predecessors of Einstein and Mach were more modern than their contemporaries exactly because of their historical readings.”⁸⁴ It is, consequently, good to know how a problem in science arose that is still of actual importance.⁸⁵ Neurath had consistently performed this historicizing method in *Empirische Soziologie*, where he had traced the historical origins of a physicalist sociology in national economy and historiography (Neurath 1931, chaps. 3–5). The lack of a historical view on knowledge in Hempel and Oppenheim’s work concerns the second aspect of the stakes surrounding the historical sciences: what does it mean to have a historical perspective on knowledge? Through his comments on the manuscript Neurath emphasized that he did not want to delete this historical aspect from the logical analysis of science.

Hempel responded to both concerns. He and Oppenheim fully agreed with Neurath’s first remark: “the empirical researchers themselves often make completely false presumptions about their methods, one should analyze their particular work.”⁸⁶ The distinction between natural sciences and cultural sciences remained, however, at the margins of their research. They would not engage with this discussion, which would require too much commitment to the large literature on the topic.⁸⁷ Nonetheless, Hempel hinted that he agreed with Neurath’s position on the futility of the distinction: their work exactly intended to prove that a distinction between sciences is logically unnecessary.⁸⁸ However, Hempel nowhere stated that he would cease to use the distinction for practical purposes, as Neurath had advised. In response to a private letter from Neurath (one that would not be read by Oppenheim) Hempel also replied to Neurath’s second concern.

⁸³ “Es ist wichtig historisierend vorzugehen, d.h. einen Autor als Repräsentant einer Richtung zu kennzeichnen, dazu muss man die Gesamtsituation der Wissenschaft andeutend zeichnen und ihr Werden.” Neurath to Hempel, 2 February 1935, Nr. 244 VCA.

⁸⁴ “Es ist äusserst reizvoll zu sehn, dass manche Autoren vor Einstein und vor Mach durch entsprechende historische Lektüre wesentlich moderner waren, als ihre Zeitgenossen!” Ibid.

⁸⁵ This historical aspect of Neurath’s reasoning about science is related to Neurath’s specific reading of Mach and Duhem that was already present in Neurath’s ideas before World War I. For further discussion see (N. Cartwright et al. 1996, 108–9).

⁸⁶ “die empirischen Forscher selbst machen oft ganz falsche Angaben über ihre Methoden, man muss ihre Einzelarbeit analysieren.” Hempel to Neurath, 6 February 1935, Nr. 244 VCA.

⁸⁷ “Natur-Geisteswissenschaften kommt nur am Rande; eingehende Diskussion überschreitet die Themastellung, und wir reisen uns nicht darum - auch aus den von Ihnen angegebenen Gründen; man muss viel Arbeit auf Lektüre nicht sehr lohnender Sachen verwenden. Wir wollen nur einen Ausblick: im Buche nachzuweisende Formübereinstimmungen der Begriffsbildung in allen Gebieten der empirischen Wissenschaft sind ein Indiz gegen die These, dass die empirische Wissenschaft in logisch prinzipiell verschiedenen Gebiete zerfalle.” Ibid.

⁸⁸ Ibid.

I dare not to engage a historicizing approach in the manuscript: I do not know enough about it and would have to make many and long preparatory studies. The logical analysis already takes up more than enough time, if it is to be done somewhat properly. Oppenheim is just as unhistorical as me. (I am also happy that Carnap is unhistorical.)⁸⁹

Neurath replied that he considered Hempel and Oppenheim's lack of historical interest a shame. According to him, it was a mistake to believe that a historicizing approach to knowledge is optional, just like skiing can be a nice hobby in life.

If one wants to come into logical contact with the empirical sciences, namely with all of them, then one should train oneself in a pragmatic-historicizing way. You are still so young that you can afford to train yourself a little in this direction. We lack this attitude in our movement, and the encyclopedic works, which I have just begun to conceive, especially require this attitude. I think it is an attitude that will be important in the future. The purely logicizing attitude, which is of decisive importance (Carnap has, so to speak, an important historical mission), can also be abused.⁹⁰

The possible abuse of logical analysis was a dominant theme in the correspondence. Neurath returned to this threat of a purely logicizing attitude again and again to persuade Hempel that the logical calculus was only a limited instrument. He summarized his position through the following metaphor: "Even with the finest instruments one cannot get more apples out of a roasted Goose than it contains."⁹¹ Although Neurath still upheld logic as an important instrument, he remarked to Hempel that "Mach clarified a lot without it". A historical understanding of scientific developments was according to Neurath an equally necessary tool for the analysis of science.

Later in 1935 Neurath also pressed Hempel on the fact that he should be careful about the kinds of terms that one introduces in analyses of science. Neurath warned Hempel not

⁸⁹ "An historisierende Betrachtung in MS wage ich mich nicht heran: ich kenne mich da fast überhaupt nicht aus und müsste sehr viele und lange Vorstudien machen. Aber die logische Analyse selbst kostet schon Zeit genug, wenn halbwegs ordentlich werden soll. O ist ähnlich unhistorisch wie ich. (Ich bin bloss froh, dass Carnap es ist.)" Hempel to Neurath (private letter), 6 February 1935, Nr. 244 VCA.

⁹⁰ "Historisierend das ist eine Sache für sich, wie Skifahren, wie gut, dass auch Carnap...so stehst nicht. Wenn man mit den empirischen Wissenschaften in logische Kontakte treten will, nämlich mit allen, muss man diese pragmatisch-historisierende Art bei sich pflegen. Sie sind noch so jung, dass Sie sich leisten können, in dieser Richtung sich ein wenig auszubilden. Wir haben Mangel an dieser Haltung in unserer Bewegung und gerade die enzyklopädischen Arbeiten, die ich allmählich immer erster ins Auge fassen bedarf dieser Haltung. Es ist glaube ich eine Haltung, die in der Zukunft wichtig sein wird. Die rein logisierende Haltung, die von entscheidender Bedeutung ist (Carnap hat eine wichtige historische Mission so zu sagen) kann auch missbraucht werden." Neurath to Hempel, 8 February 1935, Nr. 244 VCA.

⁹¹ "Man kann mit Hilfe feinsten Instrumenten aus einer gebratenen Gans nicht mehr Äpfel herausholen, als drin sind." Neurath to Hempel, 8 October 1934, Nr. 244 VCA

to take the introduction of abstract terminology and theoretical distinctions lightly. He wrote that 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."⁹² As an example, Neurath mentioned Carnap's earlier use of the metaphysically-laden term "Geisteswissenschaften". For Neurath, the danger of metaphysical terminology was related to his historicizing approach: one had to inquire into the history of scientific terminology to determine whether terms introduced in the logic of science had a metaphysical history. Hempel, however, did not care much for the historical study of terms, and he did not see the necessity for it in his logical analysis of science.

It is practically impossible to inform oneself about every new term that one introduces. If I were to pursue your advice, then I would have to make long preparatory studies before the introduction of any philosophical expression. I would have to investigate who had already used this expression and with which meaning. This appears to me as a meaningless waste of time.⁹³

Neurath rebutted him:

How is it a pointless waste of time to occupy oneself with information on terminology that one uses oneself, by e.g. looking into two or three books where this terminology is actually used? I don't understand it. This should be a good reason to familiarize oneself more with the history of one's own science.⁹⁴

Neurath's pleas for a historical approach to the analysis of science were not successful: Hempel stuck to his belief that the history of knowledge is an optional research interest. It is clear from their correspondence that Neurath took the epistemology of history and the historical perspective on knowledge as important issues which could no longer be ignored by logical empiricist philosophers.⁹⁵ For him it was a crucial aspect for the future of the

⁹² "Aber es ist nicht gut, die traditionelle Bedeutung schleicht sich ein und verwirrt alles. Und zwar nicht nur die Leser, sondern oft sogar die Verwender. Denn selbst starke Menschen verfallen leicht dem Wortidol." Neurath to Hempel, 25 March 1935, Nr. 244 VCA

⁹³ "Es ist aber praktisch nicht möglich, sich bei jedem Terminus vorher diesbezüglich zu informieren; wenn ich nach Ihrem Rezept verfahren wollte, so müsste ich vor der Einführung irgend eine philosophischen Ausdrucks jedesmal erst lange Studien treiben darüber, wer diesen Ausdruck etwa verwendet hat und in welchem Sinne. Das würde mir als eine sinnlose Zeitverschwendung erscheinen." Hempel to Neurath, 22 March 1935, Nr. 244 VCA

⁹⁴ "Wieso ist das eine sinnlose Zeitverschwendung sich mit der Orientierung über Termini, die man selbst verwenden will, soweit zu beschäftigen, dass man z.b. zwei oder drei Bücher anschaut, wo der Terminus vorkommt? Das verstehe ich nicht. Das ist doch ein guter Anlass sich einmal mit der Geschichte der eigenen Wissenschaft vertrauter zu machen, als man ist." Neurath to Hempel, 25 March 1935, Nr. 244 VCA

⁹⁵ Neurath also applied this historicizing, "Machian" attitude himself in his early work on political economy and economic history (Nemeth 2007, 287).

movement that they further articulate these issues. Hempel, on the other hand, largely shared the disinterestedness of his former teacher, Hans Reichenbach, and he tried to avoid taking up any position on the issue. When Neurath in 1935 asked Hempel's opinion on a paper that was written by Schlick on Windelband's and Rickert's philosophy of history, Hempel did not respond. However, when Neurath asked the same question about Popper's book *Logik der Forschung*, Hempel wrote multiple letters on the topic. Clearly, Neurath's plea was not having its desired effects on Hempel.

2.4.2.2 Schlick's Disconsolate Contribution

Neurath mentions Schlick's paper on Windelband and Rickert in three different letters to Hempel as a "disconsolate contribution to *Erkenntnis*"⁹⁶, and wondered what Hempel thought about it. To Neurath, the Schlick paper was evidence of what was going wrong in the movement. "Schlick appears to lack any acquaintance with modern [historical] inquiries, and he seems not to know that historical investigations have also changed over time just as other things, and that they have considered new aspects as important."⁹⁷ Neurath's reproach is thus twofold: Schlick does not want to incorporate the historical sciences in his epistemology, and Schlick does not respect the important historical developments in the historical sciences. In a second long letter to Hempel on the subject, Neurath thought it unfair when a philosopher used the best examples when physics was concerned, but only examples from primary school for historiography.⁹⁸ Even though Hempel still failed to reply, Neurath revisited the issue again one month later, exclaiming that "he cannot get over the fact that [Schlick] says that one cannot write history any differently than Thucydides."⁹⁹

In his paper published in *Erkenntnis*, Schlick had claimed that a modern historian could not principally inquire into her subject matter differently than Thucydides (Schlick 1934, 391). Neurath was right in his judgment that Schlick did not give much evidence to back up this claim: Schlick certainly did not discuss any contemporary work in historiography. Schlick's paper did, however, contain an articulated position on the issues that had driven Viennese philosophers, including Neurath, into disagreement with Reichenbach four years

⁹⁶ "den trostlosen Ausführungen Schlicks", Neurath to Hempel, 5 February 1935, Nr. 244 VCA.

⁹⁷ "Er scheint wirklich nichts von modernen Untersuchungen zu kennen, und dass die Geschichtsdarstellungen ebenso sich ändern, wie andere Dinge, neue Sachen wichtig fanden." Neurath to Hempel, 5 February 1935, Nr. 244 VCA.

⁹⁸ "Ich habe es nicht gern, wenn man beim Exemplifizieren, die Physik in ihrem besten Exemplaren verführt, während man die Geschichte der "Taferlklasse" behandelt." Neurath to Hempel, 8 February 1935, Nr. 244 VCA.

⁹⁹ "Und wenn ich mal so anfangen, so ist mir es quälend, dass Schlick sich so völlig kritiklos in Soziologie und Geschichte verhält. Nichts liest, was darauf Bezug hat, wenn es nicht ungefähr so alt ist, wie Methusalem oder so verschmeckt und metaphysiziert, wie Windelband und Rickert. Ich kann nicht drüber hinweg, dass er erzählt, man könne Geschichte nicht viel anderes schreiben, wie Thukydides usw. und dass er als Beispiel des Ableitens nur drei Momente kennt: klima, BODEN und FUEHRER." Neurath to Hempel, 7 March 1935, Nr. 244 VCA.

before. Schlick argued that the aim of philosophy was to create a worldview [Weltanschauung] from the image of the world [Weltbild] as it was given by science (Schlick 1934, 379). According to him science, on the one hand, strives for a complete, single image of the world (a set of all true sentences about reality) (Schlick 1934, 381–82). Philosophy, on the other hand, strives for a clarification of the meaning of scientific knowledge. Consequently, philosophy does not add new ideas, it only provides a worldview so that one can understand science [dass man es versteht] (Schlick 1934, 384). Schlick partly follows Windelband's story: since the 19th century the cultural sciences have a rightful claim to be incorporated as a valuable part of knowledge. In opposition to Windelband, Schlick maintained that there is only one type of knowledge. The historical sciences do not warrant "a big revolution at the scope of the world view" (Schlick 1934, 391). Historiographers do not need new principles or concepts to perform their work. On Schlick's account, if Rickert and Windelband were right that historiography was only concerned with the singular facts, then these facts could not have an impact on the worldview, since the production of the worldview in the understanding [Verstand] necessarily relied on general concepts (Schlick 1934, 394). Consequently, Rickert's and Windelband's idea of history as a value-relating intuition could never produce images of the world in the understanding, only the laws of the psychological life [Gesetze des Seelenlebens] could perform such function, and these could be reduced to the natural sciences (Schlick 1934, 391). Schlick concluded:

There is no separate natural scientific and cultural scientific world view, neither a scientific and a non-scientific one. There is only the worldview and it originates from the philosophical clarification of the image of the world. This clarification has been drawn by the understanding. The knowledge of nature is the means with which the understanding operates.¹⁰⁰ (Schlick 1934, 394)

It is no surprise that Neurath was upset with Schlick's defense of the unity of science against Windelband and Rickert: Schlick uses the metaphysical notion of the understanding to argue that Rickert's singular historical objects have no influence on a "worldview" [Weltanschauung] produced by the understanding. Along the way, Schlick used the 2400 year-old Thucydides as an example of good historical practice. In Neurath's eyes, this is not scientific philosophy, but poor metaphysics. In 1935 Neurath could not persuade Hempel to properly engage in historiography or to train himself in a historical perspective on knowledge, and Neurath could not agree with Schlick's articulated position on the relation between history and philosophy.

¹⁰⁰ "Es gibt nicht eine naturwissenschaftliche und eine geisteswissenschaftliche Weltanschauung, ja es gibt nicht einmal eine wissenschaftliche und eine nicht-wissenschaftliche, sondern es gibt nur *die* Weltanschauung, und sie entsteht durch philosophische Deutung *des* Weltbildes, welches *der* Verstand gezeichnet hat. Das Mittel, dessen er sich dabei bedient, ist *die* Naturerkenntnis."

2.4.2.3 Other Allies

Between 1935 and the outbreak of the war, Neurath would attempt to incorporate other allies in his quest to articulate a proper position on history within an empiricist view of unified science. One of those allies was Felix Kaufmann, at that time a private lecturer at the University of Vienna in legal philosophy. Kaufmann started writing to Neurath in 1935 about a book that he was working on, *Methodenlehre der Sozialwissenschaften* (Kaufmann 1936). Specifically, Kaufmann wanted to better understand Neurath's physicalist theory of the social sciences in order to grasp the methodological difference for research in the social sciences implied by Neurath's position.¹⁰¹ Kaufmann proposed to write a contribution for *Erkenntnis* that would investigate how Carnap's and Neurath's physicalist position on the social sciences differed from his own views.¹⁰² Neurath welcomed such contribution and wanted to review Kaufmann's book in *Erkenntnis*.¹⁰³ Kaufmann believed that "social theoretical analyses of neighboring movements should also be published in *Erkenntnis*."¹⁰⁴ They decided to send in some kind of discussion section for *Erkenntnis*, with Neurath's remarks and Kaufmann's answers. Carnap was alerted as editor of the journal and agreed on the idea.¹⁰⁵ Even though Neurath "had ploughed through the book many times" and was set on writing a response, he never got to finish the work.¹⁰⁶

By June 1937 Neurath was fully engaged as editor of the *Encyclopedia of Unified Science*. Consequently, he proposed to Kaufmann to publish a part of their discussion on a physicalist social science in the *Encyclopdia* – an idea that Kaufmann applauded.¹⁰⁷ However, six months later Neurath was still not finished with his first discussion note. "I have everything together. I just have to write it down. You will get it soon. [...] Why can't one multiply himself?"¹⁰⁸ By then, Neurath reported that *Erkenntnis* could no longer be published in Germany, and consequently their whole idea to start a discussion in that journal on the social sciences came undone. In 1938 Kaufmann became involved in a process of migration and Neurath was absorbed in his editorial work on the Encyclopedia volumes. When Neurath fled Holland in May 1940, a manuscript of 12 pages was left behind on

¹⁰¹ Kaufmann to Neurath, 10 July 1935, Nr. 255 VCA.

¹⁰² Kaufmann to Neurath, 9 October 1935, Nr. 255 VCA.

¹⁰³ Neurath to Kaufmann, 14 October 1935, Nr. 255 VCA.

¹⁰⁴ "Ich glaube, dass in der 'Erkenntnis' auch die sozialtheoretischen Analysen den Gebührenden Platz finden sollten." Kaufmann to Neurath, 22 October 1935, Nr. 255 VCA. Kaufmann considered his own viewpoint as outside of, but nonetheless related to the logical-empiricist movement. Consequently, he describes himself as a neighbor [Gebührende].

¹⁰⁵ Neurath to Kaufmann, 30 October 1935, Nr. 255 VCA.

¹⁰⁶ Neurath to Kaufmann, 11 June 1937, Nr. 255 VCA.

¹⁰⁷ Ibid.; Kaufmann to Neurath, 16 June 1937, Nr. 255 VCA.

¹⁰⁸ „Ich habe schon alles beisammen. Ich muss nur einmal die Sache niederschreiben. Sie bekommen das bald. ... Warum kann man sich nicht multiplizieren?“ Neurath to Kaufmann, 21 January 1938, Nr. 255 VCA.

Kaufmann's book: it was a preliminary discussion of several passages of the work, apparently written during the summer of 1936.¹⁰⁹ Kaufmann would never get to see those pages. Just before Germany's surrender, on 2 May 1945, Neurath revisited their project in a renewed correspondence with Kaufmann: "I should like to write some day about your book, which I did not finish".¹¹⁰ Neurath even proposed to arrange a symposium on Kaufmann's book and Neurath's own *Foundations of the Social Sciences* "in whatever periodical you like."¹¹¹ Even though Neurath constantly renewed his intentions, they were never actualized. He died on 22 December 1945. Neurath's interaction with Kaufmann, however, testifies of Neurath's agenda to promote attention to history and the social sciences within the logical empiricist network, both in *Erkenntnis* and later in the *Encyclopedia*. Neurath also wrote to Kaufmann about Hempel and Oppenheim's book, which had started his discussion with Hempel. Kaufmann replied: "I was especially happy with your warning [to Hempel and Oppenheim] to study the history of logic. I think we are in agreement that this postulate towards historical-philosophical studies should be generally extended."¹¹²

Another ally of Neurath who actively tried to think about sociology from an empiricist and anti-metaphysical perspective was Richard von Mises. As a member of the "first" Vienna Circle around Hahn, Neurath and Frank that had been active in Vienna before the first world war, and as an adherent of Machian philosophy, von Mises knew Neurath well. After 1933 von Mises, a mathematician and civil engineer, had found refuge at the University of Istanbul, just like Hans Reichenbach. On 3 December 1936 von Mises reported that he was working on a book about positivism, which naturally caught the attention of Neurath, especially when von Mises wrote that he was writing on questions concerning the social sciences and asked Neurath for an update on what he had already written on this topic.¹¹³ When von Mises informed Neurath that his planned publisher, Springer, was no longer able to publish the book, Neurath and von Mises immediately agreed to publish the text in Neurath's planned book series *Einheitswissenschaften* with the Dutch publisher Van Stockum & Zoon.¹¹⁴ Since Neurath now acted as editor of the text, he did not want to influence von Mises' content. However, he could not restrain himself from

¹⁰⁹ Ad Felix Kaufmann, Methodenstreit, Nr. 212 K.118 VCA.

¹¹⁰ Neurath to Kaufmann, 2 May 1945, Nr. 255 VCA. Neurath probably refers to the fact that he never finished his review/discussion of Kaufmann's book.

¹¹¹ Neurath to Kaufmann, 7 July 1945, Nr. 255 VCA.

¹¹² "Besonders hat mich Ihre Mahnung gefreut, Geschichte der Logik zu studieren. Ich glaube wir könnten uns darüber einigen, dass dieses Postulat auf philosophie-geschichtliche Studien im allgemeinen ausgedehnt werden sollte." Kaufmann to Neurath, 27 January 1938, Nr. 255 VCA.

¹¹³ Von Mises to Neurath, 9 December 1937, Nr. 268 VCA.

¹¹⁴ Von Mises to Neurath, 30 June 1938, Nr. 268 VCA; This agreement resulted in the publication of publication of the book one year later: (Von Mises 1939).

making some remarks on the manuscript, especially concerning von Mises' discussion of Neurath's own views on Marxism. Neurath was concerned that von Mises misrepresented his views: Neurath only believed that Marxist sociology had historically been the first type of empiricist sociology, and that it was a step in the right direction, against metaphysical sociologists like Sombart or even Weber.¹¹⁵ Von Mises, consequently, made some changes to his manuscript to better account for Neurath's position.¹¹⁶ Neurath's remarks to von Mises again show that an articulation of a logical-empiricist view on the social sciences was something at stake for Neurath, something that he would continue to promote wherever he could.

However, Neurath was also concerned to deal with the second aspect of the stakes surrounding history, namely to develop historical views on knowledge. To this end, Neurath solicited several authors to write an Encyclopedia monograph on history of science. Initially, Neurath had envisaged the Italian mathematician Federigo Enriques to write such a monograph. When Enriques alerted Neurath in September 1938 that he would not be able to finish the planned book, Neurath attempted to enlist a well-established historian of science, George Sarton.¹¹⁷ Sarton was a Belgian scientist who had migrated to the United States in 1913 and became a lecturer in the history of science at Harvard University (Garfield 1985, 113). Sarton was also the founder of *Isis*, an international journal for the history of science, and shared a strong belief with Neurath that science had the capacity to unite the world in a global peace. Neurath invited Sarton to the 1936 Unity of Science conference in Copenhagen, an invitation that Sarton declined.¹¹⁸ In a second attempt Neurath did get Sarton's interest, when he reported on his *Encyclopedia of Unified Science* project. Sarton replied: "I am deeply in agreement with you as to the need of unification in this mad world".¹¹⁹ Sarton also promised Neurath to have an elaborate review ready in *Isis* when the first completed volume of the Encyclopedia was finished. Furthermore, they agreed to exchange advertisement in their respective journals.¹²⁰ On Neurath's request Sarton also agreed to become member of the advisory committee of the International Encyclopedia of Unified Science. After Sarton's acceptance, Neurath asked him to write a

¹¹⁵ Neurath to von Mises, 24 May 1939, Nr. 268 VCA.

¹¹⁶ Von Mises to Neurath, 5 June 1939, Nr. 268 VCA.

¹¹⁷ Enriques to Neurath, 30 September 1938, Nr. 232 VCA; In the 1930's and 1940's Neurath also attempts to solicit a historical volume from I.B. Cohen. This attempt, however, similarly failed, see (Fuller 2001, 286). Neurath also solicited for a monography on the sociology of science with Louis Wirth, member of the Chicago school of sociology – an attempt that would also fail. Eventually the Encyclopedia would receive a monograph on the history of science only 24 years later, namely the *Structure of scientific Revolutions* by Thomas Kuhn. The relation of Kuhn to logical empiricism is a story in its own right, see (Reisch 1991).

¹¹⁸ Neurath to Sarton, 15 February 1936, Nr. 298 VCA.

¹¹⁹ Sarton to Neurath, 26 July 1938, Nr. 298 VCA.

¹²⁰ Sarton to Neurath, 2 November 1938, Nr. 298 VCA.

volume on the history of science, specifically on “the manifold importance of the history of science for an encyclopedic organization of knowledge.”¹²¹ Such a volume would require “an appendix which shows the ways in which the history of science can be worked out by means of certain scientific devices.”¹²² Neurath thought such a contribution was a necessary element in his Unity of Science movement:

Personally I regard the history of science as a very important factor in our analytic studies. It is not a mere accident, in my opinion, that Ernst Mach, Duhem and others were so extremely interested in the history of science. I think continual comparison between different theories easily leads to logical analysis and therefore the history of science is good preparation for the logic of science.

The history of science also plays another part in our encyclopedic work; it is a discipline in itself and it is very useful to understand the evolution of the sciences as the product of the efforts of Mankind. I always was very impressed by your immense work, which enables us for the first time in history to see horizontal sections through the history down the ages.¹²³

From his interaction with Hempel in 1935 we already know that Neurath considered a historical perspective on science as a necessary element in the logic of science. His request to Sarton proves that he actively attempted to incorporate this aspect within the movement of logical empiricism itself, even when Hempel, Oppenheim or Carnap at that time did not share his belief about the added value of history of science for logical analysis.¹²⁴ Neurath's long paean to Sarton on the history of science did not grant him success: even though Sarton would “gladly write the article”, he declined the monograph proposal due to a lack of time.¹²⁵

From 1937 onwards Neurath also started to correspond with Heinrich Gomperz on the role of the historical sciences within the Unity of Science movement. Gomperz was a former professor in the history of philosophy at the University of Vienna, who had taken up a position of visiting professor at University of Southern California in 1935.¹²⁶ When Neurath sent him the preliminary summary of the Encyclopedia program, Gomperz was baffled that “according to the program the historical sciences are completely left out ('social sciences'

¹²¹ Neurath to Sarton, 23 November 1938, Nr. 298 VCA.

¹²² Ibid.

¹²³ Ibid.

¹²⁴ Neurath had also conceived the encyclopedia project as a way to give the logical empiricist movement a historical consciousness of the fact that it was continuing an enlightenment tradition. However, this attempt did not find ground with his fellow contributors to the project. See (Dahms 2005, 116).

¹²⁵ Sarton to Neurath, 4 December 1938, Nr. 298 VCA.

¹²⁶ Gomperz was a former student of Ernst Mach and had his own intellectual discussion group in Vienna parallel to the Schlick circle. For more information on Gomperz' relation to the members of the Vienna Circle, see (Stadler 2015, chap. 7).

cannot treat of both sociology and the specific methods of actual historical research)".¹²⁷ Gomperz considered it a given that there were deep differences between historiography and the physical sciences. Consequently, "physicalism in all likelihood will not be helpful for history."¹²⁸ On Gomperz' account, knowing the physical, cerebral state of a reader cannot help to determine how to read a specific historical document. He proposed filling in this gap in Neurath's movement himself by writing a monograph on interpretation.¹²⁹

Since Neurath had always considered Gomperz's influence in Vienna as antimetaphysical, Neurath welcomed any potential cooperation.¹³⁰ Consequently, Neurath decided to consult his co-editor of the Encyclopedia, Charles Morris, on the possibility to add a volume. Unfortunately for Gomperz, it would turn out that there was no more room in the first program of the Encyclopedia. Nonetheless, Neurath advised Gomperz to give a talk on the matter during the upcoming fourth International congress for the Unity of Science in Cambridge, to get some more reactions from philosophers of the movement.¹³¹ Gomperz decided to attend the conference; his paper on interpretation would eventually be published in the 7th volume of *Erkenntnis* (Gomperz 1937). Just before the conference Gomperz explicitly wrote that he wanted to avoid all polemic.¹³² Afterwards, Neurath considered the paper as an inquiry into valid empirical questions on the nature of interpretation, even though he would refrain from using oppositions like causality - teleology.¹³³ When Carnap visited Gomperz in the United States, his judgement on Gomperz's paper was neutral, as can be expected from Carnap's general position on the historical sciences, namely that they are logically uninteresting. Gomperz reported that Carnap said: "no one can say anything against that."¹³⁴ So there was a general sense that Gomperz's ideas on interpretation could be a valuable intellectual addition. By that time, Neurath had already, similarly to von Mises, offered Gomperz a book publication in the complementary series *Einheitswissenschaften* that would be published by Van Stockum & Zoon in The Hague.¹³⁵ This short monograph actually appeared in 1939 under the title *Interpretation: a logical analysis of a method of historical research* (Gomperz 1939).

¹²⁷ "ich war ganz konsterniert darüber, dass nach diesem Programm die historischen Wissenschaften so gut wie ganz durchgefallen sind (denn Ihre 'Social science' kann doch nicht wohl neben der Soziologie auch noch die spezifischen Methoden der eigentlichen Geschichtsforschung behandeln)." Gomperz to Neurath, 8 November 1937, Nr. 240 VCA.

¹²⁸ "Was den sogenannten Physikalismus betrifft, so dürfte sich allerdings wahrscheinlich ergeben, dass er uns in die Geschichte wenig hilft." Ibid.

¹²⁹ Ibid.

¹³⁰ Neurath to Gomperz, 26 November 1937, Nr. 240 VCA.

¹³¹ Neurath to Gomperz, 7 January 1938, Nr. 240 VCA.

¹³² Gomperz to Neurath, 30 June 1938, Nr. 240 VCA.

¹³³ Neurath to Gomperz, 5 August 1938, Nr. 240 VCA.

¹³⁴ Gomperz to Neurath, 14 December 1938, Nr. 240 VCA.

¹³⁵ Neurath to Gomperz, 27 August 1938, Nr. 240 VCA.

Published just before the outbreak of the war, it had little or no impact inside or outside the network of logical-empiricist philosophers.

During their correspondence over Gomperz's publication, Neurath and Gomperz also discussed how to integrate an epistemology of the historical sciences into the Unity of Science movement. Gomperz believed that such integration would require a new tool, a logic of interpretation. Neurath, on the other hand, defended physicalism, but he would not compare historiography to physics. For Neurath this was a wrong reading of what physicalism implied. Instead, Neurath preferred a comparison between historiography and geology: investigating the origins of a mountain ridge bears important similarities to the investigation into the rise of institutions, and both can be based on observational reports.¹³⁶ Neurath also rebuked the idea that physicalism implied a reduction of historical concepts to brain activity.

We do not think that it is essential for physicalism to reduce human writing and thinking to cerebral appearances. We have the greatest distrust towards all "Brain mythology".¹³⁷

Neurath believed that Gomperz with his logic of interpretation meant a "historicizing scholars-behavioristic" [historisierende Gelehrten-Behavioristik]. And he wrote: "Who of us would oppose that, if someone with ample knowledge tells us how to successfully initiate such investigations?"¹³⁸

The defense of physicalism by Neurath was met with skepticism by Gomperz. The analogy with geology was, according to Gomperz, spurious: the fact that the geologist is concerned with chronology does not imply that geology and historiography are methodologically similar. That would be like inferring the equality between chemistry and history from the fact that researchers in both disciplines write books.¹³⁹ Gomperz intended to "analyze what historians really do."¹⁴⁰ He thought this was not in opposition with the Vienna circle, but with the abstract normative positions of Mill and Zilsel, who would have

¹³⁶ Neurath to Gomperz, 26 November 1937, Nr. 240 VCA; This comparison was already a major part of Neurath's 1931 monograph: the coverage of cultural groups over the face of the earth could be studied in a similar fashion as mountain ridges (Neurath 1931, 70).

¹³⁷ "Wir meinen nicht, dass es für den Physikalismus wesentlich ist das Schreib- und Denkverhalten von Menschen auf zerebrale Erscheinungen zurückzuführen. Wir haben gegen alle "Gehirnmythologie" grösstes Misstrauen." Neurath to Gomperz, 26 November 1937, Nr. 240 VCA.

¹³⁸ "Wer von uns sollte etwas dagegen haben, wenn Jemand, der das gut weiss, uns erzählt, wie man solche Untersuchungen jetzt erfolgreich anstellt [...]" Ibid. Neurath used the term "Behavioristik" to denote an empirical metatheory of science and Neurath's own version of physicalism certainly was not a program of reduction. See (Uebel 2007b, 258).

¹³⁹ Gomperz to Neurath, 11 September 1938, Nr. 240 VCA.

¹⁴⁰ Gomperz to Neurath, 18 November 1938, Nr. 240, VCA.

historians aim for the formation of laws similar to the natural sciences.¹⁴¹ Gomperz' idea to start his analysis from historical work was very close to Neurath's own remarks to Hempel three years before and Gomperz's skepticism towards the validity of general laws in the historical sciences was also close to Neurath's position. Consequently, Neurath judged in the end that they were in fact close in their beliefs, even though he disagreed with Gomperz' negative attitude towards the Vienna Circle's physicalism and its relation to the historical sciences.¹⁴²

This disagreement from Neurath's side was met with disbelief from Gomperz, who found Neurath's constant use of "we" in his defenses annoying. For Gomperz it was clear that Carnap believed in the necessity of reducing concepts from history to the social sciences and eventually to psychology. Gomperz emphasized that Carnap had claimed this in his statement at the Paris conference of 1935 and repeated this statement in his contribution to the encyclopedia. "Why should one not have to interact with such a clear and direct proposal, without having to fear that an anonymous 'we' will rebuke you?"¹⁴³ On Gomperz's view, Neurath should either say that he disagreed with Carnap or that Gomperz had misread Carnap.

If someone wants to discuss something with me, then it has to be a specific person, to whom one can ascribe words and who can stand by his words, be it Neurath or Carnap or Jörgensen, or whoever. However, to put it bluntly, I refuse a discussion with a "We". It is impossible to advance the discussion, if in such a way scientific views are impurely mixed together, views that are somewhat close, but in fact distinct.¹⁴⁴

Neurath explained that his use of "we" referred to the scientific culture of his movement in opposition to an older, "sectarian" philosophical culture.¹⁴⁵

¹⁴¹ Gomperz to Neurath, 18 November 1938, Nr. 240, VCA. Gomperz explicitly mentions Zilsel as an intellectual opponent: he did not agree with Zilsel's idea that historians have to look for laws in the same way as physicists. This is especially interesting, since Gomperz was Zilsel's dissertation advisor (Nemeth 2007, 293; Zilsel 2000b, xli).

¹⁴² Neurath to Gomperz, 2 January 1939, Nr. 240, VCA.

¹⁴³ "Warum soll man sich nun nicht mit dieser klar und scharf dargelegten Ansicht auseinander setzen dürfen, ohne befürchten zu müssen, dass einen jenen anonyme "wir" zurechtweist?" Gomperz to Neurath 11 September 1938, Nr. 240 VCA.

¹⁴⁴ "Wenn sich jemand mit mir auseinandersetzen will, so soll es ein bestimmter Mensch sein, den man beim Wort nehmen kann und der dann bei seines Wort steht, es sei nun Neurath oder Carnap oder Jörgensen, oder wer immer. Aber eine Auseinandersetzung mit jenen "Wir", das muss ich schon offen sagen, lehne ich ab. Es kann m.e.s [unclear] der Sache unmöglich zugutekommen, wenn auf solche Art einander vielleicht irgendwie nahestehende, aber tatsächlich doch voneinander verschiedene wissenschaftliche Anschauungen unreinlich verschmiert werden." Gomperz to Neurath, 11 September 1938, Nr. 240 VCA.

¹⁴⁵ The idea that the Unity of science movement was aimed at open and collective collaboration is a well-established aspect of how actors in the movement described their work and their relation to each other (Stadler 2015, 66).

Every philosopher develops his specific theses, sharpens them so that he is distinguished from others. His pupils continue this process. Divisions among them arise. In the "Vienna Circle" many believe that what we do in connection to Mach, Poincaré, etc. is a cause that develops itself in general. [...] It is about arguing among persons that have a common direction, just as in physics.¹⁴⁶

In another letter Neurath explained that Gomperz should not think of the Vienna Circle in terms of a school, but as a specific, scientific way of composing oneself.¹⁴⁷ Gomperz, however, was never convinced by Neurath's attempted formation of a collective movement. He underlined the fact that the Vienna Circle was confronted with an actual theoretical difference over the position of the historical sciences. On Gomperz's account, such differences are not a bad thing. If one has a truly historical view of scientific developments, then such differences are the signs of progress in an intellectual environment.

In my opinion it is in the nature of things that in general a school or a movement is confronted with differences of opinion and divisions, if it progresses in general. Of course, the focus of attention shifts from the evident, old cabbages to specific questions that have not been thought through. This is called historical development, and it is a good thing. For as long as there is no danger of uniform agreement over all questions, the scenario will be avoided, where people ruminate the old cabbage and thus abstain from innovative ideas.¹⁴⁸

2.5 Conclusion

With the benefit of hindsight, it is clear that Gomperz was right in the end. Neurath's movement was divided on both aspects of Windelband's challenge: the epistemology of

¹⁴⁶ "Jeder Philosoph entwickelt besondere These, spitze sie zu, so dass er sich von anderen möglichst unterscheid. Die Schüler machten das weiter so, es kam zu Spaltungen, Zuspitzungen usw. In "Wiener Kreis" fanden viele, dass das, was wir im Anschluss an Mach, Poincaré, usw. taten, eine Sache sei, die sich allgemein entwickle. [...] Es handle sich um das Argumentieren von ähnlich gerichteten Menschen, wie man das ja in der Physik kennt." Neurath to Gomperz, 1 November 1938, Nr. 240 VCA.

¹⁴⁷ Neurath to Gomperz, 2 January 1939, Nr. 240 VCA.

¹⁴⁸ "MES liegt es in der Natur der Sache, dass in einer Schule oder Richtung in dem Maße, als sich ihr Gemeinsames durchsetzt, Meinungsverschiedenheiten und Spaltungen auftreten. Denn die Aufmerksamkeit verschiebt sich natürlich von dem zur Selbstverständlichkeit werdenden alten Kohl auf die noch nicht hinreichend durchgedachten Einzelfragen. Das nennt man eben historische Entwicklung, und es ist gut so, denn da es eine vorgängige Gewähr für Übereinstimmung ueber alle Einzelfragen nicht gibt, so liesse sich jene Entwicklung nur dadurch vermeiden, dass beständig der alte Kohl wiedergekaut und jedes Weiterdenken unterlassen wird." Gomperz to Neurath, 18 November 1938, Nr. 240 VCA.

historiography (stake I) and the role of a historical perspective or knowledge (stake II). Viennese philosophers disagreed with Reichenbach, Neurath disagreed with Hempel, and Schlick had his own specific position. All actors in the network believed that the cultural or social sciences had to be accounted for in some way. However, their level of commitment to this belief varied. Reichenbach and Schlick were the least interested to incorporate perspectives on these sciences: Reichenbach was solely focused on creating institutional space for natural scientific philosophy, and Schlick believed historiography offered no epistemological challenge. Carnap was engaged in his early work to account for the social sciences within the unity of science. However, because Carnap believed that there were no logical problems for the social sciences in particular, he increasingly left the creation of a perspective on these sciences to his specialized colleagues and abstained from writing anything substantive on these fields. After the *Aufbau*, he would never revisit the incorporation of the cultural sciences in the logical unity of science, and whatever he would say about the social sciences in the 1930s, would hint only at the terminology introduced by Neurath. Both Reichenbach and Carnap were engaged in the logic of science abstracted from scientific practice, and, consequently, they were not concerned with practical problems about terminology in the social sciences. Neurath was directly opposed to this attitude and he would constantly attempt to both renew attention for the social sciences and proliferate a historical perspective on scientific knowledge. Most of his practical attempts failed: his discussion with Kaufmann never became public, Gomperz's publication remained largely unknown and the monograph for the Encyclopedia on the history of science was never written during Neurath's lifetime.

From 1935 onward Neurath also attempted to persuade Hempel, as an important member for the future of the movement, of the fact that a logic of science requires a historical perspective on knowledge. However, under the influence of Reichenbach's and Carnap's example, Hempel was not easily persuaded. For him, a logical analysis of science could be given independent of a historical perspective on science. Though Hempel agreed that both perspectives on science were meaningful, he took them to be independent from each other, just as Carnap and Reichenbach did. Between 1935 and 1939 Neurath discussed Hempel's focus on formal logic multiple times without much success. Hempel abstained from a historical investigation of scientific terminology. Through his discussion with Neurath, Hempel was nonetheless informed extensively of Rickert's and Windelband's position on a distinction between the historical and the natural sciences. After Hempel's migration to the United States in 1939 this extensive discussion would eventually lead to the 1942 paper and renewed discussion between Neurath and Hempel.

Chapter 3

New York

If only the world were not so depressive. But, as you know, I always emphasize that Leonardo was an architect of forts, Malus was a pest doctor during the Egyptian campaign, Descartes was an artillery officer, Lavoisier was beheaded, likewise Condorcet, Comenius had to flee from Prague over Poland to Holland and even Grotius and Leibniz performed their work in dark times.¹

The advancement of human knowledge does not come to a halt during turbulent times. In this message to Hempel, Neurath added the “striking” fact that the *Encyclopedia of Unified Science* was successful despite such a turbulent time, or possibly exactly because of it. In the wake of the rise to power of Nazism and Fascism, almost every German speaking member of the logical empiricist network migrated out of their home country. Most of them ended up in the United States. While Neurath was right to emphasize that this phase of migration would not halt the intellectual advancement of logical empiricist philosophy, he would come to realize by 1945 that the intellectual climate of the United States had changed the members of his movement. The issue of scientific explanation was one of the results that this migration would bring about, and as will be shown below Neurath was explicitly worried about its introduction.

In this chapter I discuss how the migration of philosophers to the United States brought about the preliminary discussion over explanation that would evolve into Hempel and Oppenheim’s 1948 paper. The central protagonists in this chapter are Carl Hempel, Edgar Zilsel and Paul Oskar Kristeller, all three of whom arrive in New York during the first three months of 1939 looking for employment. Due to the specific social and intellectual context

¹ „Wenn nur die Welt nicht so bedrückend wäre. [...] Aber Sie wissen ich wiederhole immer, dass Leonardo ein Festungsbaumeister war und Malus ein Pesthold während des aegyptische Feldzugs, Descartes ein Artillerieoffizier, Lavoisier geköpft wurde, Condorcet nicht minder, Comenius von Prag nach Polen von da nach Holland ziehen musste, dass Grotius, Leibniz usw in trüben Zeiten wirkten usw. Es ist doch eigentlich bemerkenswert, dass unsere Enzyklopädie in so schwieriger Zeit (ich würde sagen gerade deswegen) erfolgreich ist.“ Neurath to Hempel, 16 March 1939, Nr. 245 VCA.

in which they end up, these three philosophers would become involved in an important ongoing philosophical discussion about historiography in the network of New York philosophers. This discussion is hard to recognise in hindsight, since the respective research interests that brought all three migrants to the same debate during the Second World War, would refract after the war into three relatively distinct institutional and disciplinary enterprises, namely Philosophy of Science (Hempel), History of Science (Zilsel) and History of Philosophy (Kristeller). Such disciplinary refraction obscures how the writings of these three philosophers are in fact related to each other.

3.1 Off the Boat

The aforementioned three disciplinary distinctions were, however, not yet mutually exclusive when these philosophers stepped off the boat in 1939. All three were unemployed at the time and looking for a job opportunity, preferably in philosophy, at a University or College. Carl Hempel arrived on the 2nd of February 1939 and was immediately welcomed by Ernest Nagel. Hempel was no stranger to the American philosophers who were active in the logical empiricist network: as an assistant to Carnap, Hempel had spent the academic year 1937-1938 in Chicago and got to know many American philosophers who visited Carnap during that period. Upon his arrival in New York, Nagel instantly integrated Hempel into his New York philosophy network. Alongside his colleague Sidney Hook from NYU, Nagel organised an unofficial philosophical discussion group, where philosophers could discuss their new work with colleagues. This group was called the “Nagel-Hook circle” or “New York Philosophy circle”. Since New York was such a central hub of travel, almost every philosopher “passed through it at some time or other”.² Hempel indicated in his diaries that Philip Wiener, Jeraud McGill and Wiliam Malisoff were regular attendants.³ On 18th March 1939 Hempel lectured in the circle on “The Confirmation and Probability of empirical Hypotheses”, under the attendance of Wiener, McGill, Malisoff, Hook, Nagel and John McKinsey. At the invitation of Quine, Hempel had already presented this paper at Harvard University one week earlier. There, Hempel had been introduced to Nelson Goodman and Charles Stevenson. During Hempel’s initial stay in New York from February to April 1939, he also visited Nagel’s graduate and undergraduate courses in logic at Columbia University.

² (Eva) Hempel to Popper, 8 February 1941, CH 31-2 ASP.

³ Diary-entry, 18 March 1939, CH 2-1-1 ASP.

Thus, when Hempel left New York in early April 1939 to visit Carnap in Chicago, he had already met a substantial amount of American philosophers and had interacted with them about his work. After his presentation at the New York philosophy circle on 18 March, Nagel had approached him about a possible job prospect at City College New York, which eventually resulted in his first appointment as Instructor in philosophy for the evening hours at City College. By the end of June 1939, only five months after his arrival, Hempel returned from Chicago to New York to take up his first American position, lecturing on philosophy in the evening courses at City College New York. After one year, Hempel switched over to Queens College, where he would remain until his appointment at Yale University in 1948.

Edgar Zilsel arrived with his wife and son in New York on April 4, 1939. He was able to contact Max Horkheimer, at that time director of the International Institute of Social Research (IISR) in New York. Horkheimer decided to support Zilsel's efforts to find financial support for his research on the historical origins of science. In June 1939, around the same time as Hempel, Zilsel received good news about his job prospects: he was awarded a Rockefeller fellowship that would support his work for the next two years (Zilsel 2000b, xxii). Unlike Hempel, Zilsel was not immediately incorporated into Nagel's philosophy network. In a letter to Neurath, Nagel noted that he had only seen Zilsel once during his first five months in New York, at a discussion group with Horkheimer, Wiesengrund and Meyer Shapiro which Nagel afterwards thoroughly regretted attending.⁴ Neurath was worried about Zilsel's possible integration in his new country and he alerted Hempel about Zilsel's presence in New York.

A request. Please remind all our friends that they should take care of Zilsel, at least for the first eight weeks. Money is good, a job is good, but a loving environment as well. He really needs some warm company.⁵

Because Hempel was still in Chicago when Neurath wrote this in March, Zilsel was possibly disconnected from a stimulating environment during his first months in New York. However, by 1941 Zilsel was a regular attendant of the Nagel-Hook circle as well. Moreover, Zilsel and Hempel met regularly in New York between 1939 and 1943, both in their personal and professional sphere. In Hempel's diaries of 1939-40 and 1942 there are at least seven occasions where Hempel mentions that he met with Zilsel in private.⁶ Hempel

⁴ Nagel to Neurath, 21 July 1939, Nr. 275 VCA.

⁵ „Eine Bitte. Erinnern Sie alle Freunde daran, dass man sich um Zilsel wenigstens in den ersten acht Wochen kümmert. Geld ist gut, Job ist gut, aber liebevolle Umgebung auch. Er hat es sehr nötig, dass er gehegt wird.“ Neurath to Hempel, 16 March 1939, Nr. 245 VCA.

⁶ Hempel diaries, CH 02-1-1&3, ASP.

also reported to Neurath in 1942: “I think Zilsel is quite an original and stimulating person.”⁷ Likewise Ernst Nagel became a regular contact for Zilsel. Two years after his first remark on Zilsel’s presence in New York, Nagel noted to Neurath that he “has often discussed with Zilsel his views on the ‘social roots’ of science, without coming to any agreement. I think he simplifies the matter, though obviously he has a sound point to make.”⁸ Thus, when Zilsel left New York in 1943 to teach at Mill’s college in Oakland, California, he had been integrated in the same philosophical community as Hempel (Zilsel 2000b, xxiv).

Paul Oskar Kristeller arrived in New York on February 23, 1939 (Obermayer 2014, 501). Like Hempel, he was born in 1905 and had studied philosophy in interbellum Germany. Because Kristeller had taken philosophy classes at Heidelberg between 1923 and 1928, he had been trained in an intellectual climate that stood directly opposed to Reichenbach’s views on philosophy. At Heidelberg Kristeller was instructed amongst others by Heinrich Rickert and Ernest Hoffmann in historical philosophy, learning to discuss philosophy through history. Interested especially in ancient philosophy, Kristeller later pursued classical philology in Berlin, working with leading figures such as Werner Jaeger and Ulrich von Wilamowitz-Moellendorf. In 1926 he also spent a semester at Marburg to become acquainted with Heidegger’s philosophy. Kristeller followed two classes with Heidegger, *Basic Concepts in Ancient Philosophy* and *Exercises on History and historical Knowledge*.⁹ Heidegger was writing *Sein und Zeit* at the time, which heavily impressed the young Kristeller (Kristeller 1990). In 1928 Kristeller finished his doctoral dissertation on Plotinus under Ernest Hoffmann at Heidelberg – Hoffmann was himself specialized in the interpretation of Plato. After completing his education in classical philology in 1931, Heidegger agreed to oversee Kristeller’s habilitation project. He suggested Kristeller to work on Marsilio Ficino. In 1933, while Kristeller was investigating some manuscripts in Florence for the final stages of his project, the National Socialists rose to power and quickly enacted laws that prevented people with a Jewish background like himself from continuing an academic career in Germany. Fortunately for Kristeller he was well connected in Italy, and after a year as a German teacher in Florence, Giovanni Gentile got him a job at the Scuola Normale Superiore at the University of Pisa. Kristeller used his exile and new job to roam Italy in search for forgotten renaissance manuscripts. By 1938, however, racial laws had followed Kristeller to Italy. Again, he was forced to leave the country in search for another academic position. Arriving in New York in January 1939, Kristeller had the opportunity to teach on Plotinus at Yale for one semester, and was then offered a position

⁷ Hempel to Neurath, 19 December 1942, Nr. 246 VCA. Similarly, he reported to Kurt Grelling the following: “I must say that I find [Zilsel] very clear and stimulating in discussions, and that his recent articles on problems of the sociology of science strike me as far above average”. Hempel to Grelling, 1 August 1942, CH 44-1 ASP.

⁸ Nagel to Neurath, 11 October 1941, Nr. 275 VCA.

⁹ “Abgang Zuegnis Marburg”, 17 April 1926, 77-3 POKP.

as lecturer for one year at Columbia University, which resulted in a subsequent career at that institution (Kristeller and King 1994).

Through his training and experience Kristeller was by 1939 a young exponent of German academic historical philosophy¹⁰, just as Hempel was a promising young representative of German scientific philosophy. On the one hand, Kristeller was educated in the problems of historical knowledge by some of the most influential people in the field, Heinrich Rickert and Martin Heidegger. On the other hand he was meticulously trained by the best scholars of his age to use the precise tools of classical philology in interpreting philosophical texts of the past. By 1939 he was, moreover, experienced in searching for, editing and interpreting new manuscripts. Philologist, historian and philosopher all at the same time, moulding historical philosophy into a science of texts.

In the New York of the early forties Kristeller's expertise and erudition did not go to waste. In 1940 the *Journal of the History of ideas* (henceforth JoHI) was founded, and its managing editor, Philip Wiener from City College and regular attendant of the Nagel-Hook circle, advised his new colleague at Columbia to send in a manuscript for the new journal.¹¹ This resulted in Kristeller's first English paper in the third issue of the journal. His colleague in the history of philosophy at Columbia University and also chairman of the editorial board of JoHI, John Hermann Randall Jr., quickly started to send manuscripts to Kristeller for assessment. This led to Kristeller's swift appointment into the editorial board of the journal in 1943. In a letter of 22 May 1943 Randall explicitly noted that the board wanted Kristeller on board as a reward "for his multiple, valuable referee contributions".¹² Kristeller was a versatile work force, capable of handling everything in between history and philosophy. He could referee papers on post-kantian developments in German philosophy, and assess the novelty of a paper on Greek tragedy.¹³ At Columbia University, he was also able to lecture on Kant, Hegel, recent continental philosophy, the history of renaissance and ancient philosophy.¹⁴ Between 1940 and 1944 Kristeller wrote four contributions to the JoHI, all four on the history of renaissance philosophy. However, he would also write a contribution to an ongoing debate in New York philosophy on the relation between philosophy and history for the – also New York based – *Journal of Philosophy*.¹⁵ Kristeller's paper "Some Remarks on the Method of History", co-authored with the Columbia graduate student Lincoln Reis, was both a contribution that brought together some of the earlier topics of the

¹⁰ Kristeller had collected letters of reference from Heidegger, Cassirer, Hoffman and leading philologists as Werner Jaeger and Eduard Norden. Letters of Recommendation, 77-9 POKP.

¹¹ Wiener to Kristeller, 11 November 1939, 61-3 POKP.

¹² Randall to Kristeller, 22 May 1943, 46-1 POKP.

¹³ Kristeller to Randall, 16 April 1951 & 7 December 1951, 46-1 POKP.

¹⁴ Seminar notes on Continental Philosophy/Hegel, 1952-1953 & 1960, 115-6 POKP.

¹⁵ This journal was edited by three of Kristeller's philosophy colleagues at Columbia: Randall, Nagel and Herbert Schneider. As we will see below, it was probably common practice to ask participants of the Nagel-Hook circle to contribute their paper for the *Journal of Philosophy*.

New York debate on historiography and a reaction against the latest contribution to that debate, namely Hempel's "The Function of General Laws in History".

3.2 The New York Debate on History

Although there was no debate over explanation in science, or explanation in historiography in the philosophical academia of the United States in the thirties or forties, there was a question that philosophers and historians, especially in the New York network, were constantly struggling with. Between 1935 and 1943 the *JoP* had 17 contributions devoted to the philosophy of history - as a comparison, between 2008 and 2016 there was no publication on philosophy of history in *JoP*. These contributions all shared one common theme, how to conceive the relation between philosophy and historiography. In 1935 Eugen Rosenstock-Hüsey, another German philosopher trained at Heidelberg who had found refuge in the US, lamented the fact that modern history had lost its harmony with memory and tradition (Rosenstock-Hüsey 1935, 93). No longer did the historian aim her practice at healing the memory of a group. Instead, she was now "limited to rendering services to science alone" (Rosenstock-Hüsey 1935, 98). He attributed this limitation to the scientification of history by Rickert and the Neokantians (Rosenstock-Hüsey 1935, 99), and he concluded that one must overcome this epistemological betrayal of the true natural rights of history by recovering its proper function as healer of memory (Rosenstock-Hüsey 1935, 100).¹⁶ In 1936 the Neohegelian German philosopher Richard Kroner, who would migrate to New York in 1940, emphasized the necessity in philosophy to use history for understanding life itself, and the idea of man. A purely naturalist theory of life and man falls short. Instead one must study how man through the process of history has realized the idea of man (Kroner 1936, 210–12).

Such a message can also be found in a contribution of 1937 by Dorothy Walsh who was based at Bryn Mawr college: history can serve "a deep need for knowledge of [man's] own nature and that of his fellows" (Walsh 1937, 64). For Walsh history is both an autonomous theoretical practice, and at the same time the beginning of a deeper reflection on man as a creative and active being. This message was similarly reflected in the position that was taken up in 1937 by the Dilthey scholar Bonno Tapper from State University of Iowa: in understanding the historical and cultural processes physicalism fails, and consequently, one

¹⁶ This might seem surprising, that Rickert and Neokantian philosophers can be considered as transforming historiography into a science. However, this is how these philosophers were widely read at the time, even though logical empiricist philosophers believed that the neokantians had completely misunderstood what makes a discipline a science.

must understand these processes in the context of a spirit that objectifies itself in a living tradition (Tapper 1937). In a similar vein Sterling P. Lamprecht, a professor at Amherst college, argued that a philosophy of history needs to emphasize the non-determinist aspect of human agency (Lamprecht 1936, 204). For Lamprecht this also applies most adequately to the history of philosophy: as some kind of metaphysics, the history of philosophy removes parochialism from philosophy and enables one to “move in an infinite universe of discourse which contains all possible frames of reference” (Lamprecht 1939, 460).

The idea that history had a broader impact on a philosophical self-understanding of man and his ideas was clearly in vogue. Arthur Lovejoy took up the idea in his own contribution to the philosophy of history in 1939, just a year before he founded the *JoHI*: “The historian's, and especially the intellectual historian's, general and perennial problem is, as I have already intimated, the problem of human nature and human behaviour” (Lovejoy 1939, 484). Lovejoy, however, emphasized that this ultimate aim is limited by evidential material, that is “not biased by a fixation upon distinct, present problems” (Lovejoy 1939, 484). Philip Wiener, in his 1941 contribution to *JoP*, countered the “rationalist” and “organicist” tendencies in philosophy of history by arguing for a thoroughgoing naturalist empiricism. For him there are no eternal ideas or organic unities to be uncovered by history (Wiener 1941, 312). The proper philosophical inquiry into history is limited to four questions, about the historian's method of ascertaining evidence, about the inference of relevant antecedents to events, about the quest for causes, and about the search for larger tendencies that cover large spans of history (Wiener 1941, 312–13). Just as Lovejoy and Randall, Wiener was a historian of philosophy based at New York, specifically at City College. Two years before Wiener's contribution Sidney Hook had already defended a naturalist interpretation of history against what he called “dialectic” theories, covering both Spenglerian ideas of the West and dialectic-materialist theories (Hook 1939). Hook concluded that the rationale of scientific method should not be abandoned in history, which is “the quest for verifiable hypotheses, the deduction of consequences, experiment under controlled conditions, or where this is not possible, careful use of comparative method of agreement and difference” (Hook 1939, 378). Where and how to draw the boundary between empirical historiography and metaphysical philosophy was at stake for all these philosophers. Wiener and Hook both argued for a separation of empirical history from speculation about the ultimate essence of humanity, whereas Walsh, Lamprecht and Tapper had argued for a deep link between both enterprises.

A discussion over the aim of history equally involved the problem how to understand historiography on the epistemological level, in line with the natural sciences or in opposition to them. Rickert's problem of selection (the problem of intensive inestimability, see 2.1) was a central issue: the historian requires a valid, objective criterion to select which events from the past are relevant – she cannot simply describe everything. In her contribution of 1937 Walsh lamented that “it is a mark of complete misunderstanding of the historical enterprise to deplore the selective character of historical discourse” (Walsh 1937, 61).

Similarly Lovejoy claimed that the selective procedure in historiography was a truism: the historian always has to select (Lovejoy 1939, 479), and even Philip Wiener in his defense of naturalist empiricism emphasized that selection is a part of the evidentiary logic of historiography (Wiener 1941, 313). John H. Randall also noted in a contribution to the debate that the historian has to sift his infinite raw material to ascertain significant facts (Randall 1939, 467). Sidney Hook even uses Rickert's idea that values organize the materials of history, which he introduces as a valid idea against the illegitimate dialectical method that always portrays a value as an absolute, "telic" goal (Hook 1939, 374). Thus, when Hempel in 1942 would bring up the problem of selection and the possible solution of a value-relation, this issue was already part of the ongoing broader discussion, in which it was also associated with Heinrich Rickert.

Another aspect of the discussion was the role of generalizations in historiography – a problem that was, of course, central in the German debate. In the American context Lamprecht had noted in 1936 that whatever statistical correlations one finds between historical events, they could never reveal the particular agencies involved (Lamprecht 1936, 200). In 1939 Randall similarly emphasized that whatever historiography as historiography could do, it was always concerned with the particular and the unique (Randall 1939, 462). According to Randall, whenever structures, laws or invariant relations were concerned, it was the subject-matter of a general social science, not history (Randall 1939, 468). Philip Wiener did not exclude the possibility of forming generalizations in historiography, but noted the enormous difficulties for performing such empirical research given the impossibility to isolate factors and suitably compare events (Wiener 1941, 314). Hook also agreed to the principled possibility of acquiring comparative frequencies between events, but he stressed the need for "a vast statistical study of cultural dependencies which no one has ever adequately undertaken" (Hook 1939, 376). This shows that the role of general laws in historiography was already extensively debated in *JoP* before Hempel's paper.

The two German stakes surrounding historiography that had split most actors in the logical empiricist network in the 1930s were present in the New York intellectual circles in which Hempel, Zilsel and Kristeller arrived in 1939. The first stake, to articulate an epistemology for historiography, was directly present in most aforementioned contributions to the discussion, especially focused around the problem of selection and the problem of generalizations. The second stake, to articulate the effect of a historical perspective on epistemology, was mainly transformed in a struggle over the proper boundaries between a historical and a philosophical or speculative view on mankind. These topics were of central concern to the group of philosophers that held the Nagel-Hook meetings. Hook, Wiener and Randall had all recently published on philosophy of history. Whether Randall was ever present during these meetings, cannot be ascertained from the currently available sources on the circle. He was, however, editor of the *JoP* at the time and working at the philosophy department of Columbia. He shared both positions with Ernest Nagel, who had close contact with Hempel. Though Nagel had not yet participated in the debate on history himself, he

discussed issues concerning the “logic of history” in his Logic courses. In chapter 17 of his 1939 lecture notes he treats several related questions concerning the method of history, the authenticity of sources, the meaning of historical data and the problem of testimony.¹⁷ Hempel followed several of these courses upon his arrival in New York in 1939. In a list of possible topics for a graduate essay prize at Columbia that Nagel had comprised in 1941, he also puts issues like ‘Generalizations in the social sciences’, ‘the logic of historical interpretation’, and also ‘the logical structure of historical explanation’.¹⁸ Certainly, in the philosophy department of Columbia University historiography was a prime subject of interest at the time of arrival of Hempel, Zilsel and Kristeller.¹⁹

It is in the context of this New York debate on historiography that Hempel was invited to give a talk on history somewhere in 1941.²⁰ His contribution would later get published as the 1942 paper “The Function of General Laws in History”. Forty years later, Hempel could not recall what made him choose the particular topic (Nollan and Hempel 2000, 20). In a way, it was a peculiar topic for a man who had no real interest in historiography and lacked any training in that discipline. His earlier correspondence with Neurath could have played a role in convincing Hempel that he would have something valuable to add and that there were certainly things to say about historiography from a logical empiricist point of view against Rickert’s and Windelband’s views on historiography. Unlike the European situation in 1935, when Hempel had discussed these matters with Neurath, there was no logical empiricist voice which had already meddled in the New York debate concerning historiography. Clearly, now was the time for Hempel, as the young representative of logical empiricism in New York, to make a contribution to the stakes surrounding historiography that were present in his New York environment. As soon as his article was

¹⁷ 1939 Lecture Notes, Box 25, folder Miscellaneous, ENP.

¹⁸ Suggested Topics for the Jones Prize, November 1941, Box 25, folder Miscellaneous, ENP.

¹⁹ Noteworthy is the fact that Nagel’s earliest course on logic opened with a discussion of the several ways to understand a logic, formally, transcendently or psychologically. In this discussion Nagel also shows that several philosophers have used logic to denote a specific scientific methodology, and as an example of such an enterprise concerning historiography he refers to Rickert. “Notebook logic 101 - 102, 1933 – 1944”, chapter 1, section 6, Box 16, ENP.

On Nagel’s readings list for the logic of science course of 1944, one also still finds Ernst Cassirer’s *Substance and Function*. However, later reading lists for the same course in 1948-1949 and 1952-1953 have no reference to Cassirer anymore. This shows how someone like Ernst Cassirer was removed from the curricular field after the second world war. “Reading List, Logical Theory 1948-1949 & Logical Theory 1952-1953”, Box 21, ENP.

²⁰ Since the relevant portion of the Hempel diaries has not been preserved (or was never written), it is impossible to infer when he gave this talk at the circle, or who attended it. The only information available is the testimony from the autobiographical interview. There are also very few letters from Hempel in 1941, and none contain any reference to the talk.

published, he sent a copy to Neurath, eager to hear his thoughts on the matter that had been the focus of much of their earlier correspondence.²¹

3.3 Hempel's Contribution to Philosophy of History

Hempel's 1942 paper itself is a good example of the philosophical methodology that Hempel understands as logical empiricism during his initial years in the United States. When Hempel arrived in Chicago in April 1939, he was invited to give a layman presentation on logical empiricism at the Wind radio channel.²² In his radio talk, he attributed two main goals to logical empiricist philosophy. On the one hand, it performs a "critical analysis of unscientific methods in philosophy."²³ It was a critique of traditional, metaphysical forms of philosophy. On the other hand, "it consists in the development of what has been called a science of science; i.e. a study of the language of science, of the concepts used in the various branches of scientific research, of the methods by which hypotheses and theories are established and tested, and finally of the connections between the various branches of science."²⁴ According to Hempel, formal logic was the tool to execute this study of scientific language: through logic one could unambiguously reconstruct the relations between terms in the scientific language, in a way that one could not do through natural language. Hempel thus followed Carnap's 1935 identification of the logic of science with a study of the logical syntax of scientific language. For Hempel, this logic did not simply reconstruct the vague and unspecified natural language. It offered a clarification of natural scientific language which removed any of its vagueness or ambiguity. In correspondence with Reichenbach during his stay in Chicago in June 1939, Hempel clarified his methodological ideas about the analysis of science through logic. He criticized Reichenbach's wish to reproduce some ambiguities concerning singular probability statements from natural language in the logic of probability itself. Hempel claimed that one should not simply reconstruct natural language.

However, it appears to me that the logical analysis should clarify the everyday use of language, but it should not unconditionally reconstruct it. [...] As an analogy: The

²¹ This is mentioned in a later letter to Neurath. In 1941 and 1942 there are only two letters going back and forth between Neurath and Hempel. Presumably, the ongoing war prevented regular writing between the US and the UK. Hempel to Neurath, 19 December 1942, Nr. 246 VCA.

²² Hempel to Neurath, 27 June 1939, Nr. 245 VCA.

²³ Wind Radio Talk, May 1939, CH 54-1 ASP.

²⁴ Ibid.

term "heavy" is used in everyday life in sentences as "this backpack is heavy". The analysis then shows that this is a proposition of relations, that states something like "...is heavier than some kind of standard body" or "...heavier than a child can carry" or something of that kind. In a specified language one would, thus, replace the everyday one-place predicate with a two-place relation, and one would not continue to work with a one-place predicate after one had clearly seen [durchschaut] the essential imperfection of the one-place predicate.²⁵

In his example Hempel did not specify what it means to "clearly see" the essential imperfection of representing "heavy" as a one-place predicate in the formal target language. What end the clarification through logical reconstruction served, remained somewhat vague. However, the program of revisionist clarification through logical reconstruction was also the methodological backbone of the paper that Hempel conceived in 1941 to discuss historiography within the New York debate.

The paper had one major thesis to defend: "General laws have quite analogous functions in history and in the natural sciences, they form an indispensable instrument of historical research and they even constitute the common basis of various procedures which are often considered as characteristic of the social in contradistinction to the natural sciences" (Hempel 1942, 35). This thesis was directed against any ZER-like argument that defended the inapplicability of general laws to the historical sciences. In particular, it heavily focused on the purported idea that history is concerned mainly "with a description of particular events rather than with the search for general laws which might govern those events" (Hempel 1942, 35). To defend his thesis, Hempel first introduced a view on science and laws that is congruent with the Mach-Duhem anti-explanatory tradition and he gave this view a formal interpretation. The resulting, formal scheme was then used to progressively reconstruct several statements from the historical sciences. These reconstructions were supposed to show that a ZER position could not be maintained.

In the opening paragraph Hempel introduces a general law as "a statement of universal conditional form which is capable of being confirmed or disconfirmed by suitable empirical findings" (Hempel 1942, 35). Hempel also calls such statement a "universal hypothesis", which asserts a regularity over events of a specified kind C and E (Hempel 1942, 35). C and E are not individual events. They are instances of kinds of events: the assassination of Julius Caesar is an instance of a political assassination in general (Hempel 1942, 37). Whatever

²⁵ „Mir scheint doch, die logische Analyse sollte den Sprachgebrauch des Alltags klären, aber nicht unbedingt nachbilden. [...] Analogie: Im Alltagsleben wird der Term "schwer" in Sätzen gebraucht wie "Dieser Rucksack ist schwer". Die Analyse zeigt dann, dass es sich hier um eine Relationsaussage handelt, die etwa besagt "...ist schwerer als der und der Standardkörper" oder "...schwerer als ein Kind tragen kann" od. dgl. In einer präzisierten Sprache würde man dann doch den Eigenschaftsbegriff des Alltags durch einen Relationsbegriff ersetzen, aber nicht fortfahren, mit einem einstelligen Prädikat zu arbeiten, nachdem man dessen wesentliche Unvollständigkeit durchschaut hat.“ Hempel to Reichenbach, 20 July 1939, HR 27-25b-45 ASP.

one can scientifically say about the assassination of Julius Caesar is always in relation to general concepts that also cover other events. One cannot conceptualize an individual event within terms that can only apply to that event.

The complete description of an individual event would require a statement of all the properties exhibited by the spatial region or the individual object involved, for the period of time occupied by the event in question. Such a task can never be completely accomplished. (Hempel 1942, 37)

Hempel, thus, assumes that one cannot overcome what Rickert had called the intensive inestimability of the empirical manifold: the individual properties of an object are infinite, e.g. everything related to the reign of a certain king. While Rickert believed that this infinity could be overcome without conceptualizing the object as an instance of a general concept, Hempel denies this possibility outright. For Hempel one could only say something about such object or event as it is related to similar objects or events.

There is no difference between history and the natural sciences, because both give an account of their subject-matter only in terms of general concepts, and history can 'grasp the unique individuality' of its objects of study no more and no less than can physics or chemistry. (Hempel 1942, 37)

For Hempel, science has one main goal, namely to systematize our experience through laws: "The main function of general laws in the natural sciences is to connect events in patterns" (Hempel 1942, 35). Hempel says that these connections of events in patterns "are usually referred to as explanation and prediction" (Hempel 1942, 35). But, these are only names for the systematizing function of general laws. Though explanation as a term has often been used as an indication of the causes of an event, Hempel believes that any statement about *causes*, *explanation* or *prediction* should always be understood as a "statement that, according to certain general laws, a set of events of the kinds mentioned is regularly accompanied by an event of kind E" (Hempel 1942, 36). Here, the revisionist program of logical reconstruction comes to full light. Now, Hempel is in a position to introduce the central formal reconstruction of the paper: every statement about 'explanation' and 'prediction' in the sciences can be reformed as a deductive argument consisting of

- (1) A set of statements asserting the occurrence of certain events C_1, \dots, C_n at certain times and places,
- (2) A set of universal hypotheses such that
 - a. the statements of both groups are reasonably well confirmed by empirical evidence,
 - b. from the two groups of statements the sentence asserting the occurrence of E can be logically deduced. (Hempel 1942, 36)

Hempel calls (2a) an empirical condition of satisfactory confirmation, in contrast to the logical conditions. This Deductive-nomological scheme performs a revision of scientific

language in three important ways. First, it stipulates that every statement should be understood as a statement over instances of kinds of events or objects. Consequently, it takes for granted, in opposition to Rickert, that science cannot talk about individual events or objects, such as *the* one and only Kohinoor diamond.²⁶ Science can say something about the social function of diamonds in certain societies, but nothing about a specific diamond as a unique object. Again, this in line with the Mach-Duhem tradition: general concepts are developed in the sciences that can only cover multiple events. As Mach put it: “description is only possible of events that constantly recur, or of events that are made up of component parts that constantly recur” (Mach [1893] 1974, 6). Second, Hempel’s revision determines that every statement containing “cause”, “explanation” or “prediction” as a term should be understood as an argument of the above form. Later in the paper Hempel also adds other terms to this list: every statement containing terms like “interpretation of events”, “meaning of events”, “relevance of events”, “determination of events” should be transformed into the scheme. All such statements should be read as in fact referring to an argument containing (1) and (2), even if nothing like (1) and (2) can actually be found in the texts. Third, the reconstruction stipulates that elliptic formulations in natural language should always be extended. The reconstruction points to future avenues of research. Hempel knows that there are very little clear statements like (1) and (2) to be found in historical texts. However, based on the logical reconstruction, these absences should be interpreted as omissions to be filled out (Hempel 1942, 39–40). To capture this aspect of his reconstruction, Hempel introduces the notion of an *explanation sketch*, which “consists of a more or less vague indication of the laws and initial conditions considered as relevant, and needs ‘filling out’ in order to turn into a full-fledged explanation” (Hempel 1942, 42).

These three revisionist operations introduce a specific interpretation of scientific statements in general and historical texts in particular. Non-empirical, vague terms like *explanation*, *cause*, *interpretation*, *influence* can now be specified clearly and unambiguously as references to the logically transparent deductive-nomological scheme.

[Any reference in historical texts] to class struggle, economic or geographic conditions, vested interests of certain groups, tendency to conspicuous consumption, etc., all of them rest on the assumption of universal hypotheses which connect certain characteristics of individual or group life with others. (Hempel 1942, 41)

It is not because historians use words, like *tendency*, *interest* or *struggle*, that these words also bear an epistemic weight: Hempel’s logical revisionism brings all these statements back to the central idea that science systematizes events into patterns. Hempel is not much concerned with actual historical investigations – there is no reference to any historical work in the paper and the only actual example that Hempel discusses is highly problematic. In

²⁶ Events like the battle of Agincourt, can, on Hempel’s account, only be conceived as instances of 14th century battles, or medieval regal struggles or something like that. They cannot be conceptualised in their individuality.

order to show that certain sentences in historical texts can be reconstructed through the formal scheme, Hempel introduces the following passage from a handbook in economics (Girard 1939, 894–95).

As the activities of the government are enlarged, more people develop a vested interest in the continuation and expansion of governmental functions. *People who have jobs do not like to lose them; those who are habituated to certain skills do not welcome change; those who have become accustomed to the exercise of a certain kind of power do not like to relinquish their control – if anything, they want to develop greater power and correspondingly greater prestige....*

Thus, government offices and bureaus, once created, in turn institute drives, not only to fortify themselves against assault, but to enlarge the scope of their operations.²⁷

Hempel interprets this passage as an “attempt to explain the tendency of government agencies to perpetuate themselves and to expand.” He italicized one sentence to highlight the hypothetical generalization that could be recognized in this text. His example is, however, odd in several ways. The quoted passage in fact doesn't aim to explain anything. Instead it aims to characterize what the author of the chapter, Richard Girard, calls two types of government organization, the “vote calculus” in contrast to the “profit-and-loss calculus” (Girard 1939, 89–4–895). The vote calculus hinges on the fact that invested government power is most likely to perpetuate itself, because the required votes for government offices presupposes the possession of invested governmental power. In Girard's original text there is no explanandum available. Hempel's example is thus strange: the passage seemingly contains no reference to *explanation*, *interpretation* or *causation*, and its subject matter has nothing to do with history. Seemingly, Hempel is only interested in the passage because it shows that some sentences in certain texts can unambiguously be interpreted as universal hypotheses. The passage merely illustrates how the reconstruction could operate. Hempel's paper also contains a second example, the explanation of a cracked radiator pipe, which is a text about the cracking of a radiator pipe that Hempel wrote himself (Hempel 1942, 36). Hempel does not introduce this as an example of explanation in the sciences. He only introduces this text about the cracking of a radiator pipe as an idealized illustration to clarify his formal scheme in natural language (the illustration is very incomplete; it only states what kind of statements it ideally should contain, but lacks any quantitative statements concerning water pressure or bursting pressure of radiator material).

Clearly, the formal scheme is not meant to be a faithful representation of scientific results or texts. Hempel believes that his introduced formal scheme as a reconstruction of scientific language has several benefits and throughout most of the article his arguments are attempts to convince the reader of these benefits. Most importantly, the reconstruction transparently shows how the historical sciences can attain scientific objectivity. A formal reconstruction

²⁷ Quoted from (Hempel 1942, 40).

of a historical claim shows how the historical claim is amenable to objective checks of three kinds: an empirical test of the determining conditions, an empirical test of the universal hypotheses and a logical check of the deductive inference made (Hempel 1942, 38). If an attempted reconstruction cannot pass these checks, one can put the original claim aside. E.g. historical theories that contain teleological terms like “his mission in history” or “his predestined fate” can be now be proven deficient: their reconstruction does not survive empirical checks. “Accounts of this type are based on metaphors rather than laws” (Hempel 1942, 37–38). Similarly, Hempel attacks another famous concept from the German debate on historiography. In §6 Hempel brings up the alleged epistemic role of the method of empathetic understanding. As in the rest of the paper Hempel is not concerned with its original, epistemological purpose. He only summarizes the method in the following way:

The historian, we are told, imagines himself in the place of the persons involved in the events which he wants to explain; he tries to realize as completely as possible the circumstances under which they acted, and the motives which influence their actions; and by this imaginary self-identification with his heroes, he arrives at an understanding and thus at an adequate explanation of the events with which he is concerned. (Hempel 1942, 44)

Hempel then states that such a method is only a heuristic device that enables the historian to form certain psychological hypotheses about the historical actors. Such hypotheses can then be subjected to further empirical testing (Hempel 1942, 44). In German philosophy empathetic understanding was known as *Verstehen* – this notion was especially linked to Wilhelm Dilthey, a philosophical opponent of Windelband and Rickert in the German debate over the historical sciences. Dilthey's *Verstehen* was introduced as a distinctive feature of the epistemic practice of the historical sciences in opposition to the natural sciences. And at least one aspect of Dilthey's notion of *Verstehen* was exactly this self-identificatory procedure in the historian's imagination (Dilthey [1910] 1927, 7:220; Beiser 2012, 351). The reconstruction scheme allows Hempel to discard the method of *Verstehen*:

In history as anywhere else in empirical science, the explanation of a phenomenon consists in subsuming it under general empirical laws; and the criterion of soundness is not whether it appeals to our imagination [...], but exclusively whether it rests on empirically well-confirmed assumptions concerning initial conditions and general laws. (Hempel 1942, 45)

Hempel also targets the Windelband-Rickert tradition that was central in his correspondence with Neurath of 1935. In §7 the term “relevance” and its purported relation to values are discussed.

A description is not simply a statement of all the events which temporally preceded it; only those events are meant to be included which are '*relevant*' to the formation of that institution. And whether an event is relevant to the development [of an institution]

is not a question of the value attitude of the historian, but an objective question depending upon what is sometimes called a causal analysis of the rise of that institution. Now, the causal analysis of an event consists in establishing an explanation for it, and this requires reference to general hypotheses. (Hempel 1942, 46)

Hempel never states why one would consider the relevance of an event to a certain historical development the result of a value attitude. It is simply presented in the text as a naive position to which Hempel's own position is superior. As we have seen (2.1) the problem of deciding which event is relevant in a historical narrative, was central to Rickert's theory: the historian can decide which event or object from the past is relevant by relating it to a value of the historical period or institution under consideration. This notion of value-relation was also a central point of discussion in the New York debate on historiography. Hempel thus brings up Rickert's well-known solution to the problem of selection, but only to stress that a universal hypothesis can also function as a solution to the same problem: once an event can be related to a universal hypothesis through a logical inference, one can call it *relevant*. This is in line with the overall strategy of the paper: through a logical reconstruction which fits both history and the natural sciences, one can see how the epistemic distinction between two types of science from the German tradition, based on *relevance*, *teleology*, *understanding* or *value-relations* is philosophically unnecessary. This is also Hempel's conclusion:

It is unwarranted and futile to attempt the demarcation of sharp boundary lines between the different fields of scientific research, and an autonomous development of each of the fields. The necessity, in historical inquiry, to make extensive use of universal hypotheses is [...] just one of the aspects of what may be called the methodological unity of empirical science. (Hempel 1942, 48)

The paper, thus, defends the unity of science against the German separatists or Windelbandits, as Neurath called them. Hempel's logical reconstruction not only helps to remove a distinction between the sciences, it also helps to clarify what a law is. Since the inference in which laws operate is temporally neutral, the inference can also be used to represent predictions. If the event in the conclusion of the inference is not yet known to have occurred or has not yet occurred, one can call the inference a "prediction" (Hempel 1942, 38). According to Hempel, the difference between an explanation statement or a prediction statement is only pragmatic, related to the epistemic status of the agents involved. If the event was already known to have occurred, one calls the inference an explanation. If it was not yet known, the inference can be considered a prediction. The logical inference in which laws operate is entirely neutral towards this epistemic status. Thus, what one calls explanation or prediction, are in fact the same function of general laws in science: they systematize our experience of events through regularity statements. In the conclusion Hempel expands this into a broader principle of the theory of science: "the separation of

‘pure description’ and ‘hypothetical generalization and theory-construction’ in empirical science is unwarranted; in the building of scientific knowledge the two are inseparably linked” (Hempel 1942, 48). Entirely in congruence with the Mach-Duhem tradition, Hempel understands a general law and the theoretical concepts that it uses, as operations on the totality of empirical observations. One cannot describe events independent of the concepts and theoretical laws with which one systematizes them. The crucial question is, consequently, how to qualify generalizations given a set of observations: when are generalizations successful? However, this question is left entirely unanswered by Hempel in 1942. Hempel adds two further qualifications about laws in history. First, he notes that his formal scheme in principle also allows non-deterministic probability hypotheses instead of deterministic laws (Hempel 1942, 41–42). Again, this element is not worked out further. Second, Hempel believes that there are no specifically “historical” laws. Historians can use any lawful generalization between any possible object or event, as long as it can be checked by empirical tests of some kind (Hempel 1942, 47). Similar to Neurath, Hempel strictly denies any principled limitations on the regularities that historians can investigate.

A feature that stands out in the 1942 paper is Hempel’s constant use of the term explanation, 104 times. Hempel nowhere mentions that explanation is an aim of science, distinct from the aim of description, and he nowhere connects explanation to why-questions. As a term it is purely used to denote a specific understanding of science, namely that science systematizes experience through general concepts and general laws. Similar to Carnap’s remarks in 1931 on statements containing “Erklärung”, Hempel shows that statements containing the word explanation logically serve to connect phenomena or protocol sentences (from Carnap’s perspective in 1931). Explanation as a term only serves to denote this central function of general laws. Hempel’s logical reconstruction removes all possible ambiguity related to the term *explanation*, but also to terms like *cause*, *interpretation*, *influence* or *relevance*. In that sense, the reconstruction clarifies scientific language, especially in historiography and thus it executes Hempel’s own understanding of logical empiricist philosophy within the debates concerning historiography from the New York context. The paper would become the central focus of controversy in American philosophy of history for the next thirty years, and the point of discussion would be centered around the novel feature of Hempel’s paper, his constant use of the term explanation. All other contributions to the original New York debate would be forgotten, including the first reaction that was written against Hempel’s paper. This reaction was the product of the other German migrant active in New York philosophy with a background in German historical philosophy, Paul Oskar Kristeller.

3.4 Hempel's Foil: Paul Oskar Kristeller

"Some remarks on the method of History", co-authored with Lincoln Reis was the first reaction to Hempel's 1942 paper, which also appeared in *JoP*. In 1943 Lincoln Reis was a graduate student at Columbia University, working with Kristeller. He would later occupy a position at Bard College. Why exactly Reis co-authored the paper with Kristeller is unclear. Its content, however, reflects both the epistemological background of Kristeller as a Neokantian philosopher and his scholarly experience. I will, consequently, read it from that background, since it is hard to assess how much Reis actually influenced the content of the paper.

In the opening paragraph Kristeller and Reis cite Hempel's contribution first, and then refer with a "cf." to other papers (Reis and Kristeller 1943b, 225). This already shows that their prime target is foremost Hempel's paper, which is also confirmed by their specific arguments. Kristeller and Reis aim to counter the opinion that historians should draw general inferences from their facts and, consequently, aim to form general laws (Reis and Kristeller 1943b, 226). Stating that history as a social science is to follow the natural sciences in their search for general laws is a simple "lip service to the ideal of science", but this does not guarantee the application of the ideal to actual historical method (Reis and Kristeller 1943b, 225). Consequently, they "stress that function of history which deals with the description and reconstruction of specific, unique, and concrete events rather than with the formulation of general laws, which, of course, always would suppose a plurality of like instances" (Reis and Kristeller 1943b, 227). Kristeller and Reis state that the historian should aim at some kind of generality, but not in the same way as the natural or social scientist: she should look for a possibility of comparison between various local settings in time wherever possible, but not aim at regularities between kinds of events, or predictability through laws (Reis and Kristeller 1943b, 228). Thus, the introduction clearly targets Hempel's analysis, and rehearses some of the basic points from the Windelband-Rickert tradition.

The centerpiece of their argument, to be found in section II of their paper, is the complex procedure of the historian to ascertain facts or events from the past. Hempel assumed that the historian is simply capable of verifying events from the past (Hempel 1942, 38). Kristeller and Reis respond that a historical fact is never directly given, but the result of inferences and reconstructions based on source materials. These inferences are of a complex fourfold nature. The historian has to collect the data, select which data are relevant as evidence for the specific historical inquiry, then she has to evaluate her sources critically, and finally she has to reconstruct those aspects of the past that cannot be inferred from the sources (Reis and Kristeller 1943b, 235–38). This was not a new idea: they refer for this fourfold procedure back to several contemporary, standard methodologies of historiography written by Langlois & Seignobos, Bernheim and Droysen (Reis and Kristeller 1943b, 235).

On their view, a logic of the historical sciences should focus on these procedures that enable the transformation of textual data into evidence for factual, historical claims. It should not focus on an abstract ideal of the search for general laws that was the result of Hempel's rational reconstruction which equated the historical order of data with the formal notion of a hypothetical generalization of the form $\forall x(Cx \supset Ex)$.

Hempel's discussion of empathetic understanding and the use of the imagination by the historian is also countered in the article. During the final, reconstructive stage of historical inquiry, the historian uses her imagination to fill in the gaps. This use is, however, part of the epistemic process and grounded in the empirical inquiry itself.

The historian can not be satisfied with summing up the fragmentary evidence he has. He is necessarily driven to round it out as well as he can, since his purpose is full knowledge. His most important means for this task are analogies, both from his knowledge of other historical facts, and from his immediate experience. (Reis and Kristeller 1943b, 237)

So the use of imagination should be understood as an analogical inference based on other empirical evidence. It is part of the process of ascertaining the facts, and will give feedback to what kind of material the historian will look into. These analogies enable the historian to ascertain facts, but also drive his further research on the source material. There can be no separation of heuristics from empirical verification. "Collecting source materials and data requires understanding and criticism, and that a kind of interpretation is actually inherent in the very process of fact-finding" (Reis and Kristeller 1943b, 236). Thus, at no step of the inquiry does the historian "indulge himself in wild phantasies" (Reis and Kristeller 1943b, 237). This assessment of the use of the imagination in history is directly opposed to Hempel's characterization of empathetic understanding as mere heuristic preparation for actual empirical verification.

In section III Reis and Kristeller intend to refute what they call the "positivist" idea of history as a science aimed at laws, and the "romanticist" idea of history as poetry. On their view the positivists have correctly emphasized the requirement to use facts in a historical narrative, but they have neglected that interpretation of the sources is necessary for acquiring facts.

[I]n pretending to describe the facts and only facts, [the positivists] have ostensibly rejected the responsibility for understanding these facts, but since they actually can not do without some kind of understanding they have often exchanged a frank and good interpretation for an implicit or bad one. (Reis and Kristeller 1943b, 240)

They explicitly understand this as a lesson drawn from Kantian epistemology: "facts without order, discrimination, or interpretation are nonsensical" (Reis and Kristeller 1943b, 240). Such order, however, is not similar to a nomological order in the natural sciences. History cannot aim for general laws, or general causes of an event, at best it can only concern itself with the causes of particular events. There are several reasons for this. First,

the historical sciences lack the possibility of controlled replication, with which to shield off certain factors. The historical events simply do not recur enough in similar ways to set up natural experiments (Reis and Kristeller 1943b, 241). Similarly to Neurath, Kristeller and Reis are highly skeptical of the possibility to assemble enough evidence for regularities that hold over a longer period of time. Second, a historical event can be characterized in many different ways – there can be no uniformity imposed on the description of a single event. At best, the historian can relate specific events to each other based on the significance of the one for the other. Aggregating events, enabling one to acquire information about them uniformly, is simply out of the question (Reis and Kristeller 1943b, 241–42). The investigation of causes for the historian should, consequently, be considered as the inquiry into significant relations between particular events. And this is the proper kind of order for historiography, not a nomothetic order. Through these arguments Kristeller and Reis defend Rickert's anti-nomothetic, idiographic view on history: as a science it is not concerned with finding regularities between events or objects.

Contrary to Hempel, Kristeller and Reis believe that anyone who tries to claim something beyond the particular events of the past is in the process of forming a metaphysics, a philosophy of history. The range of possibilities for such a philosophy of history can be narrowed down by the ongoing empirical investigation, but this range can never be narrowed down entirely through empirical research (Reis and Kristeller 1943b, 244). Kristeller and Reis conceive of two philosophical approaches to history. On the one hand there is the logical investigation of historical method, analyzing its specific modes of inference and verification. Such logical analysis is not a rational reconstruction of historical texts through formalization, as in Hempel's paper – it is transcendental, it inquires into the conditions of possibility for the transformation of data into historical knowledge. On the other hand there is a philosophy of history, which aims to clarify how history holds within the great scheme of knowledge and reality as a whole (Reis and Kristeller 1943b, 245). The search for general laws in history belongs to the latter. By introducing this duality they have relegated the position of the young and upcoming exponent of logical empiricism to the most speculative, non-empirical aspect of the philosophical inquiry into history.

3.5 Zilsel's Laws

The third German speaking migrant that arrived in New York at the beginning of 1939 also became an active participant in the debate on historiography. During his first two years in New York Edgar Zilsel was extremely busy executing the intellectual project that he had already pitched to Reichenbach in 1930 (see 2.4.1): work out a history of science by using methods of the natural sciences, and prove through historical research that the historical

sciences are epistemologically similar to the natural sciences in a non-dilettantish way. Zilsel worked on several papers about the origins of natural science within the social hierarchy of several 15th and 16th century European countries. In the very first issue of the *JoHI* Zilsel published on the lack of a quantitative mechanics in Copernicus (Zilsel 1940), and a year later he wrote on the scientific method of William Gilbert (Zilsel 1941b). In 1942 his most well-known paper, “The Sociological Roots of Science”, got published in the *American Journal of Sociology*. It argued that the rise of modern science should not only be discussed in terms of the succession of discoveries, but also in sociological terms (Zilsel 1942b). Zilsel also wrote on the epistemology of historiography for *Philosophy of Science*. There he defended the need to search for general empirical laws in historiography, against Windelband, Rickert and a method of “understanding” for the humanities (Zilsel 1941a, 576–77). In contrast to Hempel, Zilsel openly named the philosophers whose positions he was attacking. Just as Hook and Wiener had emphasized in their contributions published in 1939, Zilsel realized that finding regular associations within human culture would be difficult. However, the obstacles to finding such associations are not theoretical like the German Neokantians claimed, the obstacles are practical: “Many scientists must establish a common program of research and cooperate according to it. By collecting and comparing the material with philological accuracy historical laws will be discovered at last not by general methodological discussions like ours” (Zilsel 1941a, 579). Hempel cites this paper by Zilsel in support of his own position in the 1942 paper, because Zilsel emphatically argues in the paper that historical laws of a statistical kind can be found (Hempel 1942, 42). Zilsel himself also revisited the issue in his contribution to the Sixth International Congress for the Unity of Science that was held at Chicago University in September 1941: “Analogous processes in as many different cultures as possible must be compared. And the single facts in each culture must be collected and worked up by statistical methods” (Zilsel 2000a, 223). To complement this systematic position, Zilsel investigated the historical origins of the concept of physical laws as well (Zilsel 1942a). His historical and epistemological research were closely entwined.

As Zilsel had maintained against Reichenbach in 1930, it was crucial to apply the natural scientific epistemology to historical practice itself. Unlike Hempel, Zilsel wanted to show his colleagues in history that statistical methods could in fact find regularities concerning the relation between social classes and the origins of modern science. In 1942 he sent a paper on the methods of humanism to the *JoHI*: it was preliminary work for a statistical analysis of the relation between the administrative class and humanistic scholarship in the 15th and 16th century across several European regions. Unsurprisingly, Zilsel's referee turned out to be none other than Paul Oskar Kristeller. The referee report was a detailed analysis of the paper, scholarly correcting many statements, and giving advice for the general argument. Kristeller wrote back to the editor, Randall: “I respect Mr. Zilsel as a person and as a scholar, although I do not always agree with his opinions. It is hence with some embarrassment that I must tell you that I was not favorably impressed by this last paper of

his.”²⁸ In his own contribution to the debate on history, Randall had already been highly critical about the possibility to formulate regularities in history. Due to the negative referee report, Zilsel’s paper would never get published.

Two elements of Kristeller’s referee report are particularly interesting. When Zilsel in his manuscript lamented that contemporary philology and historiography primarily focus on single facts rather than on general laws – thus being “further removed from the spirit of modern science”, Kristeller, a trained philologist and student of Rickert, reacts: “I disagree with your remarks about the methods and task of modern historiography and philology” (Kristeller 2003, 67). Also in reaction to the preliminary tables that Zilsel had prepared for statistical research, Kristeller rebukes: “Statistics can prove something only when they are based on consistent criteria of selection. The list is actually incomplete and arbitrary. What are the sources?” (Kristeller 2003, 70). Kristeller’s report is dated 26 December 1942, four months before the publication of his own rebuttal of Hempel’s paper on the use of general laws in historiography. Just as Zilsel’s paper was a manifestation of Zilsel’s epistemology, Kristeller’s referee report testified of Kristeller’s theoretical problems with the search for statistical tendencies in intellectual historiography. Consequently, Kristeller constantly emphasized in his report the particularity of thinkers within their contexts and the various reasons why they could not easily be compared to each other: an Italian courtier at the end of the 15th century does not have the same social role as an English courtier at the end of the 16th century – putting them in the same statistical analysis would not work.

3.6 A Proliferation of Confusion: Morton White’s Response

Hempel, Zilsel and Kristeller operated within the same intellectual network around Columbia University, regularly published in the same journals and continued in their writing the German debate on historiography within which they had grown up intellectually. Later in life Hempel may not have remembered how he came to write on history in the first place. However, between his arrival in 1939 and the publication of his article in 1942 the New York philosophy scene abounded with discussions on historiography. Zilsel, Randall, Wiener, Hook, Lovejoy, Kristeller, all these philosophers were involved in debates about historiography, and about its relation to philosophy and the natural sciences. Zilsel and Hempel argued for the same position: historians should aim to form generalizations. However, their argument for this position was very different. Hempel performed a rational reconstruction of the function of general laws, and showed how such reconstruction could

²⁸ Kristeller to Randall, 26 december 1942, 105-1 POKP.

eliminate various philosophical distinctions between history and the natural sciences. Zilsel, on the contrary, started from his own historical work, showed that the scientific method is intimately connected to the notion of law, and tried to execute his historical work by finding regularities himself. Kristeller agreed neither with their position nor with their argument for it. Instead he rehearsed Rickert's position on the use of general concepts in history by emphasizing the actual process in which historians transform their sources into evidence for their claims. Of all three positions Hempel's would take the center stage of discussion in philosophy of history.

Hempel's publication was the first manifestation of a philosophical method that would come to play an important role in professional American philosophy: the philosopher should reconstruct a scientific language within the formal framework of modern logic. None of the previous contributions in philosophy of history in the US had manifested this methodology. Hempel's example was immediately picked up by Morton White, then freshly graduated in philosophy at Columbia University. In 1943 White published a paper in *Mind*, "Historical Explanation". In his opening paragraph White takes over Hempel's logical reconstruction of statements containing the word explanation, and then asks whether there is a way to separate historical explanations in this sense from other scientific explanations (White 1943b, 213). According to White, one could only defend such distinction if one could find explanations that only contain historical terms. However, White believed that "whatever these terms are, they will not be different from sociological terms" (White 1943b, 229). He concludes that there is little to make sense of a distinct concept of historical explanation. In 1943 White, as an early adept of Hempel's Deductive-nomological scheme, also responded to Kristeller's and Reis's reaction against Hempel.

White read their paper as an argument to distinguish between history and the natural sciences based on the logical operation of generalization: whereas natural scientists aim for these generalizations all the time, historians cannot (White 1943a, 319). White believed that such a distinction could not hold: just as Hempel, White pointed out that historians use generalizations in their writing all the time and these can be reconstructed as instances of $\forall x(Cx \supset Ex)$. Kristeller and Reis, however, never denied to historians the use of generalizing sentences. When they talked about the logic of historiography, they understood this along Neokantian lines as a transcendental logic. It was about the relation between sources and inferred facts, and how this relation required the introduction of an order: the historian can only infer facts from sources after an order has been imposed on the sources, but this order could not be interpreted as a formal generalization.

White's answer was informed by an alternative understanding of the role of logic in philosophy: since historians use generalizing sentences, the philosophical analysis of their knowledge should also contain universally quantified statements. Hempel's logical reconstruction of history hinged on a similar point: one can reconstruct some writing in history as universally quantified sentences. The philosopher is required to account for these sentences in his logical analysis: doing epistemology is uncovering the formal aspects of

scientific language. Kristeller and Reis did not share this notion of formal analysis through rational reconstruction. For them a logic is not a reconstruction in a formal scheme, it is an inquiry into the conditions of possibility of knowledge, an analysis of the transformation of the given into knowledge of the world (what must be imposed on data for them to say something about the world). Consequently, in their own reply to White they don't see how White had added anything to the debate that was already there between them and Hempel: "we were encouraged by the fact that he [White] actually defends the position attacked by us and thus confirms our conviction of the need to state our thesis" (Reis and Kristeller 1943a). Whereas Hempel's paper still had implicitly engaged with the older Neokantian approaches to historiography, White's reply to Kristeller and Reis failed to address their worries in any way. Kristeller's and Reis' approach fell mute with the first generation of logically trained philosophers. It is revealing to notice that Kristeller's reply of 1943 would be completely neglected in later debates on Hempel's 1942 paper.

3.7 Opposite Advice to Hempel: Neurath and Stevenson

In December 1942 Hempel sent his paper to Neurath who was currently living in Oxford. Hempel did not talk about his publication to most of his other correspondents: unlike his work on probability or confirmation, Hempel left his paper on history unmentioned in his correspondence with Reichenbach and Carnap. Of course, given Neurath's earlier pleas to discuss historiography, Hempel alerted him of the publication. Even though Hempel argued for the methodological unity of science against the philosophers who Neurath in their correspondence had called *Windelbandits* [Windelbanditen], Neurath was not pleased with the result. Initially, he only stressed that Hempel had neglected "unpredictabilities in empiricism".

But the unpredictability of chance is already there before, in so far as items are concerned, which are not sufficiently repeated, and therefore do not allow the application of probability calculus etc. What do you think about this unpredictability business. I should appreciate it very much if you found time to answer my remarks.²⁹

Hempel, however, did not reply immediately. And after the death of his wife in January 1944, there is a large gap in the Neurath-Hempel correspondence. In a letter of 25 November 1944 Neurath revisited his problems with Hempel's paper. Neurath said that he "cannot

²⁹ Neurath to Hempel, 21 September 1943, Nr. 246 VCA.

follow from the start". "That you call M. Mandelbaum's analysis clarifying hurt me deeply."³⁰ Maurice Mandelbaum's 1936 book *The Problem of historical Knowledge* was an overview and refutation of what Mandelbaum called German historicists, e.g. Heinrich Rickert (Mandelbaum [1936] 1967). Neurath had wanted to "quote this book as an example of overwhelming metaphysical confusion." Neurath considered Mandelbaum's book as metaphysics, since it made the methodological mistake to start an analysis of science from authors *writing about* historiography instead of starting an analysis from authors *working in* historiography. Neurath complained to Hempel that he could no longer criticize the book after Hempel's positive remarks, because it would highlight an open dispute in the Unity of Science movement. Neurath's main problem, however, was Hempel's logical reconstruction of laws which are supposed to cover social and historical events. To him this reconstruction of laws seemed inapplicable, since the events that they are supposed to cover, do not occur often enough.

You see I think the so-called probability standpoint in history would imply, that we get 6000 Hitlers and then tell something how they behave "on an average". Usually people think that the behaviour of Hitler and the Nazis may be based on probability statements dealing with masses, as if that were in discussion here. IT IS NOT.³¹

Hempel's analysis of the function of laws only works if one assumes a priori the possibility to order historical events into kinds. Such assumption works in repeatable, experimental contexts. One cannot assume that it will work in historical research. Such assumption is not the result of an empiricist point of view. Consequently, Neurath sees fault in Hempel's ideas: Hempel's analysis started from abstract logical ideas about the use of laws, and it did not pay attention to actual historical practice. He complained about Hempel's assumption that grounded his logical reconstruction: "Your 'models' are shamefully concealed substitutes for the ONE world, known by god or Laplace's spirit."³² An investigation in the research practice of actual historiography would prove that the formation of laws in the realm of human events is not very meaningful. For Neurath, this applied especially to the predictive capacity of sociology and historiography. In his paper Hempel had defended that in any field of science, including historiography, predictions could be logically inferred from the hypothetical generalizations on the theoretical level. Neurath thought this was not possible for the historical sciences. He repeated one of his arguments about social predictions from *Empirische Soziologie*: "You speak of guess 'influenced' by something, but sometimes one

³⁰ Neurath to Hempel, 25 November 1944, Nr. 246 VCA.

³¹ Neurath to Hempel, 25 November 1944, Nr. 246 VCA; In his *Empirische Soziologie* Neurath had already argued that regularity-statements about social events expressed as hypothetical generalizations are not evidently applicable in the social and historical sciences. For a similar position, also see (Von Mises 1968, 162).

³² Neurath to Hempel, 25 November 1944, Nr. 246 VCA.

may speak of a guess ‘influencing’ the events, e.g. predictions made by Lenin formed a part of the events themselves...”³³ Predictions by sociologists, and reasoning about human affairs in general, often influence human behavior in ways that could not be predicted in advance. Predictions can influence the conditions on which regularities of human behavior rely. For Neurath, scientific inquiry was a part of the developments in the social world itself. This results in unpredictability in principle (Neurath 1931, 130–31). Imposing on sociology or historiography that they could produce predictions through laws in the same way as certain parts of physics is the philosophical imposition of a God-like view onto scientific inquiry from without.

Between 1943-1945 Neurath became frustrated by the fact that many people within the Unity of Science movement did not seem to listen to his arguments, especially his argument for unpredictability in principle in the social sciences (Reisch 2001; Uebel 2007b, 273–74). Neurath lamented to Carnap that "Hempel READING my papers LISTENING to my arguments did not REMEMBER afterwards these points, BECAUSE THEY ARE SO FOREIGN TO HIM."³⁴ Zilsel’s work was similarly faulty according to Neurath: “You see, I am impressed by the behaviour of Hempel in social problems, Zilsel behaves similar.”³⁵ In a letter to Charles Morris it is clear what Neurath disliked about Hempel’s methodology in particular: "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."³⁶ In Neurath’s eyes, Hempel started his analyses of science too much from a purely analytic, logical point of view, disregarding how a scientific field of study had evolved its more specific techniques of research. This methodological issue had already been present in the Hempel-Neurath correspondence from 1935 onwards.

Another issue that Neurath regularly brought up in his correspondence during those years was the use of the term ‘explanation’. In 1943 Neurath had advised Herbert Feigl to drop the term ‘explanation’ from the title of his manuscript for the *Encyclopedia of Unified Science*. When Carnap asked Neurath why he had made this suggestion to Feigl, Neurath replied:

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 ‘explanation’ as something besides finding correlations. You know the discussion about "Erklaerung" and Krichhoffs statement on ‘description’.³⁷ Of course it would be pedantic always to

³³ Neurath to Hempel, 25 November 1944, Nr. 246 VCA.

³⁴ Neurath to Carnap, 16 June 1945, RC 102-55-11 ASP.

³⁵ Neurath to Carnap, 25 September 1943, RC 102-55-03 ASP.

³⁶ Neurath to Morris, 6 April 1945, Nr. 274 VCA.

³⁷ Gustav Robert Kirchhoff (1824-1887) was a German physicist. He took up the position that the goals of mechanics are purely descriptive: "The goal of mechanics is to describe movements in nature as fully and as a

avoid the term 'explanation' but I personally should dislike to use the term as the heading of scientific analysis as such. Of course 'description' may involve, what Stebbing calls 'constructive description' or whatever, but it has been just a success, as we think, that Kirchhoff lead us away from the 'Erklaerung' ('explanation') to the correlation point of view. I should suggest that Frank discusses that with you, Morris and Feigl. I cannot think, that Feigl's point of view is chained to the dangerous term 'explanation' which just indicated through decades, what we are fighting. I think so. Occasionally using the term 'explanation' is not of importance. I think it is SERIOUS.³⁸

Neurath writes similarly to Feigl and Morris.

I have strong objections to scientific explanation...you see, as you know, we always tried to tell, that EXPLANATION, ERKLAERUNG in the traditional way is not our technique, we present correlations, and some people call some correlations 'explanation'. But not we do it...³⁹

I personally ask you a favour, please avoid the term EXPLANATION in title and text, if possible. Let me explain, why. Of course, I can imagine many definitions which render this expression harmless. But, you see, expressions have their history and their propaganda importance.⁴⁰

We fought for the DESCRIPTION attitude (Kirchhoff - Mach) against the cause-effect "explanation", therefore it seems to be important, to maintain this point, as strong as possible.⁴¹

Neurath believed that explanation as a term could in principle be harmless if it is defined in terms of correlations between events. However, given its history, the term should be avoided in an analysis of science: science describes the world and systematizes these descriptions.⁴² The introduction of explanation into the terminology of philosophers of

simple as possible" (Kirchhoff 1876, 1). Mach referred to Kirchhoff as a colleague who supported his descriptive views on natural science (Mach [1893] 1974, 325). For Kirchhoff's relation to late 19th century philosophy, see (Majer 1985). The association between Mach and Kirchhoff was common in logical empiricist writing. Richard von Mises also referred to Kirchhoff in his exposition of the anti-explanatory view on science: (Von Mises 1968, 137).

³⁸ Neurath to Carnap 1 April 1944, RC 101-55-05 ASP.

³⁹ Neurath to Feigl, 29 february 1943, Nr. 233 VCA.

⁴⁰ Neurath to Feigl, 30 April 1944, Nr. 233 VCA.

⁴¹ Neurath to Morris, 6 March 1944, Nr. 274 VCA.

⁴² George Reisch has already discussed Neurath's reaction to Feigl's use of the term 'explanation' in the context of Neurath's project of Terminology, see (Reisch 1996). Reisch shows how Neurath believed terminological consensus after open discussions was a goal for the Unity of Science Movement. Concerning explanation, it is however important to notice that Neurath's rejection of the term is not merely "descriptive and minimal" as Reisch

science might resurrect all kinds of metaphysical confusions about the aim of science. Feigl's monography would never get written. Consequently, the term would never be used in a title of the Encyclopedia. Surprisingly, however, Neurath did not comment on Hempel's explicit use of the term 'explanation' in the 1942 paper, even though he writes his comments to Hempel several months after his letters to Carnap, Feigl and Morris on his very explicit worries about the introduction of the term 'explanation'. In 1935 Neurath had already pressed Hempel on the fact that he should be careful about the kinds of terms that one introduces in analyses of science. Neurath had warned Hempel: "Even strong persons succumb to Word idols."⁴³ According to Neurath, the use of the term 'explanation' besides 'description' was precisely such a word idol – it should not be introduced into philosophy of science. Just as Mach, Duhem and Frank, Neurath considered it a great success in the history of science that scientists had grown a consciousness that finding out ultimate causes or essences was not the goal of science. These older goals of science were associated with the term 'explanation' and, consequently, this term had a specific history and should be avoided. Neurath could have overlooked the use of the term 'explanation' in Hempel's 1942 paper, but that is hard to believe given its extensive use. It is more likely that Neurath had already given up on this methodological struggle with Hempel about the introduction of older terminology with a specific history. At least, Hempel had still defined explanation in accordance with the tradition that Neurath constantly brought up in his correspondence, namely as a systematization of experience. Besides Neurath's advice, Hempel also received radically different comments on his paper from the American context, which would eventually lead Hempel to rethink his 1942 paper in a substantial way.

Immediately after the publication of the 1942 paper, Hempel received a very positive letter exhibiting an entirely different philosophical point of view than Neurath's. Charles Stevenson wrote to him: "I'm wholly convinced by the parallel between historical and other empirical explanations."⁴⁴ After lengthy discussions with Nelson Goodman, Stevenson nonetheless pressed Hempel on his crucial definition of laws in the paper. On Stevenson's understanding a law could not be defined as a generalization over kinds of events that had some level of empirical confirmation. "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'". According to Stevenson, laws are true or false, independent of the confirmation that one has of them. Hempel's definition of a law could not cover this intuition on laws. In his letter to Hempel, Stevenson argues in the following way:

claims (Reisch 1996, 83). Neurath had a clear idea of what modern science is in opposition to the older explanatory views on science, which he himself saw as an important historical insight from Mach.

⁴³ Neurath to Hempel, 25 March 1935, Nr. 244 VCA.

⁴⁴ Stevenson to Hempel, 3 February 1942, CH 38-3 ASP.

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."⁴⁵

Stevenson's philosophical response here 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 mentions scientific works or scientific methodology. Stevenson's argument counters the core of Hempel's starting point: laws are not systematizations of experience, they are true independent of the experience that one has. In the 1942 paper hypothetical generalizations were supposed to subsume elements of experience under them: they show how certain events in experience are related to others. Consequently, it would make perfect sense to say that certain laws can cease to operate as laws relative to a new set of experiences, since these laws could no longer cover the new set. This was also Frank's understanding of the dialectic of physical theory: laws only function in tandem with the available experiences at a specific point in time (Frank 1953, 479). The idea that laws are representations about the world independent of the experiences that one has, is according to the anti-explanatory tradition a metaphysical idea that cannot be supported by scientific inquiry. If one accepts Stevenson's reasoning, laws should perform more than a systematization; they should also be true independent of the experience that one has. Then, Neurath's "correlation point of view" cannot be maintained, and explanation can no longer be understood as the backwards systematization of experience, since the laws that operate in it should be true independent of experience.

Just as Neurath's complaints about the formal starting point of Hempel's paper, Stevenson too believes that Hempel's formalization in itself is not adequate. Stevenson, however, introduces a novel form of philosophical reasoning to Hempel: one can discuss the merits of a formalization by using intuitions about the use of terms. Naturally, this form of reasoning stands in great opposition to Neurath's warnings, which were exactly focused on the historical assumptions that are implicitly linked to terms and the possible confusions that might follow from formalizing such assumptions. In 1942 it was still undecided which direction Hempel would take. How Hempel implements Neurath's or Stevenson's argument in the further development of his ideas, will be picked up in the next chapter.

⁴⁵ Ibid.

3.8 New York's Parting of the Ways

Kristeller's criticism of Hempel's paper was entirely forgotten after its publication. As so many other papers on history in the *JoP* of those years, it left no mark on further debates. However, when Hempel's paper became the central focus of discussion in the 1950s and 1960s, Kristeller himself did not forget his initial contribution. In 1961 Kristeller repeated his analysis of the epistemological problems in historiography once more in the *JoP*, and he was not very keen on the recent developments in theory of history:

Another question that has been much debated by recent theorists of history is the role of causes and of general rules. I have stated before that I do not believe that it is the task of the historian to establish general laws or rules of causality. Those who assign this task to the historian tend to substitute for history a metaphysical philosophy of history, or the science of sociology. [...] The historian wants to establish particular events or developments, not general laws or rules. (Kristeller 1961, 94)

In 1985 Kristeller also revisited his standpoint on the uselessness of discussions about historical explanation during a symposium at the American Philosophy Association. At that time, Kristeller was already an acclaimed historian of renaissance philosophy, but not well-known by systematic philosophers. Kristeller's judgment was harsh: "Philosophers who claim to explore the status of historical knowledge have written about general laws of history and about causal explanation. These topics may concern the philosopher of history and also the sociologist or anthropologist, but they are speculative and derivative, and at best marginal for the practicing historian or philologist" (Kristeller 1985, 619; Megill 1989). Despite Kristeller's judgement, explanation was by 1985 a well-established, central topic of interest for philosophers of science and it had also dominated debates in American philosophy of history. When Kristeller first responded to Hempel in 1943, this was not the case: his reaction was aimed against Hempel's insistence that historians should deal with their subject matter in terms of general concepts. Kristeller was not concerned with models, theories or accounts of historical explanation. In the New York context the German debate over historiography was taken over as a debate over the applicability of general concepts in history. Kristeller had defended Rickert's position in that debate.

In 1943 Sidney Hook published a book that also discussed this problem, *The Hero in History: A Study in Limitation and Possibility*. Hook emphasized the power of individuals to influence the course of events, against Marxist tendencies to place all human agency under the control of sociological and economical laws. Reichenbach, seemingly unaware of Hempel's 1942 paper, wrote a lengthy, congratulatory letter to Hook on his new book.

I found it an excellent analysis of the subject. I think like you that the role of the influential personality has been greatly underestimated by those who have tried to construe history as a product of sociological laws.⁴⁶

Reichenbach did not deny that there were laws governing historical events, but he showed scepticism about any assumption that such laws were stable.

The laws lie on a much deeper level and reveal themselves perhaps in the course of centuries, or milleniums; but the times as we see and live them are mostly shaped by fluctuations and could have been very different if some petty events had not occurred. It may even sometimes happen that the deeper current of history is completely changed as an effect of fluctuations.

Reichenbach's reaction to Hook shows how the validity of lawful generalization in history was the central issue in the New York debate. A focus on the analysis of explanation did not immediately follow from Hempel's 1942 paper. Hempel had merely argued that historians could aim for lawful generalization, which was obviously contested, even within logical-empiricist circles. That explanation was not the issue is also reflected in a 1946 report which Charles Beard and Sidney Hook wrote for the *Social Science Research Council*. Beard stated that the modern historian should add precision to the use of his theoretical terms, and a theoretical glossary is a good tool to ameliorate this precision (Beard and Hook 1946, 105). After Beard's short justification, Hook gave such a glossary. Unlike what a modern reader would expect, explanation was not one of the important theoretical terms in the glossary. Under the lemma of "understanding" one can, however, find an overview of the debate in which Hempel's and Kristeller's papers should be situated. There, Hook described two generic schools of thought.

One school asserts that "historical understanding is a species of scientific understanding in general, and that scientific understanding is theoretically equivalent to 'explanation' "(Beard and Hook 1946, 127). The criteria for an adequate explanation are formally the same for all events and processes: one has to show how the event relates to other events through general laws or statistical generalizations. Consequently, according to this school of thought, "historical understanding differs from other forms of scientific understanding only in respects which reflect differences in the subject-matter to be understood" (Beard and Hook 1946, 127). In his description of this school of thought, Hook in effect summarizes Hempel's 1942 paper.

⁴⁶ Reichenbach to Hook, 7 May 1943, HR 37-25c-73 ASP. The letter is also interesting for another reason: Reichenbach argues that the course of the history of science does not heavily depend on individual ingenuity. He claims that scientists perform group work that negates the importance of an individual. Reichenbach believes that most important scientific contribution by individuals, like Einstein's, could also have been brought about by group work.

The other school is portrayed as the opposite of the previous one: they claim that historical understanding is radically different from scientific understanding. According to this school, understanding is some kind of 'imaginative identification' or 'valuation' which makes the historical occurrence plausible or intelligible. According to Hook, this makes historical understanding relative to the credibility schemes of individual historians, which necessarily relativizes historical understanding to cultures and time frames. This school represents a Rickert-like approach that connects the selection of historical events to a value relation that the historian has to impose. Thus, Hook in 1946 puts Hempel's position in a debate over the use of general laws in the historical sciences. The opposition between the two schools is still understood from within the original German debate on historiography, and not from the perspective of explanation as an aim of science.

By the end of the war, the debate on historiography that tightly knit the three immigrants Kristeller, Zilsel and Hempel together, began to crumble. Zilsel left New York in 1943 for a teaching position at Mills college in Oakland, California, where he committed suicide on March 11, 1944. This meant that one of the most vehement advocates of an integration of history in philosophy of science and of science in philosophy of history disappeared from the logical empiricist network. This occurred before Zilsel was able to connect all the elements of his work into a unified whole as he had promised Reichenbach in 1930. At the APA meeting of 1947, a memorial notice was read in honor of Zilsel. It was not written by a colleague from the philosophy of science, but by Paul Oskar Kristeller, who emphasized Zilsel's work as a historian. Though Kristeller noted Zilsel's interest in philosophy of science, he focused on Zilsel's philological and historical training under Heinrich Gomperz and Zilsel's early, historical work on the concept genius. Zilsel's historical work in the United States received the most attention. Perhaps as a tribute to the quality of Zilsel's latest work, Kristeller continued their disagreement about the renaissance period.

After his arrival in this country, he published a number of valuable articles on the history of early modern science and philosophy. He succeeded in pointing out the contributions made to early science by the craftsmen and artisans of the Renaissance period, although other factors may have been more important in the same process than he was willing to admit. These studies should have culminated in another significant book whose completion was prevented by his tragic and untimely death. Dr. Zilsel will be remembered with respect as a scholar and as a person by his old and recent friends. ("Proceedings of the American Philosophical Association 1947-1948" 1948, 375)

Neurath, who had also advocated for the integration of history and philosophy of science and who had struggled against the term 'explanation' during the war, died shortly after the war, having been largely ineffective in his influence on Hempel. As a consequence, few actors remained in the network of logical empiricism who aimed for an integration of history and philosophy. At the same time, Kristeller's 1943 paper was his last attempt to contribute to philosophy of science. The bleak interaction with Morton White had not strengthened his

resolve to further advocate for his position. When he arrived in the United States in 1939, he considered himself a philosopher who used history to perform philosophy. At Columbia, Kristeller became a historian of philosophy who had little or no impact on philosophy proper. In an unpublished version of his autobiography Kristeller explicitly links his migration to his own change of intellectual focus, from historical philosophy in Germany to history of philosophy in the United States:

In any case, in the German philosophical context, historical knowledge was as important and scientific as that of the natural sciences, and the attempts to adapt an epistemology that was basically Kantian to historical knowledge were appealing to me. [...]

My double interest in philosophy and history led me into the history of philosophy, and from this into intellectual and cultural history in a broader sense. If I had remained in Germany, I might have pursued a philosophical synthesis that combined elements of Neokantianism (of the realist kind), Neoplatonism and existentialism. The Neokantianism takes care of the empirical knowledge of history and its details (without being positivistic in the sense of Comte or others), the Neoplatonism of the higher intellectual dimensions including art, the existentialism of the highest inner experience that may be considered religious or mystical although it is not opposed to reason or identical with any positive personal religion, Jewish or Christian.

My emigration, first to Italy and then to America, discouraged any further work along these lines since I encountered a philosophical climate, especially in America, that was quite uncongenial to my philosophical outlook. On the other hand, my acquired skill in history and philology, and later in bibliography and manuscript research, gave me a special expertise that turned out to be the basis of my academic career.⁴⁷

In 1943 Kristeller was still working towards philosophical synthesis, defending the empirical knowledge of history through Rickert's Neokantianism. Some years later, his career path was set on history of philosophy proper; he would not return to systematic debates. In a much later phase of his career, Kristeller addresses his colleague Philip Wiener about the uncongenial climate of "analytic" philosophy. In the correspondence of 1958 with Wiener over the publication of a book manuscript of Richard Popkin, Kristeller countered the comments of a negative referee report that Wiener had received about the manuscript:

I found the general comments of the reader quite unwarranted. They merely reveal that he belongs to the "analytical" school of philosophy which is in most instances unsympathetic towards the history of philosophy, and whose judgment I am inclined to discount in such matters. [...]

⁴⁷ Autobiographical Manuscript, undated, 77-20 POKP.

With reference to the practical problem that the analysts constitute a large group among philosophers in this country, and hence the reader's opinion might be typical, I should say: by this token, no book in the history of philosophy could be published. Moreover, there are fortunately enough members of the philosophical profession who retain an interest and competence in historical studies.⁴⁸

In comparison with the situation of Reichenbach's petition in 1931, the philosophical climate in the post-war United States was configured in the opposite direction. Now, historical studies had a hard time getting published, because they were not considered philosophical enough. Perhaps unthinkable in the German interbellum context, Kristeller had to defend the value of history of philosophy. To that end, Kristeller was involved in a commission of the APA in 1958 that wanted to erect a new journal aimed at the history of philosophy.⁴⁹ According to the commission, contributions in history of philosophy were increasingly getting rejected in American philosophy journal in the 1950s, which resulted in a marginalization of history of philosophy in the journal publications.⁵⁰ When Kristeller consulted Randall as editor of *JoP* about this situation, Randall reported in a letter to the official commission that the editors of *JoP* "think there is a great need for a journal of the history of philosophy. [...] The fact that it is very difficult to place historical studies naturally reduces the number that are written. With an adequate outlet, we should have much more material prepared." Similar to Reichenbach's and Carnap's ambition with *Erkenntnis*, the origins of the *Journal of the History of Philosophy* lay in an initiative to proliferate philosophical attention against a perceived mainstream.⁵¹ Such drives create subdisciplines, like philosophy of science or history of philosophy, with their own journals, training programs and eventually their own job market.

During the Second World War the distinction between philosophy of science, history of science and history of philosophy was still blurred. Representatives of these three perspectives, like Hempel, Zilsel and Kristeller, could still discuss the same issue: the difference in their perspective did not yet refract their positions into different disciplines. After the war, the ways parted for good. Hempel's rational reconstruction began a life of its own in the decontextualized discourse of philosophy of science, while Zilsel's and Kristeller's particular historicisms were safely kept outside philosophical reasoning in the domains of the history of science and the history of philosophy.

⁴⁸ Kristeller to Wiener, 13 April 1957, 61-3 POKP.

⁴⁹ Vlastos to Randall, 13 March 1958, box 35, folder American Philosophical Association, JHRP.

⁵⁰ Randall to Vlastos, 20 December 1958, box 35, folder American Philosophical Association, JHRP.

⁵¹ The distinction between philosophers that want to use formal methods of logical reconstruction for philosophical analysis and philosophers who opposed these methods, already manifested itself in the New York philosophy scene in the 1940s, especially in the contentious relation between Randall and Nagel (Jewett 2011, 117–18).

Chapter 4

How to Talk about Explanation?

After his work on historiography, Hempel was challenged to rethink his philosophical practice from two opposite perspectives. On the one hand, Charles Stevenson invited Hempel to reconsider his definition of a general law so as to bring it closer to intuitions of what we generally take to be a law. On the other hand, Otto Neurath had scorned Hempel's formal assumptions concerning the applicability of lawful statements to historiographical practices. Hempel did not heed to Neurath's advice and he continued to approach scientific inquiry from a formal starting point. In his 1945 article "Studies in the Logic of Confirmation" that was published in *Mind*, Hempel set out to give a formal model of confirmation as a relation between a hypothesis and a body of evidence. This formal characterization of confirmation was an attempt to show how the confirmation of scientific hypotheses is not relative to a subjective "sense of evidence", a feeling of plausibility in view of the relevant data (Hempel 1945a, 8). Similar to the 1942 article, Hempel used his formal characterization to defend the objective nature of scientific inquiry. Hempel opened his search for objective criteria with what he introduced as an intuitive notion of confirmation. However, through the example of "All ravens are black" and its possible confirmation according to the intuitive notion, Hempel argues that our initial intuitions are not sufficient to think about confirmation. As in the 1942 paper, a rational revision has to be undertaken. According to Hempel, this rational reconstruction or revision of confirmation was not a question of fact. No descriptive account of the research behaviour of scientists would suffice to support the formal reconstruction.

Here, as in all other cases of logical analysis of science, the problem calls for a "rational reconstruction" of scientific procedure, i.e. for the construction of a consistent and comprehensive theoretical model of scientific inquiry, which is then to serve as a system of reference, or a standard, in the examination of any particular scientific research. The construction of the theoretical model has, of course, to be oriented by the characteristics of actual scientific procedure, but it is not determined by the latter in the sense in which a descriptive account of some scientific study would be. Indeed, it is generally agreed that scientists sometimes infringe the standards of

sound scientific procedure; besides, for the sake of theoretical comprehensiveness and systematization, the abstract model will have to contain certain idealized elements which cannot possibly be determined in detail by a study of how scientists actually work. (Hempel 1945b, 117–18)

As clarification of this methodology, Hempel introduced an analogy with the game of chess. Consider two people playing a game on a board (the game we know as chess): how would you infer the rules of the game from the actual behaviour of its players? The situation is the same for the philosopher of science, who has to abstract a formal account of scientific inquiry (the rules of the game) from actual scientific procedure. According to Hempel, the reconstructed model of the philosopher has to be simple, consistent and also “conformable to the behaviour of scientists to a large extent” (Hempel 1945b, 118). Consequently, the central question for Hempel is how to evaluate his formal reconstructions once an intuitive notion is shown to be deficient: which criteria can be used to arbitrate between various possible revisions? It has to have some descriptive adequacy and it should allow one to critically analyse science (“serve as a standard”). As in his 1939 letter to Reichenbach (see 3.3), Hempel remains very vague about the exact characterization of his standards of evaluation: he does not state when a formal model can be considered descriptively adequate or how it can be used critically. How can one see the “essential imperfection” of a formalization, such the imperfection of a one-place predicate “heavy” in the 1939 example that he gave to Reichenbach? In his paper on confirmation Hempel never referred to any behaviour of scientists, or contemporary scientific practice. The entire argument of the paper centres around the formal characterization of “All ravens are black” and the possible confirmations of it. This toy example leads Hempel to develop a definition of confirmation that applies to a restricted formal language (containing no predicate values or identity sign). How such definition can serve the descriptive and critical goal of logical reconstruction of science remained unspecified.

Hempel’s next major research paper would deviate from the path of rational reconstruction and incorporate the use of language intuitions into his analysis. Hempel would also tackle a subject that, unlike confirmation, had been absent from any logical empiricist’s agenda so far, namely scientific explanation. Whereas the New York debate on historiography offered a good contextual background for Hempel’s 1942 paper, it is much harder to assess the motivation for Hempel’s turn to the subject of explanation. Within the American philosophy scene, three philosophers discussed explanation in the years immediately before Hempel and Oppenheim’s 1948 contribution: Herbert Feigl, David Miller and John Hospers. Whereas Feigl’s contribution defends a mild version of the anti-explanatory position, Miller’s and Hospers’ contributions offer a new direction to the philosophical reflection on science (see 4.1), one that Hempel and Oppenheim took up, which lead to a subtle, but undeniable distinction with the anti-explanatory tradition (4.2 & 4.3).

4.1 Feigl, Miller & Hospers

In 1945 Herbert Feigl, who, against Neurath's wishes, had planned to write on explanation in theory construction for the *Encyclopedia of Unified Science*, participated in a special issue on operationism of *The Psychological Review*.¹ The participants to the symposium were asked to discuss eleven questions of interest. Although Feigl's contribution was entirely set within a traditional Machian anti-explanatory view, question 7 triggered him to talk about explanation. The question was set within an ongoing dispute between the Gestaltist Köhler and the American philosopher C.C. Pratt.

Must operationists in psychology relegate theorizing of all sorts to the limbo of metaphysics? [...] The Gestaltists, particularly Kohler and Koffka, have repeatedly attacked positivism (an identical twin of operationism), reproaching it for its (alleged) opposition to theoretical construction. C. C. Pratt (*Logic of Modern Psychology*, P. 147-154) on the basis of his operationism maintains that all theoretical explanation is circular or tautological. (Langfeld 1945, 242)

In reply to this question, Feigl countered the radically anti-theoretical interpretation of operationism. According to Feigl, "Ever since Galileo replaced the question 'Why?' by the question 'How?' and since Newton pronounced his (much misunderstood) 'Hypotheses non fingo', positivistic scientists have been inclined to restrict their endeavors to pure description and correlation. Explanation is considered a metaphysical misfit" (Feigl 1945, 254). However, according to Feigl, explanation can still have a legitimate role in scientific inquiry, if understood correctly. On Feigl's account, a theory should be understood as a set of assumptions from which empirical laws are derivable by logico-mathematical deduction (Feigl 1945, 255). The deduction of empirical laws from theory "merely explicates what is implicit in the premisses [theoretical assumptions]" (Feigl 1945, 255). Feigl does not believe that stable theoretical assumptions are the end point of inquiry. Instead, he considers them to be the compressed form of empirical concepts. They operate on a "conceptual economy": "Constructs are introduced in order to save statements" (Feigl 1945, 257). In Machian fashion, theory construction serves two goals. First, it guides empirical inquiry: "on the basis of only indirectly confirmed theoretical assumptions (not only of experimental laws) more specific descriptions of phenomena are logically deduced" (Feigl 1945, 254). These deductions can then serve to direct new empirical inquiries. Second, theory construction offers an economical symbolization of empirical correlations.

¹ In contrast to the two previous chapters, I mainly discuss published sources throughout this chapter. This is not a methodological choice, but a practical limitation resulting from the current state of my empirical inquiry. I have not yet had the opportunity to visit the appropriate archives to continue my investigations at the same level of detail as the two previous chapters.

Feigl only diverts from the Machian line when he makes the following statement about understanding: “Through the unifying procedure of theoretical explanation we ‘understand’ what on the level of empirical law is a mere brute fact of functional dependency or correlation” (Feigl 1945, 255). Feigl does not clarify this statement in his original contribution, but returns to it in the “Rejoinders and Second Thoughts” section of the special issue. In this text, Feigl wanted to clarify his compressed views on explanation in science, especially from a logical empiricist point of view:

A modern logical empiricism may retain the valuable antimetaphysical tendency in the older point of view while at the same time giving a methodologically more adequate reconstruction of the explanatory process as actually employed in the various sciences. (Feigl et al. 1945, 284).

Feigl’s more adequate reconstruction characterizes explanation as an inference that deduces descriptive statements from general assumptions in conjunction with other descriptive statements. In his reference Hempel’s 1942 paper is given as an inspiration for this characterization of explanation. From this logical characterization of explanation, Feigl concludes three things. First, explanation does not show the necessity of an event: it does not show why an event had to occur. It only shows that, given the premises, the conclusion, which is a statement about the event to be explained was logically necessary (Feigl et al. 1945, 285). Second, one should uphold a distinction between the term ‘explanation’ and the term ‘description’. ‘Description’ should be reserved to talk about singular statements representing fully specific facts, events or situations, whereas ‘explanation’ denotes the deductive inference that uses theoretical assumptions. This terminological distinction could avoid many confusions. Feigl explicitly notes that explanatory inferences should be judged on their logical and empirical validity only. The familiarity of the premises is of no importance. The inference is not supposed to familiarize an odd event by relating it to a well-known principle (Feigl et al. 1945, 285). Third, what does matter to the evaluation of an explanatory inference, is the generality of explanatory premises. The more empirical laws that can be deduced from a theoretical construct, the better (Feigl et al. 1945, 285). However, this generality is not an end in itself. When theory construction reaches an end, is a pragmatic matter according to Feigl: “there is no need for climbing higher on the tower of constructs if all the data one cares to see are within sight” (Feigl et al. 1945, 285). This rehabilitation of the notion ‘explanation’ as an inference that uses theoretical constructs, does not stand in great opposition to previous logical-empiricist or Machian viewpoints.

However, through his discussion, Feigl also rehabilitates why-questions in science: “the question ‘why’ (in the sense of a demand for explanation) is answered by deduction either from empirical laws or from theories” (Feigl et al. 1945, 286). Here, Feigl also reintroduces a notion of understanding: “It is on this theoretical level (the ‘row of genius’ as I like to call it) that we gain a ‘real insight into the nature of things’ (as metaphysicians call it)” (Feigl et al. 1945, 286). According to Feigl, the higher level theoretical assumptions introduce

existential assumptions on the micro-structure of the observable phenomena. These posits give us an “aha-experience that is much stronger for these deductions from theories than for the much simpler deductions from empirical laws” (Feigl et al. 1945, 286). Consequently, Feigl seems to imply that an explanatory inference can do more than offer the economical representations of empirical results and the guidance of future empirical inquiry. This extra element of explanation is connected to the answer to why-questions and the aha-experience that accompanies answers that operate with high theoretical assumptions which implicitly contain multiple empirical correlations. Although Feigl’s contribution, through this notion of understanding, hints at a more robust notion of explanation, Feigl does not yet identify explanation as a distinct cognitive aim of science.²

An alternative way to think about explanation as an autonomous cognitive activity was provided in the late 1940s by American scholars who had little affinity with the anti-explanatory view on science which was taken for granted in logical empiricist circles. In 1946 John Hospers, at that time working at the University of Illinois, attempted to analyse the nature of explanation in the *Journal of Philosophy*. Hospers got his PhD at Columbia University in 1944, where he had briefly studied under G.E. Moore (O’Grady 2011).³ Hospers’ paper started his discussion of explanation from an analysis of ordinary language:

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)

Consequently, why-questions and explanations are intimately linked in Hospers’ understanding.⁴ Hospers never discussed actual scientific research in his paper. Instead, he used his linguistic intuitions to evaluate several possible positions on what constitutes a proper explanation statement: a statement in terms of purposes, a reduction to something that is more familiar, a statement of some general law, a statement that connects two

² Since the very beginning of his philosophical career, Feigl had been under the spell of neokantian critical realists, like Schlick and Alois Riehl. In the 1930s and 1940s his realistic conception of theoretical terms in science lay dormant. From 1950 onwards Feigl would argue for a semantic realism, and by 1974 he would claim that science is more than an economical summary of experience and also a search for explanation (Neuber 2011, 171). However, in the 1945 paper these realists elements were not yet fully articulated by Feigl, who remained within the Mach-Duhem tradition concerning the notion of explanation.

³ The information that I currently have about Hospers’ teaching at Columbia is extremely limited. The fact that he was another philosopher present in the New York scene at Columbia and was dealing with explanation, may not be a coincidence. Was Hospers present in Nagel’s logic courses? Was he a part of the Nagel-Hook circle? Did he know Hempel? These are still open questions, worthy of pursuit.

⁴ John Hospers may be best known for his textbook *An Introduction to Philosophical Analysis* that first came out in 1953, but went through multiple reprints thanks to its popular use in introductory courses in philosophy (Hospers [1954] 1973). The book also contains a section on explanation in science that introduces the topic through the same kind of ordinary language analysis by investigating how one answers questions like “Why is the door open?” (Hospers [1954] 1973, 161)

phenomena previously unrelated, or a reference to a cause. On Hospers's account all five possibilities fail: not everything can be explained through purposes; explanation cannot be relative to the familiarity of a subject; generalities yield no satisfying answers; the formulation of connections are actually similar to the formulation of generalities; and the reduction to causes does not solve the issue, since it is not clear what causes are or what kinds of causes can serve in explanations (Hospers 1946, 338–45). In the first part of his paper Hospers reached only a preliminary conclusion about his analysis of explanation: "If there is satisfactory explanation, there is some general principle involved" (Hospers 1946, 344). If the phenomenon to be explained is particular, then some antecedent conditions need to be added to the principle. Though this seems very close to Hempel's ideas in 1942 or Feigl's conception in 1945, neither paper was mentioned. Again, Hempel had not discussed explanation as a distinct aim of science in 1942. Moreover, unlike Hempel or Feigl, Hospers had no interest in formal characterizations of his subject matter. He was mainly interested in the analysis of satisfying answers to why-questions and builds his analysis around the answers to why-questions in the English language. Similar to Stevenson's remark to Hempel, Hospers added that "the laws alleged must be true ones, else there is no true explanation" (Hospers 1946, 345). At the end of the paper Hospers also briefly considered the possibility that science actually explains nothing but only describes. He denied this by looking to why-questions. Given that one can ask why-questions which can be answered, people can actually state explanations. (Hospers 1946, 354). Consequently, scepticism with regard to the possibility of explanations in science was for him unwarranted.

Around the same time, another American philosopher also engaged with the topic of explanation. In 1946 David Miller, professor at the University of Texas, wrote a reaction to Feigl's position on explanation for *The Psychological Review*. Miller specifically focused on the distinction between description and explanation, which for him were two distinct cognitive aims of science. He attributed to his contemporary logical empiricist colleagues the position that any statement other than a descriptive one "is to be condemned as metaphysical and as a hindrance to science" (Miller 1946, 241). He referred to Mach, Pearson and Poincaré, as the origins of such position and contrary to their opinions he argued that, explanation, next to description was also "a function of science": "explanation gives an intelligent, rational account of what would otherwise be logically and rationally disconnected sense experiences. Description without explanation is blind and sterile" (Miller 1946, 241). Miller attributes two cognitive features to the function of explanation that distinguish it from description. First, every explanation must make a prediction. According to Miller, scientific laws posit a relation between events that goes beyond the empirical evidence. This bestows a predictive capacity on a law that goes beyond the collection of experienced events from the past. This is "one of the essential characters of explanation; the world of sense experience begins to take on the character of intelligibility" (Miller 1946, 243). Unlike Feigl, Miller believes that theoretical assumptions do more than direct the empirical research. They offer some kind of intelligibility. This is related to the

second distinctive feature of explanation: an explanation makes a phenomenon intelligible by referring to more basic or more fundamental experiences which the person to whom the phenomenon is being explained will grant as psychologically satisfactory. Scientists from the past “looked for kinesthetic sense experiences in terms of which, when made explicit to the mind, the world could become intelligible, or in terms of which their science would be significantly true of the world in the sense that phenomena would then be explained” (Miller 1946, 244–45). Miller calls these explanatory experiences a “native acquaintance with the elements into which a situation can be analysed” (Miller 1946, 244). He gave the example of the experience of walking or lifting that bridges the gap between our human practice and abstract mechanics. Miller remained extremely sketchy about this notion of acquaintance. Nonetheless, it served the purpose to distinguish between description and explanation in scientific inquiry.

In 1947 Miller continued to develop his account of explanation against the positivists in the discussion section of *The Philosophical Review*. Again, he emphasized the distinction between description and explanation as different aims of scientific inquiry: “Are we to conclude that description is as far as science can and should go? To these questions I answer, No” (Miller 1947, 306). Miller now clearly states that he considers explanation as a *metaphysical* aim of science that surpasses the descriptive aim:

“Metaphysics explains sense objects by appealing to inexperienceable entities (to a noumenal order). But why appeal to a noumenal order? Simply because sensed objects are spatially or temporally separated, and to find an order between and among these separated objects of experience we must fill the gaps which separate them. Filling these gaps makes the experienced world intelligible, for it enables us to predict and thereby to control the order of sensed objects. (Miller 1947, 307).

For Miller, modern explanation aims to account for the spatiotemporal continuity between data, which in turn makes these phenomena intelligible. One explains data by positing “interphenomena”. These are “subexperiential phenomena” that are not experienceable, but serve as a logical connection between the phenomena of experience (Miller 1947, 311). Miller admitted that these interphenomena are metaphysical entities. However, he claimed that we must posit them if the universe is to be understood as a complete, and thus intelligible, system (Miller 1947, 312). Interphenomena allow one to posit a lawful relation between phenomena that will also hold in the future. They make scientific prediction intelligible. In the conclusion Miller emphasizes that his concerns were metaphysical. He could not agree with the interpretation of theoretical assumptions in science as logical constructs that have no physical reality.

We cannot identify interphenomena, like light waves or electrons, with their logical structure. They are metaphysical entities. They occupy space and time. They have mass or energy, etc. To say that all we mean by interphenomena are phenomena is false. (Miller 1947, 312)

In modern day terminology, Miller could be considered a realist about scientific theories, even though most contemporary realist would be uncomfortable with Miller's 'native acquaintance'.⁵ In 1947 Miller attempted to rethink what a scientific theory is through the notion of 'explanation', against what he took to be the received view on scientific knowledge. He argued that scientists should be concerned with the intelligible nature of reality, and consequently, they should be concerned with explanation. Miller's standpoint was exactly why Neurath believed it important not to talk about explanation: the notion would reintegrate a metaphysical view on science. Miller's paper is published in the discussion section of the *Philosophical Review* alongside Peter Carmichael's "First Philosophy First". Both papers defended the necessity of metaphysics against strict empiricism. Carmichael's conclusion is a metaphysician's call to arms:

Metaphysics, though it is the foundation of knowledge, the object of knowledge, and the goal of knowledge, is left out, and philosophy itself, wandering in this foreign place, paradoxically appears as antimetaphysics, which is to say as antiphilosophy. (Carmichael 1947, 305)

It is in this context that Miller defended explanation as an autonomous, metaphysical aim of science that is distinct from description and necessary to make the world intelligible. From Stevenson's and Hoppers' inquiry in language analysis to Miller's defense of the need to posit metaphysical entities, the American intellectual climate stood in great contrast to Neurath's outcries from the continent, warning against the dangerous term 'explanation', referring to a scientific tradition of Mach and Kirchhoff. Two aspects of these early discussions on the notion of explanation in the United States should be noted. First, Feigl, Hoppers and Miller all had in different ways discussed explanation as distinct from description. Feigl wanted to uphold the distinction between descriptive statements and explanatory inferences. Hoppers aimed to show that there are valid scientific answers to why-questions, explanations, which cannot be considered as descriptive statements of the world. Lastly Miller, on his own terms had distinguished between the empiricist aim of description and the metaphysical aim of explanation that produces intelligibility, and he took both to be aims of scientific inquiry. Miller's and Hoppers's positions made explanation an autonomous aim of science – a position which was considered antithetical to logical empiricism at that time. Second, Hoppers and Feigl had both analyzed the notion of explanation as an answer to why-questions. Feigl merely noted this relation in passing, while Hoppers had started his entire analysis of explanation from the answers to why-

⁵ Note that Meyerson's discussion of Cassirer similarly showed his 'realist' reliance on some kind of intuitive acquaintance: it is the mark of a genius scientist to postulate, without much evidence, the true explanatory hypothesis. In the American context before the second World War, Meyerson was not unknown. His work had been discussed both in the *Philosophical Review* (Wiener 1935) and in the *Journal of Philosophy* (Costello 1925). However, Meyerson is not mentioned by Miller.

questions in ordinary English. These two aspects of the early discussion on explanation would be reflected in the next phase of Hempel's reflection on the notion of 'explanation'.

4.2 Hempel and Oppenheim on Explanation

In the summer of 1946 Hempel announced that he and Oppenheim had started a new project involving explanation.⁶ He wrote to Carnap that it is "logically on a quite elementary level (without reference to confirmation at present; later that may come in as a sort of 'Ausblick' without details); where it will lead, I don't know."⁷ This labour would lead to the 1948 paper *Studies in the Logic of Explanation*, which would take up the aforementioned aspects from the American discussion and, in contrast to earlier logical-empiricist views, offer an actual analysis of explanation as a central concept in the understanding of scientific inquiry. The 1948 paper features the modern concept of scientific explanation immediately in its opening paragraph. There, Hempel and Oppenheim stated what conceptual content the term 'explanation' covered according to them:

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. (Hempel and Oppenheim 1948b, 135)

Whether Hempel or Oppenheim considered this opening paragraph a break with their previous usage of the term explanation is not clear. It was, however, a break with the previous equation of explanation with description in the writings of Mach, Duhem, Schlick, von Mises and Frank. Hempel and Oppenheim now understand explanation as an objective of any kind of rational inquiry that is separate from and cognitively superior to description. They introduce a distinction that was entirely absent from their own tradition of philosophy. And similar to Hospers' linguistic analysis, they understand an explanation as an answer to the question why something happens. Both aspects (the distinction with description and the answer to why-questions) are still crucial in contemporary accounts of scientific explanation

⁶ Hempel maintained his collaboration with Oppenheim throughout his period in New York. From his diaries, it is clear that they met weekly or even bi-weekly. The research episode in the summer of 1944, where they retreated with Olaf Helmer to a hut at alake in order to work on a logic of confirmation, is a well-known, somewhat romanticized episode in Hempel's career (Rescher 1997). The summer of 1946 most likely played a similar role: Hempel and Oppenheim retreated during the summer to finish a paper.

⁷ Hempel to Carnap, 17 June 1946, RC 84-19-25 ASP.

(Woodward 2017, sec. 1). Later in the paper, Hempel and Oppenheim state that explanation should be considered as "understanding in the theoretical, or cognitive, sense of exhibiting the phenomenon to be explained as a special case of some general regularity" (Hempel and Oppenheim 1948b, 145). Hoppers' paper is mentioned in the footnote about predecessors to their account (Hempel and Oppenheim 1948b, 140). Possibly Hempel and Oppenheim picked up the connection between explanation and why-questions from Hoppers' paper. Besides Hoppers they also mention Mill, Jevons, Ducasse, Popper, Hull and Feigl as philosophers who have characterized explanation and prediction in a similar way. They describe their own work as a summary and explicit statement of what many scientists and methodologists had already recognized. The actual historical emergence of explanation in their work, however, shows that this is a misrepresentation of what is actually happening: an autonomous concept of scientific explanation is a novel notion, at least given the background of their own philosophical tradition. In the 1948 paper the concept of explanation has reached a certain degree of autonomy: it is the prime objective of rational inquiry and it should yield insight or theoretical understanding why phenomena occur. In contrast to the Mach-Duhem tradition, the term 'explanation' now covers a distinct aim of science and the philosopher of science has the task to analyze this goal properly:

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 1948b, 135)

It is not clear why Hempel and Oppenheim assume a "general agreement" over this aim of science. Four years before, Neurath had vehemently pleaded against a relapse into the conceptual distinction between description and explanation. Moreover, many scientific philosophers of the generation before Hempel had refused to incorporate a separate concept of explanation in their analysis of science. And even though Hempel had used the term in his 1942 paper, he had not started his investigation from the idea that explaining and describing phenomena are different. The term "explanation" there only covered the idea that science investigates the regular connection between events. Based on the available historical evidence it is difficult to account for how Hempel and Oppenheim procured the idea that explanation is a well-established aim of science distinct from description. In Hempel's correspondence explanation is mentioned very little, and Hempel's diaries of the relevant years have not been preserved. Oppenheim only briefly mentions his work with Hempel on explanation in a letter to Reichenbach.⁸ However, it is clear, based on

⁸ Oppenheim to Reichenbach, 18 November 1946, HR 038-02-17 ASP.

Stevenson's letter and Hoser's or Miller's paper, that 'explanation' was not considered to be a philosophically problematic notion in American philosophy of the 1940s. There is no evidence that Hempel or Oppenheim read Miller's contributions, even though Miller's position also posited explanation as a distinct, though metaphysical, aim of science. So, Miller most likely was not the source. Herbert Feigl's discussion had already shown that one could legitimately discuss the notion 'explanation' from a logical empiricist's standpoint, if only one had a proper understanding of it. From the point of view of Feigl's and Hoser's paper, an autonomous notion of explanation as an aim of science is no longer something dangerous or metaphysical. The younger generation of logical empiricism, like Feigl or Hempel, had not experienced the anti-explanatory debate first-hand: unlike Frank or Neurath, they had not read Mach and Duhem as exemplars for philosophy of science in the first decades of the 20th century. Instead the younger generation had read Carnap, Schlick or Reichenbach, who were no longer heavily arguing against an explanatory view on science.⁹

After Hempel and Oppenheim introduced the autonomous explanatory aim of science in the 1948 paper as their starting point, they intended to perform a philosophical analysis of this concept. Contrary to the 1942 paper, scientific explanation and scientific laws are not defined by stipulation and then formally reconstructed, they are now analyzed, starting from certain intuitions, similar to Stevenson's and Hoser's approach to the subject. Hempel and Oppenheim open their article by stating some intuitive cases of explanation: the explanation of a temporary drop of a mercury thermometer after its immersion in hot water, or the explanation of the bent appearance of an oar immersed in water. Both cases are meant to show how one intuitively answers the question "Why does the phenomenon happen?" with "according to what general laws, and by virtue of what antecedent conditions does the phenomenon occur?" (Hempel and Oppenheim 1948b, 136). From these "sample cases" Hempel and Oppenheim "abstract some general characteristics of scientific explanation" (Hempel and Oppenheim 1948b, 136). First, they introduce the abstract analytic terminology of explanans and explanandum. Second, they formulate four conditions of adequacy for the explanans that are abstracted from the intuitive cases. These conditions form the basis for the schema of the deductive-nomological model, which is now explicitly understood as a model, not a definition, of scientific explanation (Hempel and Oppenheim 1948b, 138).

Two of the conditions of their analysis resemble conditions of the 1942 definition of explanation. The condition (R1), that the explanandum must be a logical consequence of

⁹ For Hempel, Carnap's *Aufbau* was the exemplar of philosophy that drew him to the professional discipline. In a letter of 7 August 1952 to Carnap he wrote about the book: "I re-read parts of the *Aufbau* and I marvelled again at it (it once was The book of philosophy for me and brought me to the Prague congress to see Carnap for the first time), and I must say that I find it still pretty incomprehensible how somebody can write a book calling for this kind of concentrated effort and organization." CH 11-01-19 ASP.

the explanans, was already present in condition (2b) of the 1942 definition, namely that the argument contains universal hypotheses that allow a deduction of the occurrence of the event to be explained (Hempel 1942, 36). And the condition (R3) that the explanans must have empirical content was contained in the old condition (2a) that statements of hypotheses and determining conditions had to be well confirmed (Hempel 1942, 36).

Two conditions are novel. It is now stipulated through condition (R2) that general laws must be required for the derivation of the explanandum (Hempel and Oppenheim 1948b, 137). This condition is added to prevent the possibility of an absence of laws from the explanans. Such absence is taken to go against the earlier introduced intuition that an answer to a why-question has to contain a reference to general laws. This condition could not have been part of the 1942 definition, since there was no analysandum (explanation) that required general laws in its analysans. Hempel had previously defined explanation as the backwards systematization of experience through laws. Stating that explanations (as systematizations of experience) must contain laws would have been tautologous.

Condition (R4) addresses one worry that was introduced in Stevenson's letter of 1942 and can also be found in Hospers' paper. It stipulates that laws and initial conditions must be true. Thus, laws that operate in sound explanations are not just well-confirmed hypothetical generalizations. Laws that operate in sound explanations must be true regularities, independent from the current state of research. Otherwise the concept of a law would be relative to the state of the evidence, and Hempel and Oppenheim consider this not to be in accord with "common usage" (Hempel and Oppenheim 1948b, 138 & 152). Consequently, only true generalizations yield a sound explanation.

This initial analysis of explanation into four conditions of adequacy also leads Hempel and Oppenheim to discuss some counter-examples. They refer to cases where the general laws are not part of an explanation, e.g. a car turned over on the road 'because' one of its tires blew out while the car was travelling at high speed (Hempel and Oppenheim 1948b, 139). 'Because' is used as an indication of an explanation statement, and since this statement does not contain a general law these examples are a challenge to the model. Such cases, Hempel and Oppenheim argue, should be interpreted as incomplete explanatory arguments that can only be evaluated as successful if the tacit general law is made explicit. The solution is similar to Hempel's discussion of explanation-sketches in his 1942 paper, but now it was applied to common-sense explanations rather than scientific history. In §4 Hempel and Oppenheim notice that their characterization of scientific explanation so far is based only on a study of cases taken from the physical sciences. "But the general principles thus obtained apply also outside this area" (Hempel and Oppenheim 1948b, 140). Consequently, Hempel and Oppenheim introduce some examples and intuitions from the social and biological sciences and try to argue that their model can fit these intuitions. Whereas Hempel in 1942 was interested in refuting a methodological distinction between various types of science, the interest in the social sciences is now understood as a test-case for the offered analysis of scientific explanation. The use of these cases shows that Hempel and

Oppenheim assume some kind of unity of science. However, the unity of science receives no explicit mention in the paper, since its goal is to analyze the explanatory aim of science (which is taken in the opening paragraph to be an aim that is universal across scientific activity – an assumption that had been strange to logical empiricist writing up until that point).

By performing the analysis, Hempel and Oppenheim diverge from Neurath's advice, and the tradition of thought in which he was situated. Contrary to Mach's, Duhem's or Frank's work, they do not start their philosophy of science from scientific work, or a history of scientific developments. Instead they start from an intuitive concept, find some intuitive examples, abstract conditions and perform a logical analysis, which can in turn be tested against new intuitive case studies. Hempel and Oppenheim thus take language intuitions about explanation as valid elements of their inquiry into science, which is in line with the methodological approach of Stevenson and Hossers. They constantly pay attention to toy-examples like the car accident or the bent oar, and give no reference to scientific work. Moreover, they use the "customary usage" of a term as valid criteria of adequacy for analysis and often pay attention to the use of the word "because". Besides this ordinary language analysis, Hempel and Oppenheim also aim to rework their model of explanation into "definitions of law and of explanation for a formalized model language of a simple logical structure" (Hempel and Oppenheim 1948b, 152).

This formal definition of law and explanation is discussed in Part III of the paper through the introduction of a formal model language *L*.¹⁰ Because Hempel and Oppenheim found out in their intuitive cases that the subsumption under laws is a crucial condition of explanation, it is of central importance for their analysis to precisely formulate what a law is – this was also Stevenson's initial advice. The introduction of the model language *L* enables Hempel and Oppenheim to state precisely what a law is, which is impossible given the vagueness of the English language concerning purely qualitative predicates. They define a law as a true, purely universal sentence (Hempel and Oppenheim 1948b, 158). A purely universal sentence is characterized syntactically as a universally quantified sentence containing no individual constants. From these initial definitions, they define a fundamental theory as a true, purely generalized sentence, and a derivative theory as an essential, but not purely, generalized sentence that also is the consequent of a fundamental theory (Hempel and Oppenheim 1948b, 159). This leads them to the following definitions.

¹⁰ Gary Hardcastle puts much emphasis on this formal analysis in 1948 paper, which contains the heart of the argument according to him. For a more detailed analysis of the formalization that Hempel and Oppenheim introduce, see (Hardcastle 2002, 140–43). According to Salmon, the formal explication is also the heart of the 1948 paper (Salmon 1989, 19–23).

(7.5) An ordered couple of sentences, (T, C) , constitutes a potential explanans for a singular sentence E only if (1) T is essentially generalized and C is singular, and (2) E is derivable in L from T and C jointly, but not from C alone.

(7.6) An ordered couple of sentences, (T, C) , constitutes an explanans for a singular sentence E if and only if (1) (T, C) is a potential explanans for E , and (2) T is a theory and C is true. (Hempel and Oppenheim 1948b, 160)

(7.5) is, however, problematic according to them, since it does not exclude self-explanation. Any potential explanation of the schema $\{T, C\} \rightarrow E$ “can be restated in the form $\{T, C_1, E_1\} \rightarrow E_1, E_2$, where E_2 follows from T alone, so that C_1 is entirely unnecessary as a premise; hence, the deductive schema under consideration can be reduced to $\{T, E_1\} \rightarrow E_1, E_2$, which can be decomposed into the two deductive schemata $\{T\} \rightarrow E_2$ and $\{E_1\} \rightarrow E_1$ ” (Hempel and Oppenheim 1948b, 162). Hempel and Oppenheim believe that this purely formal characteristic that is a consequence of their formal definition of theories, cannot be avoided. According to them “customary usage” provides no guidance to delimit the level of self-explanation that can be allowed. Another type of partial self-explanation, however, leads them to amend (7.5):

Let $T_1 = '(x)P(x)'$ and $E_1 = 'R(a, b)'$; then the sentence $C_1 = 'P(a) \supset R(a, b)'$ is formed in accordance with the preceding instructions, and T_1, C_1, E_1 satisfy the conditions of (7.5). Yet, we would not say that (T_1, C_1) constitutes a potential explanans for E_1 . The rationale for the verdict may be stated as follows: If the theory T_1 on which the explanation rests, is actually true, then the sentence C_1 , which can also be put into the form $'\sim P(a) \vee R(a, b)'$, can be verified, or shown to be true, only by verifying $'R(a, b)'$, i.e., E_1 . In this broader sense, E_1 is here explained by itself. [...]

The assumption that T is true must not imply that every class of true basic sentences which has C as a consequence also has E as a consequence.

In order to avoid these purely formal counter-examples Hempel and Oppenheim add a third condition to (7.5): T is compatible with at least one class of basic sentences which has C but not E as a consequence (Hempel and Oppenheim 1948b, 163). This final emendation leads them to conclude that they can give a precise interpretation of the frequently used phrase “this fact is explainable by means of that theory”, an interpretation that could withstand all kinds of counterexamples, formal and informal. At the same time, they fully realized themselves that their model language L is not sufficient for an application to actual scientific theories. They could not give a single application of their formal definitions to actual theories. Constantly, they either reverted to formal examples, or to toy examples like “all metals are good conductors of heat”. Nonetheless, they believed that their analysis of law and explanation is “far from trivial even for our model language L , and our analysis still sheds light on the logical character of the concepts under investigation also in their application to more complex contexts” (Hempel and Oppenheim 1948b, 158). Unlike Hempel’s 1942 paper the formalization did not serve a program of emendation: the formalization was not meant to show how historical claims were in fact empirical. The

formalization was an analysis of a purported, central cognitive function of science. The analysis had started from intuitions about explanation, and was continued in a formal language. For the first time, Hempel had used all kinds of language intuitions in his philosophical reasoning process.¹¹ Even though the end result of analysis satisfied all four conditions of adequacy and thus an intuitive notion of explanation, it was far removed from any application to scientific language or scientific practice.

4.3 The Revenge of Systematization

One would expect the paper to stop once the goal of analysing the concept of explanation has been reached. The paper, however, continues with Part IV, which is explicitly attributed to Hempel alone. In this part, Hempel connects the analysis of explanation to the research done by Carnap, Helmer, Reichenbach and himself on theory evaluation through inductive logics. As Hempel had promised Carnap in 1946, this section offers a certain "perspective" on confirmation starting from the analysis of explanation. Thus, Nicolas Rescher was right to emphasize the historical relation between the 1948 paper and previous work on inductive logics by Helmer, Hempel and Oppenheim (Rescher 1997, 346–47). However, Hempel's added perspective also testifies to the tensions arising from the introduction of explanation as a distinct aim of science. Hempel, seemingly unaware, shifts his perspective on laws and explanation in Part IV.

Scientific laws and theories have the function of establishing systematic connections among the data of our experience, so as to make possible the derivation of some of those data from others. According as, at the time of the derivation, the derived data are, or are not yet, known to have occurred, the derivation is referred to as explanation or as prediction. (Hempel and Oppenheim 1948b, 164)

The quest for Hempel is to find a quantitative measure for the derivational power of a theory given a set of data. Hempel has already discussed a qualitative measure of this kind in his 1945 discussion of confirmation. In comparison to the analysis of explanation in sections I–III, this discussion of confirmation is a new goal: a sound explanation could only contain true laws, and these are unrelated to the current state of inquiry or data-collection. From the

¹¹ Arguably, Hempel's 1945 paper also starts with an intuitive notion of confirmation in section 3. However, in that section Hempel presents it as the formulation of the confirmation-relation by Jean Nicod. While he had previously characterized it as the "the intuitive notion of confirming evidence", he remains mostly silent throughout the rest of the essay why Nicod's account would be intuitive (Hempel 1945a, 8). In contrast, the 1948 paper openly discusses intuitive toy examples of explanation as a starting point for analysis.

above quote, however, it seems that laws and theories function in relation to experimental data. This was also the 1936 definition of explanation as "a specification of laws that connect data in a determined way." In this sense of the word 'explanation', connections between data don't yield some kind of theoretical understanding, they are mere systematic representations of our experience. In part IV Hempel constantly talks about "the systematic power of a theory T with respect to a finite class K of data, or the degree to which T deductively systematizes the information contained in K" (Hempel and Oppenheim 1948b, 165). Contrary to the previous analysis of explanation, Hempel is thinking about the systematic relation between theory and data.¹² Thus, in part IV of the 1948 paper the use of the term 'explanation' slides back into the anti-explanatory tradition where the systematization of experience takes up the central role. In the last paragraph of the paper it is possible to see how Hempel understood the relationship between both kinds of analysis:

But the theory of systematic power, in its narrower as well as in its generalized version, is, just like the theory of logical probability, purely formal in character, and a significant application of either theory in epistemology or the methodology of science requires the solution of certain fundamental problems which concern the logical structure of the language of science and the interpretation of its concepts. (Hempel and Oppenheim 1948b, 173)

If the program of inductive logics is to work, one has to be able to apply the logic of theory evaluation to actual scientific theories. This requires an analysis of scientific language – and scientists do sometimes use the term 'explanation', a term that had often been used vaguely and had not yet received any attention in the logic of science. Part III of the paper was meant to remove this lacuna in the logic of science by analyzing the term 'explanation': sentences like "this fact is explainable by means of that theory" could now be interpreted through the offered analysis of explanation in the model language L (Hempel and Oppenheim 1948b, 164). Even though model language L is not sufficient for application to actual scientific theories, the idea seems to have been that future work in the logic of science would realize such a goal.

Hempel's "Ausblick" on theory confirmation shows how the 1948 paper stands on a juncture between the older anti-explanatory view on science and a new perspective on science as the quest for explanation. On the one hand, Hempel's definition of explanation in part IV understands scientific laws and theories as mere systematizations of experience,

¹² Gary Hardcastle has argued that the 1948 paper presents scientific explanation as systematization, and that Hempel and Oppenheim merely assimilate knowing why and knowing that in their paper (Hardcastle 2002, 143–44). Though this might be a good reading of part IV of the paper, it does not correspond to the previous parts, where Hempel and Oppenheim do claim to be analysing explanation in opposition to mere description, and where they add the truth-condition for laws as a necessary element of an answer to why-questions. On my account, this shows how the paper stands on a juncture between two different views on science (see *infra*).

which remains in the boundaries set by the anti-explanatory tradition. On the other hand, the precise analysis of explanation in part III starts from an opposite intuition, that explanation has to yield understanding, independent of the current state of inquiry. For the anti-explanatory tradition this last intuition was a remnant of metaphysical prejudices: explanations should only be understood as systematized forms of the current state of empirical inquiry. Explanation as analysed in part III is not relative to the current state of inquiry: condition (R4), the condition that was added to answer Stevenson's worries, explicitly denies such a connection.

The symmetry thesis in the 1948 paper is another indication of the juncture. In part IV the symmetry between explanation and prediction rests exactly on the relation between theoretical systematization and experience: if the strength of systematization is measured against new data, it can be called 'prediction', if measured against old data, the term 'explanation' is appropriate. The same symmetry is also stated in part I of the paper. There, however, it is not supported by the relation between theory and experience, which was broken off by condition (R4). When Hempel republished the paper in his 1965 book *Aspects of Scientific Explanation* he removed this paragraph about the symmetry thesis¹³:

It is this potential predictive force which gives scientific explanation its importance: only to the extent that we are able to explain empirical facts can we attain the major objective of scientific research, namely not merely to record the phenomena of our experience, but to learn from them, by basing upon them theoretical generalizations which enable us to anticipate new occurrences and to control, at least to some extent, the changes in our environment. (Hempel and Oppenheim 1948b, 138)

Hempel did not say in 1965 why he removed this paragraph. One possibility might be that the paragraph hinges on the idea that explanation is related to the systematization of our experience. One can measure the strength of systematizations by evaluating how they deal with new experiences. This, however, is not in line with the concept that the paper starts to analyze, which goes beyond mere systematizations of experience. Consequently, the paragraph does not belong in part I.

It may be coherent to have part III and part IV in the same paper. One might say that explanation is discussed from different perspectives. Hempel clearly thought that both perspectives could relate to each other: the program of theory evaluation (part IV) required a logical reconstruction of scientific language (part III). However, the understanding of explanation in part III was grounded in intuitions that were foreign to the earlier anti-explanatory program. These intuitions assumed the cognitive superiority of explanation as an aim of science over description, and they connected the analysis of explanation to the analysis of why-questions. In that sense it introduced crucial elements of Hoser's and

¹³ This was already noted in (Douglas 2009, 450).

Miller's views on science into the discussion. It also diverged from the logical empiricist's reasoning on explanation. Feigl in 1945 had still taken up a traditional position, similar to Carnap, von Mises or Schlick, and incorporated a notion of explanation into the philosophical analysis of science by calling a specific deductive inference that explicates the relation between theoretical assumptions and observations an "explanation". Feigl's incorporation of explanation into philosophy of science, in 1945 at least, does not yet rely on an identification of scientific explanation as an aim or aspect of scientific inquiry. Unlike Feigl, Hempel and Oppenheim had not attempted to show how a logical empiricist can talk about explanation. Instead they assumed a starting point that differed from their own tradition: scientific explanation is a fundamental aim of science, and they offered a model of this aim through the logical analysis of science. Moreover, since part I of the paper also introduced the method of analysis through language intuitions, Hempel and Oppenheim allowed their analysis to be criticized from ordinary language perspectives.

Hempel and Oppenheim in 1948 would not have understood the innovation of part I-III as the introduction of a juncture in philosophical reasoning about science, since this only becomes clear in hindsight, once philosophers start to conceive of science as a cognitive activity aimed at explanation. This only happens from the late 1950s onwards. For Hempel and Oppenheim at the time of writing, parts I-III was probably just an addition to the logical empiricist program of rational reconstruction of scientific languages, even though the methodological and conceptual starting point of the analysis was taken over from the American context and was foreign to the Mach-Duhem tradition.

4.4 The Truth-Condition

Hempel and Oppenheim's paper was immediately commented on by David Miller in the next issue of *Philosophy of Science*. Miller focused on the truth-condition that Hempel and Oppenheim had introduced. Miller found it strange that Hempel and Oppenheim "accepted the theory of finality and absoluteness of law", which is "incompatible with the general anti-mechanistic, anti-rationalistic view accepted by the authors (Miller 1948, 349). Miller quotes the argument of the truth-condition from the 1948 paper, which is the argument contained in Stevenson's letter, in full, and he believed that it was inconsistent with Hempel and Oppenheim's conception of evidence. According to Miller they should not accept a notion of a law that is absolute and final, unconditional on the evidence that one currently has:

I think we will find that if the test of a given "law" is in what can be predicted on the basis of it then no law can be accepted as final. In fact, it is the exceptional experience [...] that puts the law in question. And, as we will see from the authors' own argument,

if new "evidence" shows at best that a given law is probably false, and if new "evidence" is coming in all the time, no law can be accepted as final or as necessary. (Miller 1948, 349)

Miller countered Hempel and Oppenheim's argument for the addition of the truth condition. Hempel and Oppenheim had claimed that such addition was in accordance with the customary usage of the notion 'law' among scientists. However, since this notion of 'law' was inconsistent with their general empiricist epistemology, Miller argued that they "should not lean on usage for their definitions at the expense of consistency" (Miller 1948, 349). Miller did not bring his own metaphysical conception of explanation under discussion. The quest to make the world intelligible by positing entities and relations beyond the experiential order was left unmentioned. His commentary solely revolved around the internal inconsistency within the analysis of the 1948 paper.

Hempel and Oppenheim's argument for the truth-condition was taken over from Stevenson: according to ordinary usage, a law is true independent of the confirmation that one has of it. Similarly, Hospers on his ordinary usage account had argued that the laws used in an explanation must be true ones. Consequently, on these accounts, explanation entails an independence from the available evidence. This also was the explicit motivation for Hempel and Oppenheim to add the truth-condition. However, according to the anti-explanatory tradition, this idea that laws are representations of the world independent of the empirical evidence that one has, is a metaphysical idea that cannot be supported by scientific inquiry. As argued in 2.2, this was also Frank's understanding of the dialectic of physical theory: laws only function in tandem with the available experiences at a specific point in time. Feigl had not diverted from this tradition in his 1945 article: the function of general laws and theoretical assumptions is to cover "all relevant and available descriptive data; and there is no need for climbing higher on the tower of constructs if all the data one cares to see are within sight" (Feigl et al. 1945, 285). So for Feigl, theoretical laws can only be understood in relation to a set of empirical evidence. Duhem also claimed that theoretical laws function in relation to the available set of observations, but according to Duhem this function was explicitly historical. A theoretical law could be considered a natural classification, if it was able to incorporate novel observations and empirical laws. In historical hindsight, some classifications are better. Duhem took his reference to certain laws as natural classification to be representative of the actual historical developments of concepts and laws in the sciences. The process of natural classification in the history of science shows that it is wrong to attribute finality to theoretical laws or concepts: although one can evaluate a theoretical law or concept based on its ability to incorporate future empirical observations, one can never reach an endpoint for the evaluation of a law or a concept.

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: the law is essentially provisional. The estimation of its value varies from one physicist to the next, depending on the means of observation at their disposal and the accuracy demanded by their investigations: the law is essentially relative. (Duhem [1906] 1991, 174)

Thus, Hempel and Oppenheim's truth-condition, which served to sever the understanding of laws from their empirical evidence, had no equivalent in the anti-explanatory tradition. Also their motivation for the condition, the usage of the term "law", was strange to this tradition.

Hempel's opponent in his 1942 paper, Heinrich Rickert, also had a truth-condition in his account of explanation. This condition was explicitly added by Rickert to go beyond the anti-explanatory tradition: classifications have to be necessary. Unlike Kirchhoff's descriptivist understanding of natural scientific laws that posited laws as simplifications of a set of observations, Rickert believed that laws ought to have an unlimited validity (Rickert 1929, 108–13). This implies that they are not relative to a specific point of empirical inquiry. "The difference [between a descriptivist and a natural law] lies in the fact that the descriptivist have no validity independent from the knowing subject, while natural laws do" (Rickert 1929, 114).¹⁴ Natural laws are unconditionally valid and have to overcome the randomness of conceptual abstraction from of a finite set of empirical observations (Rickert 1929, 61). For Rickert such unconditional validity is a logical endpoint, the highest form of the natural scientific concept formation. Only in this form can the theoretical laws explain: they show how an event was absolutely necessary. Rickert also talks about this condition of unconditional validity as the truth condition:

For all natural scientific concept formation this is the implicit requirement: one cannot only intend to form mere words or their complex in the form of definitions, but one also has to intend the level of judgements, i.e. a level of logical structures that are true.¹⁵

Rickert's motivation to introduce the truth condition was not similar to Hospers' or Stevenson's. He was concerned with finding an absolute endpoint of logical inquiry: only necessary classification, as an ideal endpoint to strive for, could overcome the extensive and intensive inestimability. Hospers and Stevenson, however, were concerned with ordinary language intuitions about laws and explanation. Nonetheless, in their conclusion all three agreed that laws could only operate as explanations if they are independent from the empirical observations that are currently available. Because Hempel and Oppenheim in

¹⁴ Der Unterschied ist nur der, dass diese Geltung keine vom erkennenden Subjekt unabhängig bestehende ist wie die der Naturgesetze.

¹⁵ Das ist bei aller naturwissenschaftlichen Begriffsbildung die stillschweigende Vorraussetzung. Es können daher nicht nur Wortbedeutungen oder deren Komplexe in der Form von Definitionen, sondern es muss stets auch der Gehalt vom Urteilen, d.h. von logischen Gebilden, die wahr sind, gemeint oder verstanden werden.

their 1948 paper pursued a philosophical methodology that was close to Hoppers and Stevenson, they ended up endorsing the truth-condition.

The concept of law will be construed here so as to apply to true statements only. The apparently plausible alternative procedure of requiring high confirmation rather than truth of a law seems to be inadequate: It would lead to a relativized concept of law, which would be expressed by the phrase "sentence S is a law relatively to the evidence E". This does not seem to accord with the meaning customarily assigned to the concept of law in science and in methodological inquiry. (Hempel and Oppenheim 1948b, 152)

When Miller pointed to the resulting inconsistency with an empiricist epistemology, Hempel and Oppenheim reacted. Initially, they agreed with Miller that an empiricist epistemology implies that one needs empirical evidence to assess a lawful statement, but, according to them, this does not imply that an explication of a law for a formal language also has to refer to empirical confirmation. But then why did they add the truth-condition? Although their original position argued for the addition based on the purported usage of the term in the language of scientists, they admitted in their reaction to Miller that the requirement could "be stated more explicitly" (Hempel and Oppenheim 1948a, 351). Their rationale for the introduction of the truth condition is now given the following interpretation:

Whenever we assert a given statement to be a law, we surely mean to assert, by implication, that statement itself. But this is equivalent to asserting the truth of the given statement. Hence, ascription of the property of being a law includes ascription of truth to a sentence. (Hempel and Oppenheim 1948a, 351)

Interestingly, this new rationale has seemingly no implications about the relation between laws and evidence. They defend this rationale by introducing Tarski's truth-schema. They consider the following example:

- (1) Copper is a good conductor of electricity
- (2) Statement (1) is a fundamental law.

By the definitions of the 1948 paper, (2) means the same as

- (3) Statement (1) is true, and statement (1) is purely universal and contains only purely qualitative predicates.

However, by virtue of the Tarski's biconditional for the truth-predicate, (3) can be restated as

- (4) Copper is a good conductor of electricity, and the statement ‘Copper is a good conductor of electricity’ is purely universal and contains only purely qualitative predicates.

Thus in every individual instance of the definition of law, the notion of truth can be eliminated. However, Hempel and Oppenheim could not remove the notion of truth from their definition itself:

Such simple avoidance of the term 'true', however, is no longer generally possible when the term 'law' is to be eliminated from a sentence containing variables for names of statements, such as 'If S is a fundamental law, then the denial of S is not'. This is the reason why the term 'true' could not be eliminated from our general definitions of 'fundamental law' and of 'law'. (Hempel and Oppenheim 1948a, 351)

In this new account the truth-condition was only introduced to attain semantic ascent over laws, which allowed Hempel and Oppenheim to add in their definition that the assertion of a sentence to be a law implied an assertion of that sentence. This reinterpretation based on Tarski can be interpreted in two ways. Either, Hempel and Oppenheim actually retract their original interpretation of the truth condition, since this Tarskian interpretation has no positive or negative implications about the relation between a law and its evidence, while in their original version the condition made a law independent from its evidence. Alternatively, Hempel and Oppenheim misinterpret their Tarskian reading, and interpret it much stronger than it actually is, so that it implies that laws are independent from evidence. Since Hempel and Oppenheim nowhere admit to retracting their interpretation, the latter option is more likely. Whatever the case may be, Hempel and Oppenheim are clearly no longer on board of the anti-explanatory tradition that gave the relation between laws and evidence a central role. Hempel and Oppenheim can reason about explanation and its required laws independently from the relationship between theoretical terms and observations within an empiricist epistemology. Their initial willingness to add the truth-condition based on language intuitions concerning “law” and “explanation”, is a good indication of this.

This does not imply that Hempel and Oppenheim reclaim the explanatory aim of science as metaphysics, as Miller had argued for it. Hempel and Oppenheim nowhere claim that explanation makes the world intelligible by positing interphenomena or relations that go beyond empirical evidence, or that explanation has to show the ultimate necessity of events. However, they do separate description and explanation as two distinct aims of science, and, as a consequence, they now take it as a legitimate task for the logician of science to analyse scientific explanation separately. At the same time, their philosophical method was changing: unlike Hempel’s earlier work, intuitive examples of language use are taken as valid elements in the analysis of a concept. Whereas Hempel in 1942 was mainly concerned to reform language use in historiography in order to show how historiographical claims are

objective, the goal in 1948 is largely to conform the logical model to language use. In Section 1 of the 1948 paper Hempel and Oppenheim argue that the four conditions of adequacy, which constitute the basis for the logical account of explanation, conform to the given set of intuitive examples.

The endpoint for the logic of science in Hempel's reasoning has changed. By 1948 Hempel has equated the formal analysis of scientific concepts with a conceptual analysis. One can assume to have access to proper examples of explanation through intuition. These intuitive examples lead one to analyse scientific explanation as an inference containing lawful generalizations. This initial analysis can then be made precise by capturing it into a formal language, which results in necessary and sufficient conditions for explanation. In the 1942 paper the reconstruction of the function of general laws in a formal language system was introduced in order to account for the stakes surrounding the objectivity of science. By actively reconstructing historical texts based on the offered logical schema, a clear path was offered to interpret historical claims as objective. In that sense the 1942 paper still participated in the debate surrounding Windelband's first stake, how to account for historiography as an objective science. Rickert and Kristeller shared that philosophical question with Hempel, even though they had a radically different answer. By 1948 Hempel had changed the relevant philosophical question. Now, the central question is how to find the logical structure of scientific explanation as it is represented by some intuitive cases. The logic of science has thus shifted from an investigation in the conditions of possibility for objective knowledge to the analysis of language through intuitions.

4.5 What is a Logic of Science?

Hempel did not operate with the methodology of pure language analysis for a long time. By 1965, when his *Aspects of Scientific Explanation* came out, he had abandoned the program of analysis, and adopted what he understood as Carnapian explication.

Like any other explication, the construal here put forward [Covering Law Model] has to be justified by appropriate arguments. These have to show that the proposed construal does justice to such accounts as are generally agreed to be instances of scientific explanation, and that it affords a basis for a systematically fruitful logical and methodological analysis of explanatory procedures used in empirical science. (Hempel 1965, 489)

The program of analysis was definitely abandoned. The model was now required to meet the criteria of similarity and fruitfulness. In *Aspects* Hempel no longer abstracted initial conditions of adequacy from intuitive cases, and the endresult was not an analysis in terms

of necessary and sufficient conditions: “The construal here broadly summarized is not, of course, susceptible to strict ‘proof’; its soundness has to be judged by the light it can shed on the rationale and force of explanatory accounts offered in different branches of empirical science.” (Hempel 1965, 425). How to identify the rationale of explanatory accounts in science, or even identify generally agreed upon instances of scientific explanation, remained vague. The model certainly did not have to be descriptive of the actual formulations of explanations by scientists, or at least not entirely:

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

Consequently, the model is required to have some descriptive adequacy, vis-à-vis the explanations that one finds in science, and Hempel assumes that at least some instances of scientific explanation can be singled out as generally accepted exemplary cases. The question remains, however, exactly how to achieve this descriptive adequacy. A good example of Hempel’s approach to the problem is his argument for the expansion of the covering law model with inductive-statistical explanations. This expansion implies that a valid explanans can not only make the explanandum logically necessary, but also probable. In order to argue for this expansion, Hempel gives three examples. The distribution of phenotypical characteristics in a population of pea-plants can be explained through statistical Mendelian principles. Similarly, one can explain the radio-active decay of radon based on statistical laws, or the Brownian movement displayed by small particles suspended in a liquid can be explained by statistical probabilities which Hempel does not specify (Hempel 1965, 392–93). Without further motivation, Hempel identifies these examples as valid explanation, and then concludes that his model has to be expanded in order to account for them:

As is illustrated by these examples and by others that will be considered soon, accounts in terms of statistical laws and theories thus play a very important role in science. Rather than deny them explanatory status on the ground that nonrealization of the explanandum is compatible with the explanans, we have to acknowledge that they constitute explanations of a distinct logical character, reflecting, we might say, a different sense of the word ‘because’. (Hempel 1965, 393)

At the same time, Hempel also refuses to account for certain common-sense conceptions of explanation. Certain laws, like the law concerning the period of a suspended pendulum, are laws of coexistence and can be used in multiple ways. One can deduce the length of the pendulum from the conjunction of the law and a statement of its period, but one can also conversely deduce the period of the pendulum from the conjunction of the law and a

statement of its length. Hempel admits that it is hard on common sense to recognise the former as an explanation. However, “in cases such as this, the common-sense conception of explanation appears to provide no clear grounds on which to decide whether a given argument that deductively subsumes an occurrence under laws is to qualify as an explanation” (Hempel 1965, 353). Why the example of the pendulum does not solicit a change to the model, but Hempel’s examples of statistical principles do, is never clarified. Also, how scientists themselves think about the term “explanation” is accounted for in various ways. In discussing whether a valid explanation has to be a reduction to familiar principles, Hempel both uses and discards the reasoning of scientists about explanation. First, he quotes what a sociologist, George Homans, says about explanation:

Incidentally, Homans goes on to say that the requisite ordering of a body of empirically established sociological facts, represented by low-level generalizations, calls for an explanation of those facts; and that such explanation is achieved by means of a 'set of more general propositions, still of the same form as the empirical ones, from which you can logically deduce the latter under specified given conditions. To deduce them successfully is to explain them.' (Hempel 1965, 431)

This is taken as an argument for Hempel’s model. Three pages later, Hempel discusses what two physicists, William Thomson and Oliver Lodge, in the past have said about mechanical models as explanations. But, the accounts of these scientists had to be reinterpreted in light of Hempel’s own account. Hempel argues that their position on the required familiarity of explanatory principles must be reinterpreted:

These pronouncements [of Thomson and Lodge] reflect variants of the idea that explanation in science must involve a reduction to the familiar. What this variant demands is not simply that an explanation somehow render a phenomenon plausible or familiar, but more specifically that it provide a model governed by the laws of mechanics, which in this context are accorded the status of familiar principles. (Hempel 1965, 434)

Hempel reinterprets Thomson and Lodge as saying that mechanical explanation follows his Covering Law model. Why Hempel is justified to reinterpret their views in light of his own model is unclear. Either he can reinterpret what scientists say about explanation based on a normative model of explanation, but then he cannot use the account of individual scientists as an argument for his normative model, as he did in the case of Homans. Or he can account for his model in terms of what scientists say about explanation, but then he should be consistent and attempt to incorporate everything scientists say about explanation, unlike what he did with Thomson and Lodge. Although Hempel constantly uses examples or quotes to argue for the descriptive adequacy of his model, he never states why he chooses particular instances of scientific explanation as exemplars for the model and why he excludes others. One might say that Hempel was in fact looking for a reflective equilibrium between the available intuitions and examples. However, Hempel himself never stated that

this was his philosophical agenda. Officially Hempel executed the program of Carnapian explication. Against that latter background, Hempel interpreted the crucial fruitfulness-condition in the following way: “soundness [of the model] has to be judged by the light it can shed on the rationale and force of explanatory accounts offered in different branches of empirical science” (Hempel 1965, 425). But who or what is the measure for the rationale that a model can uncover? Possibly, this could be the judgement of a philosopher who has weighed all his intuitions against taken-for-granted examples, but Hempel does never explicitly tackle this methodological question, which arguably was haunting him ever since his first correspondence with Neurath on the use of logic in the analysis of science.

Hempel’s methodological issues already manifest themselves in his introduction of the covering law model in *Aspects*, where he uses an example of explanation that he attributes to Dewey. Supposedly, Dewey had given the following explanation for the emergence and retraction of soap bubbles from under the rim of a glass that had been taken from hot suds during dish washing:

Transferring the tumblers to the plate, he had trapped cool air in them; that air was gradually warmed by the glass, which initially had the temperature of the hot suds. This led to an increase in the volume of the trapped air, and thus to an expansion of the soap film that had formed between the plate and the tumblers’ rims. But gradually, the glass cooled off, and so did the air inside, and as a result, the soap bubbles receded. (Hempel 1965, 336)

Hempel takes this example to introduce both types of explanatory premises that his model demands: particular facts and uniformities expressible by means of general laws. While the example contains instances of the particular facts that could easily be reconstructed as “the tumblers had been immersed in soap suds of a temperature considerably higher than that of the surrounding air”, the example contains no reference to general laws. Hempel solves the issue by saying that some laws are hinted at, and that others “are clearly presupposed in the claim that certain stages in the process yielded others as their results” (Hempel 1965, 336). Thus, already alongside the introduction of his first exemplary illustration of explanation, Hempel has to adjust his example in order to have it fit the model. In the 1942 paper, the deductive-nomological scheme was introduced as a scheme that could serve the normative reconstruction of historical texts. In 1965 Hempel intended the scheme to be a model that did justice to generally accepted instances of explanation. But, unlike in the 1948 paper, the model was no longer subjected to strict, necessary and sufficient conditions. This resulted in a loose interpretation of the descriptive adequacy of the model: how a case of scientific explanation related to the model constantly varied. All kinds of examples were taken into account: intuitive, everyday examples like Dewey’s dishes, hyperspecialised examples that were never made explicit, like Brownian motion, and sometimes also the accounts of explanation by scientists. Though the model rarely applied directly to any of them, Hempel always showed how the model could shed a light on any of these cases in one way or

another. Dewey's dishes is a good example of this process: in the quoted passage Dewey does not discuss explanation as an aspect of inquiry; instead he discusses how reflection and experimentation drive inquiry. Given Hempel's work, one can, nonetheless, understand Dewey as giving an explanation. Hempel's work realigns various texts or activities under the umbrella of explanation.

By 1965 *scientific explanation*, or at least its "rationale", could be attributed to a great variety of texts: scientists were concerned with it, scientific theories were aiming for it, and scientific language was rife with it. From the point of view of a philosopher of science working in the 1930s it would have been unimaginable to debate the implications of Dewey's dishes for the interpretation of scientific explanation. By the 1960s this had become part of the reasoning process in philosophy of science. What one takes to be exemplars of science, and how one reasons about these exemplars had changed between the 1930s and 1960s, so that explanation could become an undeniable aspect of science, so much so that the concept could cover the great variety of exemplars that Hempel discussed in *Aspects*.

4.6 The Reification of Scientific Explanation

How did scientific explanation become a unifying theme within the philosophical domain of reasoning about science? One could assume that Hempel and Oppenheim's 1948 article aligned a widely accepted aspect of scientific inquiry, namely explanation, with logical-empiricist philosophy of science. However, this assumes that scientific explanation was already considered as a major aim of scientific inquiry among philosophers reflecting on science before the 1948 paper and that there were exemplars of scientific explanation that had already been identified in philosophical reasoning. As mentioned in 4.2, there is no proof to support this claim: it is highly unclear why Hempel and Oppenheim introduced scientific explanation as a generally accepted aim of scientific inquiry in their paper. What actually occurred was the opposite: their assumption concerning scientific explanation was picked up, and through the discussion that came out of the paper, the assumption was reified. Hempel and Oppenheim's analysis of scientific explanation in the 1948 paper attracted critical attention from the middle of the 1950s onwards. There were counter-examples both to the formal and informal conditions and characteristics of the model. Both types of criticism reified Hempel and Oppenheim's assumption about explanation as an aim of science, and it especially ensured that all kinds of examples or texts were framed as scientific explanations. After this phase of early criticism scientific explanation had become exactly what Hempel and Oppenheim opened their article with, namely a widely accepted

aspect of science. Four early critics of the informal conditions and characteristics from the 1948 paper should be mentioned: Scheffler, Scriven, Yolton and Hanson.

Israel Scheffler reacted in 1957 to Hempel and Oppenheim's basic assumption "that explanation and prediction represent the central purpose of science and are epistemologically basic" (Scheffler 1957, 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 given a current configuration of celestial bodies and a set of laws, deduces a configuration of celestial bodies from the past. Though such deduction fits the logical model of Hempel and Oppenheim, Scheffler believes that this deduction 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, 301). Since the scientific inference of Hempel and Oppenheim's model did not account for a causal notion, Scheffler concluded that they, in fact, 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, 302)

Thus Scheffler introduces 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 ideas about explanation which were introduced through the quite shallow example of the astronomer: inferring past events from current ones cannot be conceived as explaining. Because Hempel and Oppenheim had started their analysis in 1948 from intuitive examples of explanation, they were susceptible to this kind of refutation which introduced alternative intuitions concerning such examples. Sylvain Bromberger's well-known flag-pole example that was introduced to Hempel at a conference (Bromberger 1992, 8), would also exploit the same intuitive ambiguity: inferring the length of a flag-pole from the length of its shadow, could not be considered a valid explanation of the length of the pole.

Scheffler also discussed the truth-condition: he agreed that explanations should be true, and he gave this condition its original interpretation, namely that an explanation should be independent from the current state of the evidence. Whatever Tarskian explanation Hempel had given for the truth-condition, it was clearly neglected in the subsequent debate.

Scheffler uses the truth-intuition and its accompanying condition to logically distinguish explanation from prediction: “far from differing only in pragmatic relationships, explanation and prediction have different logical characteristics: explanations are true, predictions need not be” (Scheffler 1957, 298). Here again, Scheffler used the introduction of language intuitions concerning explanation, namely that it should be true, to argue against a characteristic of Hempel and Oppenheim’s model.

Another early critic was Michael Scriven, who, similarly to Scheffler, disagreed with the intuitions about explanation that Hempel and Oppenheim had introduced to arrive at the four conditions of adequacy. Scriven 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, 53)

For Scriven an explanation yields information that makes a phenomenon intelligible to an epistemic agent. As a result of the introduction of this alternative intuition about 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, 63). By realigning explanation to human understanding, Scriven reintroduced the relation between explanation and an epistemic agent, which condition (R4), 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, 196). “Not deducibility, but intelligibility constitutes the basic feature of the logic of explanation” (Yolton 1959, 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, 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 Newton, 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, 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 on various bases the four conditions of adequacy 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”. Similar to *historiography as a science* in German philosophy at the beginning of the 20th century, *explanation* increasingly became something at stake for philosophers of science at the end of the 1950s. Accounting for this stake, partly meant reacting to Hempel and Oppenheim’s paper, and their opening examples and intuitions. Unlike, “Some aspects of theoretical biology”, the paper by Ralph Lillie that was published in the same issue of *Philosophy of Science* as the 1948 paper, “Studies in the Logic of Explanation” became an important point of reference, a paper that exemplified a particular image on science. Because all criticisms identified the 1948 paper as a reference point, the paper became the “received view” on a question that had not been the point of focus for philosophers of science before the paper was written, but that was installed through the debate that resulted from the paper. Second, these criticisms 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 over science.

Hempel and Oppenheim’s formal criteria for 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, six years

after Miller's direct response. Samuel Gluck had recently received his PhD at Columbia under Ernst Nagel, who had encouraged him to work on scientific explanation - he indicated this in his first note in the paper (Gluck 1955, 34). Gluck accepted the general conditions of adequacy that were laid down by the 1948 paper, but he 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, which 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, 36). In this sense, Gluck's short paper prepares the expansion of Hempel's formal covering law model with statistical laws. 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', which they believed diverged from the "customary notion of explainability" (Eberle, Kaplan, and Montague 1961, 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, and they expanded or solidified the examples of "explanations in science" that could be accounted for in philosophy. They showed that seemingly nonsensical examples about the Eiffel Tower and mermaids could be used to learn something about scientific explanation. These reactions also show that the discussion on explanation had a slow start: at the beginning of the 1950s scientific explanation was not yet a topic of debate; there was no received view. Investigating anthologies for philosophy of science of that period, shows that explanation was not yet settled as a topic for philosophy of science. 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 Feigl's 1945 papers from the *Psychological Review* and earlier discussions of a regularity account of causation by Schlick. In 1953, two anthologies in Philosophy of Science came out, one edited by Feigl and May Brodbeck, and another edited by the New York historian of philosophy, Philip Wiener. In the Feigl-Brodbeck volume, Hempel and 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 analysing the logic of explanation. In the preface Feigl and Brodbeck noted 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 and Brodbeck 1953, 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, vi)

Consequently, Wiener decided to focus on basic concepts and problems, “rather than on the defense of any one school of thought”. He also mentioned the importance of a historical perspective: “Historical perspectives broaden the scope of logical analyses” (Wiener 1953, vi). Neither the 1942 nor the 1948 paper made it into Wiener’s anthology. In the section “Philosophical Analyses and Syntheses”, however, Wiener compiled what he took to be a consistent philosophical debate on the aim of theory construction, starting with Aristotle, who distinguished *knowing why* from *knowing that* in *Metaphysics A*. Then, the anti-explanatory tradition was introduced through texts by Mach, Duhem, Schlick and Frank, while Emile Meyerson was added as the 20th century representative of the Aristotelian distinction. As the English translator of Duhem, Wiener was well-acquainted with the anti-explanatory tradition, and most likely did not see the added value of Hempel and Oppenheim’s paper to that debate.

In 1960 another set of anthologies appeared, and now Hempel and Oppenheim’s paper is taken up consistently. In their anthology Sidney Morgenbesser and Arthur Danto added a section on laws and theories, that contained an excerpt from Hempel and 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 and Morgenbesser 1960, 180). Edward Madden’s anthology *Introduction to Philosophy of Science* contained the first part of the 1948 paper, discussing the four conditions of adequacy; as the only paper discussing explanation, it was part of a section on “Making sense of Science”, which Madden introduced by narrating how Toricelli explained the fact that a suction pump could not raise water more than thirty-four feet (Madden 1960, 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*, editor Peter Nidditch judged that “explanation is widely regarded as the primary explicandum for the philosophy of science, while the best known explications offered mould 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 and 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 during the 1960s. Between 1948 and 1958 only four papers discussed Hempel and Oppenheim, 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. How does the covering law model work, what does it account for, what are some counter-examples? All these questions had become standard.¹⁶

In Carnap's 1958 course on philosophy of science, that got rewritten in *Introduction to Philosophy of Science*, Carnap still talked about explanation in congruence with the anti-explanatory tradition: the term explanation should be understood as the systematization of phenomena within the framework of empirical laws (Carnap [1966] 1995, 12). Even though Carnap mentioned that why-questions had been suspect in early logical empiricist circles and even refers to Mach and Krichhoff, Carnap believed that the term 'explanation' was harmless within the climate of American philosophy, because it could be used as one way to talk about the systematization of experience through laws (Carnap [1966] 1995, 12–16). The distinction between description and explanation as two different cognitive aims of science was never mentioned by Carnap. The truth-condition for explanations from Hempel and Oppenheim's paper was also absent from his writing.¹⁷ Thus, when Salmon stated that Carnap never wrote about scientific explanation (Salmon 2000, 312), this depends on what you take the term to refer to: Carnap simply remained within the bounds of the anti-explanatory tradition in which he had grown philosophically, and where explanation is only considered as the backwards systematization of observations through theoretical assumptions and hypothetical generalizations.

Opposed to Carnap's position was Ernst Nagel's use of explanation in *The Structure of Science* of 1961. Nagel was one of the few philosophers who did not take the distinction between explanation and description from Hempel and Oppenheim's paper for granted. He articulated a philosophical motivation for it in the first chapter of his book. Nagel argues for a difference between common sense knowledge and scientific knowledge: "it is the

¹⁶ This investigation into anthologies is only preliminary: though it shows how the 1948 paper certainly increases in popularity over time, this information should be supplemented with information about the editors, their network, publishing strategies, and preferably also the educational use of these kinds of books.

¹⁷ Carnap does not explicitly refer to the 1948 paper for his discussion of the term 'explanation'. However, it is clear from Carnap's use of the deductive-nomological schema that he understands his account to be in line with Hempel's. Moreover, Carnap did not need to refer to Hempel, since he had used a version of the schema earlier himself.

organization and classification of knowledge on the basis of explanatory principles that is the distinctive goal of the sciences" (Nagel [1961] 1979, 4). Nagel's distinction between description and explanation in the first chapter of his book stands in contrast to Carnap's opening statement that emphasizes the continuity between regularities used in everyday life and in science: "The laws of science are nothing more than statements expressing regularities as precisely as possible" (Carnap [1966] 1995, 10).¹⁸ Carnap would never discuss what makes a regularity statement explanatory, while this was the central issue for Nagel. On Nagel's account, knowledge had to be organized and classified in a particular way in order to become scientific knowledge.

A marked feature of much information acquired in the course of ordinary experience is that, although this information may be accurate enough within certain limits, it is seldom accompanied by any explanation of why the facts are as alleged. (Nagel [1961] 1979, 3)

This starting point resembles Aristotle's philosophy of science:

But yet we think that knowledge and understanding belong to art rather than to experience, and we suppose artists to be wiser than men of experience (which implies that Wisdom depends in all cases rather on knowledge); and this because the former know the cause, but the latter do not. For men of experience know that the thing is so, but do not know why, while the others know the 'why' and the cause. (Aristotle 1953, 444)

This similarity is no coincidence: from the late 1930s onwards Nagel taught Aristotle's *Posterior Analytics* in his Logic of Science courses at Columbia. After Hempel attended Nagel's graduate course on 14 February 1939, two weeks after his arrival in the United States, he noted in his diary: "many historical perspectives; critical discussion of Aristotelian and [xxx] conception of science."¹⁹ Nagel's discussion of the explanatory structure of science was connected to his reading of Aristotle. Not only is the opening of his book modeled on Aristotle's distinction between descriptive and explanatory ways of knowing, Nagel also starts his discussion of the deductive pattern of scientific inference by referring back to Aristotle's position on the matter (Nagel [1961] 1979, 29).

Like Nagel, most philosophers of science in the 1950s and 1960s accepted the cognitive distinction between explanation and description. It was never noticed that Carnap's use of the term explanation was opposed to it. The term 'explanation' that is common in both

¹⁸ Richard von Mises has the same idea that the systematizations in science are not epistemologically different from the systematizations in everyday life. They are only more systematic and unified. See, (Von Mises 1968, 138).

¹⁹ „Viele historische Ausblicke; kritische diskussion Aristotelische und [xxx – unreadable handwriting] Wissenschaftsauffassung.“ Hempel Diary, 14 February 1939, CH 02-1-1 ASP.

Carnap's and Nagel's account hides the opposition of their competing views on science – explanation is not an innocent term, as Neurath had well understood. Except Philip Frank who kept on writing Duhem-inspired pieces like “Why do scientists and philosophers disagree?”, there were no real representatives of the European anti-explanatory tradition in the United States after the second World War. After the initial introduction of explanation as an analysandum in the 1948 paper, it gradually became a research objective in philosophy of science to find a model of scientific explanation that could withstand all counter-examples. The formation of this quest occurred between 1948 and 1965, and during this period scientific explanation was reified as an undeniable aspect within the philosophical domain of reasoning about science.

4.7 Explanation and History

Hempel's 1942 and 1948 papers also had a similar effect on the philosophical reasoning over historiography: by the 1960s philosophy of history had transformed into a reflection on Hempel's covering law model, because the stakes had shifted. When the journal *History and Theory* was launched in 1960, its first issue contained four papers, that, in various respects, discussed Hempel's views on historiography. Isaiah Berlin argued that history should not aim to form general laws (Berlin 1960, 12–16). Arthur Lee Burns investigated whether historians should incorporate theoretical generalizations from studies of international politics into their explanations (Burns 1960). William Dray discussed the search for historical laws by Toynbee, and Gerald Gruman attempted to unravel the explanatory strategy of Edward Gibbon (Dray 1960; Gruman 1960). For a philosopher of science who earlier in his career had found little to no philosophical interest in historiography, Hempel by 1960 stood at the centre of attention in the philosophy of history. In 1961, Rudolph Weingartner in the *Journal of Philosophy* summarized the post-war philosophy of history as a commentary on Hempel:

The discussion [on historical explanation], now so lively, did for all practical purposes begin with Hempel's article; moreover, almost every paper written on this question makes Hempel's analysis of historical explanation its own starting point. (Weingartner 1961b, 29–30)

Louis Mink gave a similar judgement on the philosophy of history in 1966 in *History and Theory*.

Prima facie, almost all of the philosophical literature on philosophy of history in the last decade has dealt with the logic of explanation, and specifically has consisted of

defense and criticism of an increasingly sophisticated version of the "covering-law" model of explanation and a fortiori of historical explanation. (Mink 1966, 26)

The applicability of the covering law model to history had become the entry-point to discuss the epistemological nature of historiography and its relation to the other sciences. Most of the earlier papers from the New York debate on historiography were entirely forgotten. Both Mink and Weingartner believed that this exclusive focus on Hempel's model failed to identify the proper question for philosophers of history, and they both sought to escape the game of analysing historical explanation through counterexamples.

According to Mink, "the gravamen of the issue has rarely been fully evident" (Mink 1966, 26). The true issue, on Mink's account, was the antagonistic confrontation of the scientific culture and the humanistic culture. Ever since the 17th century, philosophers had focused on the "the analysis of the logic of scientific theory, especially physics", whereas historians were concerned with the "discovery and synthesis of 'facts'" (Mink 1966, 27). Mink connected this antagonism to Windelband's distinction between nomothetic and ideographic sciences which could no longer be upheld as an adequate analysis of science by any side of the divide. For Mink the solution could be found by refocusing the attention away from Hempel's model of explanation. Historiography is different from natural science, not because it explained events differently, but because "it cultivates the specialized habit of understanding which converts congeries of events into concatenations, and emphasizes and increases the scope of synoptic judgment in our reflection on experience" (Mink 1966, 47). Abandoning the focus on historical explanation, and thus abandoning the rationale behind Hempel's model would advance the debate.²⁰

Weingartner also sought to escape the debate that came out of Hempel's paper. He believed that the dispute between Hempelians and anti-Hempelians was, in fact, an unrecognised dispute of philosophical method: "the 'distance' from which Hempelians look upon historical explanation is much greater than that from which their critics regard it" (Weingartner 1961b, 31). Philosophers, according to Weingartner, aim to reach "*insight* into what an explanation is; all that follows constitutes a reconstruction and elaboration of that insight in terms of a philosophic position" (Weingartner 1961b, 36). Historians, however, see no reflection of this philosophical insight in their own practice and,

²⁰ From the 1970s onwards this is also what happens in analytic philosophy of history: the question what historical explanation is and by extension much of analytic philosophy of history as a field in anglophone philosophy vanishes once philosophers and historians give up the quest to fit historiography in the same explanatory structure as Hempel's model (Roth 2013, 547). From the 1970s onwards, due to the influence of Hayden White's work, theory of history became focused on the narrative structure of historical texts and its relation to the structure of literary texts. This engendered a whole controversy on its own that sidetracked the question of what makes a narrative explanatory. At the same time, it distanced reflections on historiography from anglophone philosophy (Roth 2007, 280). Recently, historical explanation in the form of narrative explanation has been given new philosophical attention (Roth 2008, 2017; Uebel 2017).

consequently, find the models of the Hempelians lacking in comparison to their practice. Finding a balance between philosophical reconstruction of historical method, exemplified by Hempel, and the mere reporting of what historians do in their work, was the crucial question, according to Weingartner. Endlessly debating over the applicability of a philosophical idea, would not make any advancements.

The effects of Hempel's papers on the philosophy of history were, thus, similar to the effects on philosophy of science: they realigned the discussion towards an analysis of historical explanation, so much so even that by the 1960s philosophers of history, like Weingartner or Mink, had to start their work from Hempel's model, and show how and why their own ideas exactly differed from it. Although both recognised that the larger philosophical questions concerning historiography were much older than Hempel – Weingartner referred to “the great change that had taken place in the method and style of philosophy since the work of Dilthey, Simmel, Weber, Rickert, and others”; and he noted that “Hempel's article is the earliest written wholly in the analytic style of philosophy” (Weingartner 1961b, 30) – neither could escape the grip of the analysis of explanation that was holding philosophical reasoning over historiography captured in a game of counterexamples. Despite their insight that a Hempelian analysis of historical explanation had not increased the insight into historiography, their papers were still written with the Hempelian analysis at the centre of attention. By arguing, like Mink, that history could not fit the explanatory structure of science, one still acknowledged the Hempelian assumption that scientific inquiry aims to explain. Discussing the logic of explanation had become wholly inescapable.

Chapter 5

Conclusion: Scientific Explanation at the Intersection

At the beginning of the twentieth century, there were two intellectual problems concerning history at stake in German academic philosophy: to present an epistemology of history and related fields as sciences, and to use a historical perspective on knowledge itself. These were both challenges that aligned philosophical concerns across various philosophical movements. Logical empiricist philosophers, although most of them were directly concerned with developments in the natural sciences and mathematics, did not escape this theoretical configuration of the German philosophical discourse. Schlick, Carnap, Neurath, Zilsel and von Mises, in different ways, actively took up positions against Windelband's and Rickert's plea to expand the logic or epistemology of science. However, such a negative position also implied a positive project to incorporate history and related fields in what they took to be the unity of science, which engendered controversy among the logical empiricists. Also, the second stake, what it meant to have a historical perspective on science, caused disunity in the logical empiricist network. Zilsel and Neurath believed that a historical perspective on science should be understood as an intellectual method or engagement within scientific philosophy, while Schlick, Reichenbach and Carnap conceived it as a viewpoint external to scientific philosophy proper.

At the end of the 1960s anglophone philosophy of science and philosophy of history were to a considerable extent centred around the analysis of scientific explanation. The question of what scientific or historical explanation is aligned an entire set of intuitive examples, scientific and historical texts, and episodes from the history of science as elements in the philosophical reasoning process about explanation. Hempel and Oppenheim's paper "Studies in the Logic of Explanation" stood at the centre of this intellectual problem in anglophone philosophy: to participate in the debate on explanation was also always to participate in the reflection on the cases, examples and model that Hempel and Oppenheim had brought together in their paper, or on the cases, examples and models that had been given in response to their paper. As a result, 'scientific explanation'

as an independent aim of science and an answer to why-questions came to form a crucial and undeniable aspect of the philosophical analysis of science in the anglophone world.

The development of Hempel's ideas on explanation stands at the intersection of these two configurations of philosophical discourse. Hempel's first use of the term 'explanation' in his 1942 paper "The Function of General Laws in History" still occurred within the older, German configuration: by reconstructing historiographical text within the bounds of a formalized scheme that represented an interpretation of science as the search for laws, Hempel showed how a view on historiography was possible that diametrically opposed Rickert's view. In that sense, Hempel participated in the German debate and took up a position similar to Carnap's or Neurath's: there is no need to introduce a distinct epistemology or logic of historiography. Hempel's formal reconstruction of historiographical texts was, however, open to various interpretations and criticisms. Depending on how one interprets the goal of a formal reconstruction, various possible routes for further development could be taken. Neurath warned Hempel that the formal reconstruction as offered in Hempel's paper could not be used to represent historiographical inquiry, and that it introduced a norm from without, which, in Neurath's view, should never be the result of a formal reconstruction. Stevenson advised Hempel to bring his formal reconstruction more in line with language intuitions about laws. In his new American context, Hempel decided to bring his logical reconstructions closer to a philosophical practice of analysis by using intuitive examples of his analysandum as elements to which a reconstruction could be held accountable. Despite Neurath's many attempts, Hempel always maintained his logicizing attitude. By using language intuitions that were similar to those of John Hospers, Hempel and Oppenheim in 1948 took 'scientific explanation' as their analysandum, which they introduced as a central aim of science and as an answer to why-questions. Now, the re-alignment of the philosophical conception of science as the search for explanation could begin. Through the game of analysis and its institutional representation that ensued from the late 1950s onwards, the configuration of the search for the model of explanation was installed in philosophy of science.

The crucial intellectual problem that configures the intersection between the two above described configurations, is the question that intersects a large part of 20th century philosophy: what does it mean to investigate the logic of science? This question was already at the centre of Meyerson's review of Cassirer, and it also took up a central place at the intersection from which contemporary ideas on scientific explanation were born. Carnap's 1935 lecture at the Paris conference for the Unity of Science was an active attempt to bring scientific philosophers to a new consciousness about their task as logicians of science. Carnap's interpretation of *Wissenschaftslogik* intended to leave behind any question about the possibility or ultimate justification of scientific knowledge; logic was not to be used to shed philosophical light on epistemological or ontological questions (Friedman 2008, 394–95). Michael Friedman has described Carnap's program as a program with two sides:

On the positive side, as I have said, the Carnapian logician of science participates, together with the scientists themselves (especially applied mathematicians), in the development and clarification of formal inferential frameworks for articulating empirical theories and testing them by experimental methods. On the negative side, however, the Carnapian logician of science is also concerned with a systematic method for defusing persistent and unresolvable metaphysical controversies. (Friedman 2008, 395)

This dual aspect of the logic of science can also be found in Hempel's 1939 summary of Logical Empiricism for the Chicago Wind radio channel. On the one hand, Hempel identifies a critical, negative aspect of the logic of science: "in contradistinction to a view maintained by some philosophical schools, Logical Empiricism holds that there is no realm of specifically philosophical truths which are not tautologous, and which cannot be checked by observation or experiment, but which can be discovered by a particular kind of philosophical intuition or speculation".¹ This criticism of unscientific methods in philosophy also has a practical use, since unscientific reasoning in philosophy also "involves the danger that [its results] might be misused to give a pseudo-justification of principles which in fact do not admit of any scientific justification". Hempel, implicitly referring to Nazism, adds "And such misuse has happened".² The negative, critical goal also has a positive counterpart:

The critical analysis of unscientific methods in philosophy is, however, only part of the work done by Logical Empiricism; another, and probably more important, part consists in the development of what has been called a science of science, i.e. a study of the language of science, of the concepts used in the various branches of scientific research, of the methods by which hypotheses and theories are established and tested, and finally of the connexions between the various branches of science.³

The intellectual intersection of the 1942 paper offers a good indication that Hempel conceived his own work within the official boundaries of the scientific philosophy as Carnap had set them, but that at the same time Hempel's own conception of those boundaries was also open to change. The 1942 paper is set within an agenda of logical reconstruction: it aimed to show how historiographical claims could be represented as using universal hypotheses. The paper mainly served the negative, critical goal, since it showed how all kinds of philosophical terminology like *value-relation*, *understanding* or *tendency* were unnecessary in an articulation of historical knowledge. However, the offered logical reconstruction still had to represent the inferential framework that was implicitly articulated

¹ Wind Radio Talk, May 1939, CH 54-1 ASP.

² Ibid.

³ Ibid.

by empirical, historiographical theories. This could only succeed if the introduced, logical norm of the DN schema was representative of historiographical inquiry, which according to Neurath and most historiographically oriented philosophers after him, it was not.⁴ By 1948 Hempel had changed the positive aspect of the logic of science: the reconstruction of scientific language was not meant to clarify the inferential framework of empirical theories. The logical reconstruction was transformed into an analysis of scientific language, starting from intuitive examples. Arguably, Hempel had by then also abandoned the negative aspect of Carnap's logic of science: even though they were not the intuitions and speculations typical for German, interbellum philosophers, Hempel did use philosophical intuitions about the aim of science and its representation in scientific examples. As Miller indicated, the idea that language usage could arbitrate a logical analysis of science did not belong to Hempel's empiricist tradition: by using that norm Hempel was shifting the boundaries of his tradition. By 1965 Hempel described his work on explanation as Carnapian explication, but he was clearly absorbed by the search for a reflective equilibrium concerning the intuitions and examples that had been introduced by the philosophical criticisms of his model. In Hempel's hands the logic of science had returned to typical disputes about philosophical questions, and the logician of science certainly did not cooperate with the empirical scientists themselves, but instead directed his arguments towards his philosophical colleagues.⁵

While Hempel's work on explanation has often been identified as Carnapian explication (Reck 2013; Salmon 2000; Hardcastle 2002), it is a hard case to make. Carnap's conditions of similarity and fruitfulness work in tandem: the exactly formulated explicans can diverge from the vague explicatum in multiple applications to the extent that the explicans promises to be more fruitful than any other exactly formulated explicans that is similar to the explicatum (Carnap [1950] 1962, 6). And fruitfulness is defined by the systematizing power of a concept: "A scientific concept is the more fruitful the more it can be brought into connection with other concepts on the basis of observed facts; in other words, the more it can be used for the formulation of laws" (Carnap [1950] 1962, 6). Hempel, however, after 1948, never discussed how his explication of explanation could increase the systematizing power of scientific theories. The fruitfulness that Hempel attributed to his explication in 1965, was its capacity to align all kinds of intuitions and examples of scientific explanation.

⁴ Hempel's model is now widely taken to be *prima facie* ill-suited for historiography (Roth 1999, 249; Little 2012, chap. 3.1; McGill 1989).

⁵ Ronald Giere already hypothesized a similar shift from Carnap's notion of explication to a program of analysis, operating with language intuitions and executing a game of counterexamples (Giere 1996, 340–41). Giere refers to the influence of G.E. Moore and the later Wittgenstein. The above discussed impact of Charles Stevenson and John Hospers, who had both followed courses with Moore, shows that Giere's hypothesis can be historically verified.

His explication did not engender scientific progress as Carnap's ideal would have wanted, but it did engender philosophical progress, in the sense that most philosophers of science could afterwards argue about the benefits and problems of all kinds of models of explanation.⁶ This positive aspect of Hempel's explication is still used to motivate why philosophers of science should know Hempel's model of explanation: it serves as the perfect introduction to the debate, shows how the debate evolved, and also how one can argue about a meta-scientific concept like explanation in philosophy of science (Reck 2013, 318). Thus, one might say that Hempel reconceived what it means to give an explication: instead of driving scientific progress, it now drives philosophical progress.

If Hempel's reasoning process from the late 1940s onwards increasingly focused on philosophical issues, in contrast to the original, more radical ideals behind scientific philosophy, this does not imply that Hempel consciously diverged from it. Throughout his career, Hempel applied a scientific ethos which he inherited from two of his most important teachers, Hans Reichenbach and Rudolf Carnap. According to this ethos philosophy had to secure the status of a science by participating within a scientific culture (Richardson 2008, 90). This implied that philosophers had to collaborate on common issues by building on each other's results, and in close collaboration with the sciences. Instead of philosophical controversy, there would be something like scientific collaboration. Upon arrival in Istanbul in 1933 Reichenbach complained to Carnap about the lack of such scientific culture in his new environment.

Getting used to philosophy here will not be as easy [as getting used to the dirt and stench]. All students are "pure" philosophers, i.e. they learn philosophy at school, continue this at the university, and then go back to the schools to teach philosophy. They have no idea of working within unified science. I will have a lot of work before I will ever be able to do something with these people.⁷

At the university of Istanbul, philosophy was still conceived as a practice internal to itself, with particular questions from a philosophical tradition to which the great philosophers had given a set of particular, worthwhile answers that should be studied and passed on by philosophers. Reichenbach believed this type of philosophical culture was also still present at German universities. Two years before his complaint about the philosophical culture in

⁶ Michael Friedman also positions Hempel's work on theoretical terms in more traditional philosophical concerns in contrast to Carnap's program of Wissenschaftslogik (Friedman 2008, 395). I believe Hempel's reasoning about explanation also testifies to this divergence from Carnap's self-conception of his philosophical method.

⁷ "Mit der Philosophie hier wird es nicht so einfach sein. Die Studenten sind hier alle "reine" Philosophen, d.h. sie lernen in der Schule Philosophie, setzen das dann auf der Universität fort, und gehen dann wieder in die Schulen, um das Gelernte weiter zu geben. Von einzelwissenschaftlicher Arbeit haben sie keine Ahnung. Ich werde da noch allerhand Arbeit haben, bis ich mit den Leuten etwas machen kann." Reichenbach to Carnap, 29-10-1933, HR 13-41-23 ASP.

Istanbul, he lamented to Carnap about the philosophy of his colleagues at the University of Berlin:

I have attended the conference [on Hegel] a little, while my hairs stood upright all the time. Nicolai Hartmann made the fine discovery that the contradiction [Widerspruch] within sentences corresponds to an antagonism [Widerstreit] in real things, and consequently we should try to artfully construct models containing antagonism [Widerstreit] for systems of sentences containing contradictions [Widerspruch]. Hegel is now dead for 100 years, but these people are like the snake animals that grow two more heads and a philosophy chair, whenever one beheads them.⁸

Hartmann's metaphysical concerns could only beget more internal controversy among philosophers. Instead of burning heads like Heracles, Reichenbach decided to launch a petition to the Ministry of Education for more scientifically oriented philosophy chairs (which ironically ended in a controversy with his Viennese colleagues, see 2.4.1). Hempel also reported a similar judgement about Hartmann to Reichenbach in December 1933, when Hartmann was recommended as the second examiner for Hempel's doctoral examination by August Köhler, a gestalt-psychologist at Berlin who was the stand-in supervisor given Reichenbach's forced absence. Hempel reported to Reichenbach that Hartmann had shown "an icy restraint" towards this proposal from Köhler. Nonetheless, Hempel followed Hartmann's course on ethics, of which he reported the following:

His lectures are captivating and often elegant; otherwise, I have the ever increasing impression that contemporary philosophy has astonishingly little to do with objective science. [...] For now, in all earnest, I still entirely lack any feeling for when Hartmann considers the solution of a metaphysical question as "deep" or as "superficial", or for the point at which he finds "antinomies of knowledge" or "logical quarta mistakes" – as he calls them. I have no clue how I can pass the exam for him.⁹

⁸ "Wir haben uns einiges angehört, während sich unsere Haare bis zum Himmel sträubten. Nicolai Hartmann hat die schöne Entdeckung gemacht, dass dem Widerspruch zwischen Aussagen in den realen Dingen ein Widerstreit korrespondiert, und wir werden uns also bemühen, künftig für widerspruchsvolle Satzsysteme Modelle mit 'Widerstreit' zu konstruieren! Dabei ist der Hegel nun 100 Jahre tot, aber diese Leute sind wie die Schlangentiere, wenn man einen Kopf abhaut, wachsen zwei wieder und werden dann auf philosophische Ordinariate gesetzt." Reichenbach to Carnap, 21 October 1931, HR 13-41-47 ASP.

⁹ "Der Vortrag ist fesselnd und oft elegant; im übrigen aber verstärkt sich bei mir immer weiter den Eindruck, wie erstaunlich wenig die 'Philosophie' unserer Tage mit objektiver Wissenschaft zu tun hat. Enttäuscht war ich auch noch insofern, als selbst die Erwartung, hier den Niederschlag subtilerer psychohlogischer Analysen -etwa des Wertempfinden der Westeuropäer unserer Tage - zu finden, sich nicht erfüllte. Vorläufig fehlt mir, ganz im Ernst, auch noch fast jedes Gefühl dafür, wann Hartmann die Lösung etwa einer metaphysischen Frage als 'tief', wann er sie als 'oberflächlich' bezeichnen würde; an welchen Punkten er 'Antinomien der Erkenntnis'; und an welchen

Hempel described his problem with Hartmann's philosophy to Reichenbach in the way that Reichenbach or Carnap would have approved: Hempel had not yet acquired the right "feeling" for Hartmann's metaphysical questions. Scientific philosophy was supposed to rely on a more objective resolution of its problems than a certain feeling. Despite the initial icy restraint, Hartmann nonetheless agreed to Köhler's request and served as the second referent of Hempel's doctoral examination, which, against Hempel's expectations, went excellent. No feeling stood in the way of Hartmann to openly discuss the philosophical ponderings on logic and mathematics from the positivist side. Apparently, Hartmann also had a *scientific* ethos that allowed him to openly converse with Hempel, who reported the following about the examination to Reichenbach¹⁰:

I find it wonderful that Köhler and Hartmann through their approval have confirmed that philosophically useful offspring can be gotten even from the despicable positivist side.

For Hartmann I studied Leibniz for the first time in my life, and I revisited Hume, Kant and the history of modern philosophy. During the examination Hartmann limited himself to an expansive discussion of the foundations of logic and mathematics in so far as these are related to the antinomies, especially those of set theory.¹¹

Hartmann's examination showed Hempel that one could openly reason with metaphysically inclined philosophers. After his doctoral examination, Hempel moved to Brussels to work

er 'logische Quartanerfehler' - wie er das nennt - zu finden meint. Ich weiss vorerst garnicht, wie ich bei ihm durch die Prüfung kommen soll." Hempel to Reichenbach, 26 December 1933, HR 13-46-32 ASP.

¹⁰ In the autobiographical interview with Richard Nollan, Hempel also notes his surprise about Hartmann's openness: "Hartmann could have, I'm quite sure, failed me in this examination if he had wanted to, but we conducted a very amiable conversation about some broad philosophical questions and so everything went perfectly well in the examination and I must say I appreciate this broad spirit that Hartmann showed and that not all his colleagues would have shown" (Nollan and Hempel 2000, 8).

¹¹ "Das finde ich auch insofern schön, als Köhler und Hartmann damit schliesslich bestätigt haben, dass auch von der verruchten positivistischen Seite her ganz brauchbarer philosophischer Nachwuchs herangebildet werden kann. ... Hartmann zeigt sich bei der zweiten und dritten Vorbesprechung schon wesentlich freundlicher als bei der ersten, in der er eine ganz eisige Reserver zur Schau getragen hatte. [...]

Und schliesslich Hartmann, für den ich - zum erstenmal in meinem Leben - eingehend Leibniz studiert und Hume, Kant sowie die Geschichte der neueren Philosophie wiederholt hatte, beschränkte sich auf eine ausführliche Besprechung der Grundlagenfragen der Logik und Mathematik, soweit sie mit den Antinomien, besonders der Mengenlehre, zusammenhängen, und schloss dann einige kleine Exkurse an." Hempel to Reichenbach, 19 March 1934, HR 13-46-30 ASP. Hempel also recounts Hartmann's interesting response to Hempel's answers: "Unheimlich war mir lediglich, dass Hartmann es als platt empfand, die Antinomien durch eine Sprache mit geeigneter Syntax zu vermeiden; er fragte mich einmal ganz betroffen: Sind sie wirklich der Ansicht, dass man sich bemühen solle, die Antinomien aus der Mathematik zu eliminieren? Er selbst hat die Idee, dass sich im Auftreten der Antinomien eine Antinomialität der Welt offenbaren könne, und dass es daher ein blosses rednerische Wegeskamottieren von echten Schwierigkeiten darstelle, wenn man die Sprach durch Typentheorie od. dgl. von Antinomien befreie."

under the supervision of Paul Oppenheim. His most direct contacts during those years were the scientifically oriented philosophers that operated in the logical empiricist network: Carnap, Reichenbach, Oppenheim, Neurath, Grelling, etc. Hempel attempted to proliferate the ethos that Reichenbach and Carnap advocated: collaborative work on common issues, instead of controversies. Differences of opinion were to be commonly debated. His expansive correspondence with Neurath during those years is the testimony of this ethos, which Neurath also made explicit in his letters:

The republic of scholars is an important institution, and correspondence between scholars is also an important institution today, as it used to be in the past, which is insufficiently replaced by publications. It gives me satisfaction to see that the contact between members of our movement is maintained through this institution of correspondence.¹²

In their mutual correspondence Hempel always continued to discuss Neurath's points about the necessity to investigate historiography, or to use a pragmatic-historicizing attitude, even though Hempel never applied Neurath's viewpoints to his own work. Upon arrival in the United States, Hempel immediately came into contact with a wide range of American philosophers, to whom he applied the same ethos of a scientific philosopher. He listened to their ideas and incorporated their criticisms in his arguments: his interaction with Stevenson is a prime example. In the United States Hempel always belonged to a philosophy department, published mainly in philosophy journals and proliferated his ideas for philosophers. By the end of the 1960s there were philosophy of science centres, in Pittsburgh, Minneapolis and Indiana, that trained philosophers of science, partly by studying Hempel's answer to the by-then already standard question "what is scientific explanation". Hempel's ethos towards his colleagues had remained the same – open collaboration on common issues¹³ – but the culture in which Hempel operated in the United States, was in a way similar to the culture that Reichenbach had scorned in Istanbul or Berlin. Hempel operated in a discipline, philosophy of science, that answers a set of problems with which a disciplined philosopher can engage, e.g. finding a model of scientific explanation. In answering such a problem the philosopher goes back to the answers that were already given in the discipline of philosophy: Aristotle, Hempel, Salmon, etc. Philosophy of science became school philosophy again.¹⁴ In the case of Paul Oskar

¹² Die Gelehrtenrepublik ist eine wichtige Einrichtung und der Briefwechsel, heurte, wie ehemals eine wichtige Einrichtung, die durch Publikationen ungenügend ersetzt wird. Es freut mich, wenn der Kontakt zwischen den Mitgliedern unserer Bewegung erhalten bleibt. Neurath to Hempel, 8 February 1935, Nr. 244 VCA.

¹³ For testimony of Hempel's continued, open "scientific" ethos throughout his career, see (Fetzer 2000, xxvi; Friedman 2000a, 61; Salmon 2000, 320).

¹⁴ Ronald Giere also hypothesized that the institutional position of Feigl and Hempel was crucial to understand how philosophy of science is distinguished from the European project of Wissenschaftliche Philosophie (Giere 1996, 339).

Kristeller a similar process took place: instead of participating in a culture where he could integrate history and philosophy, he became the exponent of history of Renaissance philosophy and was unable to impact analytic philosophy in any way. Similarly, Hempel was never able to integrate the logical analysis of science with the natural sciences themselves, but instead successfully proliferated a philosophy of science.

Contrary to what could be argued for the logical empiricist practical and collaborative program of the Unity of Science in general (Reisch 2005), the migration to the United States did not influence the political aspect of Hempel's reasoning about science. Hempel was active within the Unity of Science network between 1934 and 1939 and aided Neurath during those years in the organisation of the conferences. From his letters with Neurath or his 1939 description of logical empiricism at the Wind Radio Channel, one can see that Hempel conceived of logical empiricism as a collective and collaborative program, but he rarely emphasized its practical and political dimension. Neither in Europe nor in the United States did Hempel attempt to extend the effect of his work beyond the boundaries of professional philosophy. Much to Neurath's dismay, Hempel always kept on operating with logic as his preferred tool for abstract work on scientific language. This apolitical aspect meant that his work was able to thrive in the partitioned and specialized field of philosophy of science as it arose in American departments during the 1950s (Reisch 2005, 376).

Nikolay Milkov has recently argued that Hempel, throughout his career, remained on the Berlin side of the logical empiricist divide, working on Reichenbach's program for an internal philosophy of science "that analyses the facts of concrete scientific practice" (Milkov 2013, 28). I find it unilluminating to situate Hempel under the influence of either Neurath, Carnap or Reichenbach exclusively.¹⁵ Hempel engaged with the ideas of all figures that he met in his philosophical life, both in Europe and the United States – this was the philosophical ethos for a scientific philosopher that Hempel had acquired through his contact with Reichenbach, Carnap and Neurath early in his career. Milkov's thesis is meant as a response to Michael Friedman's comparison between Hempel's late ideas about provisos in science and his earlier contact with Neurath's naturalism in the 1930s. The Hempel-Neurath correspondence, as I discuss it in 2.4.2, has proven that Neurath regularly attempted to bring Hempel closer to his pragmatic-historicizing approach, contrasting it to Carnap's logic of science. Consequently, Friedman's claim that "in Hempel's own eyes,

¹⁵ Milkov also states that Grelling, Kurt Lewin and Walter Dubislav, as Berlin philosophers, continued to influence Hempel's thinking over the course of his career, but Milkov does not specify how this is manifest in Hempel's work (Milkov 2013, 298). Apparently, this should prove that Hempel was always working within the Berlin-side of logical empiricism. Throughout the rest of his text, Milkov primarily discusses the relation between various works of logical empiricist philosophers, both from Berlin and Vienna. This shows that all these philosophers, including Hempel, aimed to collaborate on common problems, as the ethos of scientific philosophers would have it. Consequently, it is pointless to argue that Hempel was first and foremost a *Berlin* logical empiricist: at various points in his career he shifts his method and the topic of his attention according to the influence of a wide range of philosophers.

Kuhn's *Structure* could be seen as applying a fundamentally Neurathian perspective to the detailed study of science" (Friedman 2000a, 60), can certainly be historically grounded in Hempel's extensive contact with Neurath during the second half of the 1930s. However, this does not imply that Hempel *in the end* turned towards Neurath's conception of philosophy of science, or abandoned Reichenbach's. The boundaries of Hempel's philosophical discourse on science were in a permanent flux, in the same way as the boundaries of contemporary philosophical discourses on science are still drifting. Within this flux, there is also contingently formed consistency: roughly between 1900 and 1945 philosophers reflecting on science also had to reflect on the place of history vis-à-vis the sciences, which cannot be said today. Ever since the 1960s the concept 'scientific explanation' has played and continues to play an important function in the philosophical reasoning about science. This latter configuration came into being in part because of the fluctuating boundaries in Hempel's practice as a logician of science: from rational reconstruction, over logical analysis to the search for a reflective equilibrium. And these fluctuations in turn reflect Hempel's migration from the European context of scientific philosophy to the United States, where Hempel came to operate within philosophy departments exclusively and interacted with a great variety of philosophers, also those who used ordinary language analysis as a philosophical method. Hempel's migration to the United States influenced both the boundaries of Hempel's approach to the logic of science and shaped the effect that Hempel's analyses were to have within the rise of partitioned and specialized subdisciplines of philosophy in academia.

The consistency of 'scientific explanation' resulted from a varied set of contingent matters during Hempel's migration. Hempel started a conversation with Neurath on Rickert's philosophy in 1935 which gave him the intellectual background to contribute to the New York debate on history. Hempel was asked to contribute to the debate on historiography for the New York circle in 1941. Hempel came into contact with Hoser's analysis of explanation, Stevenson's reasoning over laws and Nagel's introduction to Aristotelian philosophy of science, which enabled him to introduce intuitions about explanation as an autonomous aim of science. There was no adherent to the Mach-Duhem tradition in American philosophy in the 1950s, except Philip Frank, who was not widely read by philosophers. Hempel's papers on explanation were canonized early on in the 1960s, within a novel publishing culture of anthologies and introductory handbooks that accompanied the early formation of subdisciplines in philosophy. Hempel took up central positions in Yale and Princeton where he instructed a large number of the new generation in anglophone philosophy. These are all contingencies that eventually formed the consistency of 'scientific explanation' in the discourse of philosophy of science.

My historical narrative shows the contingent formation of philosophical reasoning about scientific explanation. This narrative does not make the contemporary consensus about scientific explanation as an important problem for the philosophy of science illegitimate. As I stated in chapter one, scientific explanation as a meta-concept is productive.

Philosophers can, through their training, now identify all kinds of examples and scientific texts as explanations, e.g. a scheme from a scientific handbook depicting the depolarization of a nerve cell can now be introduced as a case of scientific explanation (Machamer, Darden, and Craver 2000, 17). Philosophers now also have all kinds of intuitions about it and they can discuss episodes in the history of science through the concept (Wallace 1972; McMullin 1992). Philosophers of science can continue to reason about explanations as a central aim of science. The real question is why a philosopher should aim to do that? Why identify a text, an example or a diagram as an explanation, knowing that such identification brings with it the problem of what a scientific explanation is? Why not discuss the text, the example or the diagram in different terms? What justifies explanation as a meta-concept to articulate science?

One possible reply might be that scientists themselves talk about explanations in their work, or aim to explain phenomena in their practice, and that this is a brute aspect of scientific practice or reasoning that cannot be removed from a philosophical articulation of scientific knowledge. This line of response presumes that some (epistemo)logical aspects of scientific knowledge in general, such as the aim of explanation, can be made transparent to us and can be used as givens in the philosophical articulation of this knowledge. Important philosophers of science, like Duhem, Mach, Carnap, Reichenbach or Neurath, were either blinded from this transparency for some reason, or there has never been such a transparency. Given the historical emergence of the consensus about scientific explanation in the second half of the twentieth century, I would argue that there has never been such a transparency. There never was an argument or a scientific breakthrough that showed why Duhem or Neurath were blinded from the light; in the first three decades after Hempel and Oppenheim's 1948 paper there never was an actual debate on the usefulness of 'scientific explanation' as a meta-concept to discuss scientific knowledge; there was only an assumption that was introduced at a specific point in time, that was subsequently proliferated through the reification of analysis, and that eventually justified itself by referring back to the point of introduction. As Meyerson warned in his review of Cassirer, no one, not even scientists, can see themselves thinking. Whatever the logic of science is, it is never wholly transparent to us. Consequently, one cannot justify the use of scientific explanation as a meta-concept in the philosophy of science by referring to intuitions about science or brute scientific practice, as "what scientists are simply doing". For what scientific practice or scientific reasoning in practice is, is exactly what the logic of science has to discern; one cannot turn this the other way around: an instance of practice cannot show what the logic of science is made of. Similar to Meyerson, Duhem warned that it is futile to point to Descartes, Huyghens or Fresnel's aspiration to explain [*expliquer*] phenomena. These various attempts at explanation blind the individual theorists from what Duhem through his history of science argued was the real logic of science, namely natural classification. Though invisible to the individual theorists, their explanations were like waves striking a beach.

Whoever casts a brief glance at the waves striking a beach does not see the tide mount; he sees a wave rise, run, uncurl itself, and cover a narrow strip of sand, then withdraw by leaving dry the terrain which it had seemed to conquer; a new wave follows, sometimes going a little farther than the preceding one, but also sometimes not even reaching the sea shell made wet by the former wave. But under this superficial to-and-fro motion, another movement is produced, deeper, slower, imperceptible to the casual observer; it is a progressive movement continuing steadily in the same direction and by virtue of it the sea constantly rises. The going and coming of the waves is the faithful image of those attempts at explanation which arise only to be crumbled, which advance only to retreat; underneath there continues the slow and constant progress whose flow steadily conquers new lands, and guarantees to physical doctrines the continuity of a tradition. (Duhem [1906] 1991, 38–39)

Of course, one does not have to agree with Duhem about his views on the illegitimate nature of explanation as a concept to discuss the logic of science. Meyerson certainly did not, but one has to acknowledge that one cannot justify explanation as a valid meta-scientific concept because of the purported transparency of scientific knowledge or scientific practice. A passage where Darwin talks about the explanation of the facts by his theory of natural selection, does not legitimate explanation as a meta-scientific concept, just as Newton's famous hypotheses-non-fingo paragraph does not transform explanation into an illegitimate concept.¹⁶ One can quote as many varied uses of the verb “explain” by scientists, this still does not legitimate the concept of explanation in the logic of science. In articulating what it means to produce or attain scientific knowledge, it is pointless simply to refer to what scientist believe their method to be. The use of scientific explanation as a meta-scientific concept should not only be accountable to what scientists say they believe or say they are aiming for with their theories. Wesley Salmon legitimated the turn towards explanation in the second half of the twentieth century this way:

Steven Weinberg's view on this matter [that science aims to explain] is shared by large numbers of philosophers and scientists. In fact, I cannot think of any contemporary philosopher of science who denies the possibility of scientific explanation. While I cannot claim any comprehensive knowledge of the attitudes of

¹⁶ That specific paragraph from Newton is a classic in the debate over the legitimacy of explanation as a meta-scientific concept. It plays a central role in Duhem's, Cassirer's and Meyerson's views (Meyerson 1911, 124). In more recent times, Bas van Fraassen also used the passage to argue that explanation is not a virtue of scientific theories that is preferred over all others (Van Fraassen 1980, 94–95). Unlike Van Fraassen's use of the passage, I believe, similarly to Duhem, Cassirer or Meyerson, that only a thorough historical contextualization of the passage could enable one to discuss its bearing on the validity of scientific explanation as a meta-concept.

contemporary scientists on this subject, I do feel confident that Weinberg speaks for a substantial group.¹⁷ (Salmon 2000, 313)

I think this is irrelevant: a set of beliefs of scientists can never legitimate the validity of a meta-concept. Then to what is our use of scientific explanation as a meta-scientific concept accountable? As an answer to this question, Ernst Nagel's justification of the concept proves illuminating:

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, 4)

Nagel defends a particular philosophical norm that is valid for a particular form of knowledge, science. Science, unlike "ordinary experience", should strive for "an explanation of why the facts are as alleged".¹⁸ Not all philosophers have upheld this philosophical norm of scientific knowledge: Duhem, Mach, Carnap, Frank, von Mises did not articulate scientific knowledge in terms of the explanatory principles that distinguish it from ordinary experience. Thus, the use of scientific explanation as a meta-scientific concept is primarily accountable to the philosophical reflection on what it means to produce or attain scientific knowledge. And there, at least, the norm of scientific knowledge as explanatory knowledge is not self-evident. Aristotle's distinction relied on the idea that scientific knowing should be an expression of the insight into the essential being of the subject of knowledge. If one abandons Aristotle's epistemological and metaphysical project, the central philosophical question becomes whether and why we should endorse the norm of explanation as the signature mark of scientific knowledge. The use of scientific explanation as a valid meta-scientific concept cannot remain neutral to these questions, which stood at the centre of the debate between Cassirer and Meyerson who each argued in opposite directions based on their history of modern science. Within the Hempelian

¹⁷ Earlier in his career Salmon also expressed this in the following way: "It is now *fashionable* to say that science aims not merely at describing the world - it also provides understanding, comprehension, and enlightenment. Science presumably accomplishes such high-sounding goals by supplying scientific explanations" (Salmon 1978, 684).

¹⁸ Not coincidentally, Bas van Fraassen also discusses this passage in Nagel's work, since Van Fraassen also aims to argue that explanation is not the primary aim of scientific theories (Van Fraassen 1980, 92). Van Fraassen reduces the aim of explanation to a theory's ability to provide certain bits of information in response to contextually defined queries (Van Fraassen 1980, 126). In that sense, Van Fraassen remains on the descriptivist side of Duhem, Mach, Carnap and von Mises. According to Van Fraassen, science contributes nothing to explanation over and above the descriptive and informative content of the scientific theory: "a success of explanation is a success of adequate and informative description" (Van Fraassen 1980, 157). Van Fraassen does not deny that one can give explanations, but, unlike Nagel's view and the views of most contemporary philosophers of science, Van Fraassen does not consider explanation to be a norm to which scientific theories are epistemically accountable.

tradition there has been little reflection on these questions: most of the time scientific explanation has been taken as a valid meta-scientific concept because of the supposed brute fact of what science is. Through this dissertation I have shown that this cannot be maintained: if one wants to have a theory or a model of scientific explanation as an aim of science, one also needs a philosophical reason to uphold the norm that scientific knowledge should offer explanatory principles. By articulating such a philosophical reason, other epistemological and metaphysical commitments are often brought to light. E.g. Wesley Salmon, if he is not pointing to the fashion rage over scientific explanation in contemporary philosophy, ends with a defence of a realist interpretation of scientific knowledge; “science satisfies a deep desire to understand the nature of our universe and how it came to be as it is” (Salmon 2000, 317). Discussing science as an explanatory activity or practice is never epistemologically or metaphysically neutral. Consequently, anyone who discusses science through the meta-concept of explanation, should be aware of the implicit commitments that necessarily come with it. Naturally, it should also be possible to uphold an abolitionist position about the concept of scientific explanation along Neurathian lines: in articulating what scientific knowledge is, one can choose to deny explanation any role and translate any reference to the term into other epistemological concerns, e.g. over prediction, evidence, confirmation, etc.

By writing a history of the modern concept of explanation, I have argued that scientific explanation cannot be taken for granted as an undeniable aspect in the philosophical articulation of scientific knowledge. I have shown that scientific explanation emerged as a topic for philosophy of science between 1948 and 1970 without a proper debate over its validity as a meta-scientific concept. It emerged after a period of migration, at the intersection of American interbellum philosophy with German historical and scientific philosophy. After its emergence, the topic aligned all kinds of intuitions about and examples of science, and it partly structured how a philosopher reads scientific texts. Because the topic engendered much debate over its proper analysis, this alignment became increasingly standard, to the extent that it was unnecessary to philosophically justify the use of scientific explanation as a meta-concept. This history of the modern concept of scientific explanation aims to have two effects. On the one hand, it aims to argue that the continued use of scientific explanation as a meta-concept should be constantly held accountable to the epistemological or metaphysical commitments that necessarily come with it. One cannot legitimate the use of scientific explanation by referring to the supposed, simple *fact* that science explains the world. On the other hand, it aims to proliferate the consciousness that the concepts with which philosophers reason about science always have a historical, contingent point of emergence. Consequently, philosophers who aim to be responsible for their own forms of thinking and the concepts that operate in it, should also profess a critical, historicizing attitude towards these forms of thinking.

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Summary

In this dissertation I present a genealogy of scientific explanation, which in contemporary philosophy of science is almost universally taken to be a central aim of science. Today, philosophers of science constantly reason with and about this aim of science. Through my genealogy I show that this particular way to reason about science should not be taken for granted. It does not constitute a transparent aspect of science that is given to us. Instead it contingently emerged within a period of intellectual migration. My genealogy consists of three parts.

First, at the beginning of the twentieth century, there were two intellectual problems at stake in German academic philosophy concerning history: to present an epistemology of historiography and related fields as sciences, and to use a historical perspective on scientific knowledge itself (Chapter 2). These were both challenges that aligned philosophical concerns across various philosophical movements. Logical empiricist philosophers, although most of them were directly concerned with developments in the natural sciences and mathematics, did not escape this theoretical configuration of the German philosophical discourse. Schlick, Carnap, Neurath, Zilsel and von Mises, in different ways, actively took up positions against Windelband's and Rickert's plea to expand the logic or epistemology of science. However, such a negative position also implied a positive project to incorporate history and related fields in what they took to be the unity of science. This positive challenge engendered controversy among the logical empiricists. Also, the second stake, what it meant to have a historical perspective on science, caused disunity in the logical empiricist network. Zilsel and Neurath believed that a historical perspective on science should be understood as an intellectual method or engagement within scientific philosophy, while Schlick, Reichenbach and Carnap conceived it as a viewpoint external to scientific philosophy proper.

Second, at the end of the 1960s anglophone philosophy of science and philosophy of history were to a considerable extent centred around the analysis of scientific explanation (Chapter 4). The question what scientific or historical explanation is aligned an entire set of intuitive examples, scientific and historiographical texts, and episodes from the history of science as elements in the philosophical reasoning process about explanation. Hempel and

Oppenheim's paper "Studies in the Logic of Explanation" stood at the centre of this intellectual problem in anglophone philosophy: participating in the debate on explanation was also always participating in the reflection on the cases, examples and model that Hempel and Oppenheim had brought together in their paper, or on the cases, examples and models that had been given in response to their paper. As a result, 'scientific explanation' as an independent aim of science and an answer to why-questions came to form a crucial and undeniable aspect of the philosophical analysis of science in the anglophone world.

The development of Hempel's ideas on explanation stands at the intersection of these two configurations of philosophical discourse (Chapter 3). Hempel's first use of the term 'explanation' in his 1942 paper "The Function of General Laws in History" still occurred within the older, German configuration: by reconstructing historiographical text within the bounds of a formalized scheme that represented an interpretation of science as the search for laws, Hempel showed how a view on historiography was possible that diametrically opposed Rickert's view. In that sense, Hempel participated in the German debate and took up a position that was similar to Carnap's or Neurath's: there is no need to introduce a distinct epistemology or logic of historiography. Hempel's formal reconstruction of historiographical texts was, however, open to various interpretations and criticisms. Neurath warned Hempel that the formal reconstruction as offered in Hempel's paper could not be used to represent historiographical inquiry, and that it introduced a norm from without, which, in Neurath's view, should never be the result of a formal reconstruction. Stevenson, however, advised Hempel to bring his formal reconstruction more in line with language intuitions about laws. In his new American context, Hempel decided to bring his logical reconstructions closer to a philosophical practice of analysis by using intuitive examples of his analysandum as elements to which a reconstruction could be held accountable. Despite Neurath's many attempts, Hempel always maintained his logicizing attitude. By using language intuitions that were similar to those of John Hospers, Hempel and Oppenheim in 1948 took 'scientific explanation' as their analysandum, which they introduced as a central aim of science and as an answer to why-questions. Now, the recalibration of philosophy of science as the search for a model of explanation could begin. Through the game of analysis and its institutional representation that ensued from the late 1950s onwards, the configuration of the search for the model of explanation was installed in philosophy of science.

By writing a history of the modern concept of explanation, I have argued that scientific explanation cannot be taken for granted as an undeniable aspect in the philosophical articulation of scientific knowledge. I have shown that scientific explanation emerged as a topic for philosophy of science between 1948 and 1970 without a proper debate over its validity as a meta-scientific concept. It emerged after a period of migration, at the intersection of American interbellum philosophy with German historical and scientific philosophy. After its emergence, the topic aligned all kinds of intuitions, examples of science, and it partly structured how a philosopher reads scientific texts. Because the topic

engendered much debate over its proper analysis, this alignment became increasingly standard, to the extent that it was unnecessary to philosophically justify the use of scientific explanation as a meta-concept. This history of the modern concept of scientific explanation aims to have two effects. On the one hand, it aims to argue that the continued use of scientific explanation as a meta-concept should be constantly held accountable to the epistemological or metaphysical commitments that necessarily come with it. One cannot legitimate the use of scientific explanation by referring to the supposed, simple fact that science explains the world. On the other hand, it aims to proliferate the consciousness that the concepts with which philosophers reason about science always have a historical, contingent point of emergence. Consequently, philosophers who aim to be responsible for their own forms of thinking and the concepts that operate in it, should also profess a critical, historicizing attitude towards these forms of thinking.

Samenvatting

In dit doctoraat presenteer ik een genealogie van wetenschappelijke verklaring, wat in contemporaine wetenschapsfilosofie bijna unaniem beschouwd wordt als een centrale doelstelling van wetenschappelijk onderzoek. Wetenschapsfilosofen redeneren heden ten dage voortdurend met en over wetenschappelijk verklaring als doel van wetenschap. Aan de hand van mijn genealogie toon ik aan hoe deze specifieke manier om over wetenschap na te denken geenszins als vanzelfsprekend beschouwd mag worden. Deze manier van redeneren is geen transparant gegeven, maar kwam op contingente wijze voort uit een periode van intellectuele migratie. Mijn genealogie bestaat uit drie delen.

Het eerste deel beschrijft hoe twee intellectuele problemen omtrent historiografie aan het begin van de twintigste eeuw op het spel stonden in Duitse academische filosofie: enerzijds het presenteren van een epistemologie van de historiografie en aanverwante domeinen van de wetenschap, en anderzijds het gebruiken van een historisch perspectief op wetenschappelijke kennis zelf (Hoofdstuk 2). Dit waren twee intellectuele uitdagingen die de filosofische aandacht van verschillende filosofische stromingen samenbrachten. Ook filosofen behorende tot de logisch empiristische stroming in de filosofie ontsnapten niet aan deze theoretische configuratie van het Duitse filosofische discours, ook al ging hun initiële belangstelling uit naar ontwikkelingen in de natuurwetenschappen en de wiskunde. Schlick, Carnap, Neurath, Zilsel en von Mises namen, op verschillende manieren, actief een positie in tegen de theorieën van Windelband en Rickert, die beiden pleitten voor een uitbreiding van de logica en de epistemologie. Deze kritische oppositie vanuit het logisch empiristische kamp bracht ook een positieve uitdaging met zich mee: deze filosofen moesten nu ook historiografie en aanverwante domeinen een plaats geven binnen de eenheid van de wetenschap. Deze uitdaging ontlokte controverse tussen alle actoren in het logisch empiristische netwerk. Ook de tweede uitdaging, omtrent een historisch perspectief op wetenschappelijk kennis, veroorzaakte onenigheid. Zilsel en Neurath geloofden dat een historisch perspectief op wetenschap deel uitmaakte van de intellectuele methode van de wetenschappelijk filosofie. Schlick, Reichenbach en Carnap ontkenden dit, en plaatsden een historisch perspectief op wetenschap buiten de wetenschappelijke filosofie.

Het tweede deel van de genealogie beschrijft hoe aan het einde van de jaren '60 de analyse van wetenschappelijke verklaring een centrale bekommernis was in de domeinen van de Engelstalige wetenschapsfilosofie en geschiedfilosofie (Hoofdstuk 4). De vraag wat wetenschappelijk of historische verklaring is, bracht een hele verzameling samen van intuïtieve voorbeelden, wetenschappelijk en historiografische teksten, en allerlei episodes uit de geschiedenis van de wetenschappen. Al deze zaken werden nu elementen in het filosofische redeneringsproces omtrent verklaring. De paper "Studies in the Logic of Explanation" van Hempel en Oppenheim nam een centrale rol in binnen dit intellectuele probleem in Engelstalige filosofie. Deelnemen aan het debat over verklaring betekende op dat moment deelnemen aan de reflectie over het model, de casussen en de voorbeelden die Hempel en Oppenheim hadden samengebracht in hun paper, of op z'n minst reflecteren over de modellen, casussen en voorbeelden die in de reacties op Hempel en Oppenheim waren gebruikt. Hierdoor werd 'wetenschappelijk verklaring' een cruciaal en onmiskenbaar aspect van de filosofische analyse van wetenschap in de Engelstalige wereld.

De ontwikkeling van Hempels ideeën over verklaring staat op de intersectie tussen deze twee configuraties van het filosofische discours (Hoofdstuk 3). Hempels eerste gebruik van de term 'verklaring' in zijn paper "The Function of General Laws in History" (1942) vond nog steeds plaats binnenin de oude, Duitse configuratie. Hempel reconstrueerde historische teksten binnen de grenzen van een geformaliseerd schema dat de interpretatie van wetenschap als de zoektocht naar wetten voorstelde. Hierdoor toonde Hempel hoe een beeld op historiografie mogelijk was dat diametraal stond tegenover Rickerts visie op historiografie. Hempel participeerde dus nog in het Duitse debat, en nam een positie op die gelijkaardig was aan die van Carnap of Neurath: de introductie van een specifieke epistemologie of logica van de historiografie is, binnen deze positie, onnodig. Hempels formele reconstructie van historiografische teksten was echter vatbaar voor verschillende interpretaties en kritieken. Neurath waarschuwde Hempel dat zijn formele reconstructie niet gebruikt kon worden voor de representatie van historiografisch onderzoek. Bovendien introduceerde de reconstructie een norm die buiten de historiografische praktijk lag, en zo'n introductie kon volgens Neurath nooit de bedoeling zijn van een formele reconstructie. Stevenson daarentegen adviseerde Hempel om zijn formele reconstructie in overeenstemming te brengen met taalintuïties over wetten. In zijn nieuwe Amerikaanse context besloot Hempel om zijn logische reconstructie te laten aansluiten bij een filosofische praktijk van analyse. Bij gevolg gebruikte hij intuïtieve voorbeelden van zijn analysandum als elementen waaraan een reconstructie moest voldoen. Ondanks Neuraths verwoede pogingen bleef Hempel zijn logicistische attitude aanhouden. Door gebruik te maken van taalintuïties die gelijkaardig waren aan die van John Hospers, kozen Hempel en Oppenheim op die manier 'wetenschappelijke verklaring' als hun analysandum, en introduceerden zij dit concept als een centrale doelstelling van wetenschap en als een antwoord op een waarom-vraag. Vanaf dat moment kon de recalibratie van wetenschapsfilosofie als de zoektocht naar een model van verklaring beginnen. Via het spel

van analyse en haar institutionele representatie vanaf de late jaren '50 werd de configuratie van wetenschapsfilosofie als de zoektocht naar het model van verklaring geïnstalleerd.

Door het schrijven van deze geschiedenis van het moderne concept van verklaring, heb ik beargumenteerd dat wetenschappelijke verklaring niet begrepen kan worden als een onmiskenbaar aspect in de filosofische articulatie van wetenschappelijke kennis. Ik heb getoond dat wetenschappelijke verklaring tot stand kwam als een onderwerp voor wetenschapsfilosofie tussen 1948 en 1970 zonder een debat over de geldigheid van verklaring als meta-wetenschappelijk concept. Het kwam tot stand na een periode van migratie, op de intersectie van Amerikaanse interbellum filosofie en Duitse historische en wetenschappelijke filosofie. Nadat het tot stand was gekomen, bracht het onderwerp allerlei intuïties en voorbeelden van wetenschap samen en structureerde het gedeeltelijk hoe een filosoof een wetenschappelijke tekst leest. Omdat het onderwerp zoveel controverses teweeg bracht omtrent de geschikte analyse ervan, werd het onderwerp standaard, tot in die mate zelfs dat het onnodig was om een filosofische motivatie te geven voor het gebruik van wetenschappelijke verklaring als meta-concept. Deze geschiedenis van het moderne concept van wetenschappelijke verklaring heeft twee doelstellingen. Aan de ene kant wil deze geschiedenis aantonen dat het huidige gebruik van wetenschappelijke verklaring als meta-concept voortdurend verantwoordelijk gehouden moeten worden aan de epistemologische en metafysische vooronderstellingen die ermee samen gaan. Men kan het gebruik van wetenschappelijke verklaring niet legitimeren door te refereren naar het vooronderstelde, simpele feit dat wetenschap nu eenmaal de wereld verklaart. Aan de andere kant, wil de geschiedenis het bewustzijn tot stand brengen dat de concepten waarmee filosofen over wetenschap redeneren altijd een historisch en contingent punt van oorsprong hebben. Daarom moeten filosofen, die verantwoordelijk willen zijn voor hun eigen vormen van denken, altijd een kritische, historische attitude aannemen tegenover hun eigen vormen van denken.

