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The place of historiography in the network of logical empiricism

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ABSTRACT

In this paper I investigate how intellectual problems concerning an epistemology of history and a historical view of knowledge played a role in the network of logical empiricist philosophers between 1930 and 1945. Specifically, I focus on the practical efforts of Hans Reichenbach and Otto Neurath to incorporate these intellectual stakes concerning history. I argue that Reichenbach was mainly concerned with creating more institutional space for scientific philosophy. Consequently, he was interested in determining his relation to historically oriented philosophy on the practical level only. Otto Neurath, I claim, was interested in promoting an intellectual incorporation of an epistemology of history and a historical view of knowledge into the Unity of Science movement. His attempts, however, largely failed. I conclude that the intellectual stakes concerning history did have an effect within the network of logical empiricist philosophers, but that, by 1945 these stakes were entirely dissolved. The displacement of the network to the United States removed Reichenbach's practical problems, while Neurath was unable to persuade enough actors before his death.

KEYWORDS

Logical empiricism;
historiography; Otto Neurath;
Hans Reichenbach; Carl
Hempel

At the beginning of the twentieth century, German philosophy was faced with the task of incorporating the newly found historical disciplines of the nineteenth century into philosophy. At least, this was the conclusion of Wilhelm Windelband's 1904 reflection on the state of philosophy one hundred years after Kant's death.¹ Certainly not all philosophers agreed with Windelband's specific description of this problem, but many German philosophers understood that there was something at stake for philosophy surrounding the newly found historical perspective. It is my goal to investigate how these intellectual issues played out within the Berlin–Vienna network of logical empiricist philosophers between 1930 and 1945. Even though these philosophers rarely published on history, they did write about it to each other. I investigate this correspondence and show that intellectual problems concerning historical knowledge played an important role in conflicts among the actors in the network. This proves that, up until 1945, it was still an undecided matter whether an epistemology of history and a historical perspective on knowledge would be incorporated in logical empiricist philosophy. By 1945, however, the death of Edgar Zilsel and Otto Neurath had removed the most active, internal defenders of a

historical perspective within the German-speaking branch of logical empiricist philosophy, while the U.S. representatives were not particularly concerned with the German issues concerning the relation between history and philosophy. Consequently, when the American and the remaining German-speaking logical empiricists initiated philosophy of science as a discipline in U.S. departments, neither historiography nor a historical perspective of knowledge were considered as intellectual problems for philosophy of science.

In order to make the claim about internal conflicts over history in logical empiricism, it is first necessary to define what was at issue surrounding historiography in Germany at the beginning of the twentieth century. The issue had two aspects. First, there is the almost universally accepted idea at the time that historiography as a science needs to be accounted for in epistemology. This idea is articulated very well in Windelband's 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".² This novel task for epistemology was already an important problem of nineteenth-century Germany philosophy and became a central epistemological question for many German philosophers around the turn of the century.³ A second related, but not identical, aspect of the issue is the philosophical task of applying history to the analysis of knowledge itself. For Windelband, it was of central importance to ground historical understanding in the history of human thinking itself.⁴ Wilhelm Dilthey also emphasized that history had its own value for philosophy: "The extent of the historical part [of my investigations] emerged not just from the practical need of an introduction, but also from my conviction that historical self-reflection has its value besides epistemological self-reflection".⁵ Presenting a history of knowledge, thus, becomes part of a philosophical analysis of knowledge. I show below that both aspects turn up in conflicts within the network of logical empiricism.

From the outside, logical empiricist philosophy was often conceived as an inadequate philosophy concerning both the epistemology of historiography and a historical perspective on knowledge. Two examples of early criticism of logical empiricist philosophy exemplify this negative reception. First, in 1937, Max Horkheimer lamented the new metaphysical reductionism of two important representatives of logical empiricism: Carnap and Neurath.⁶ According to Horkheimer, their epistemology left no room for any knowledge concerning human values or human ideas. This hostile publication by Horkheimer was also the definitive break between the *Institut für Sozialforschung* and Neurath's "Unity of Science" movement.⁷ Second, in 1942, Ernst Cassirer attacked Carnap's physicalist epistemology as a reductionist denial of the possibility of historical knowledge.⁸ Recent work on logical empiricist philosophers has already dispelled the idea that these philosophers were not engaged with sociological and historical views on science.⁹ It is, however, still an open question how the issues concerning history played a role within the network of these philosophers and to what extent these issues produced actual struggles between the actors involved. The exposition below is not a complete survey of how these issues played out in the broad logical-empiricist network; however, it shows that they played a significant role in the interaction between the members of the network.

It is hard to define the philosophical content of logical empiricism, and probably impossible to maintain that there was one distinct logical empiricist view of historical knowledge, or one view of the relationship between history and science.¹⁰ Rather than thinking in terms of doctrines or philosophical attitudes, it is much easier to define

logical empiricism as a network of scholars mainly centered around Vienna and Berlin, operating in the 1930s, regularly writing on each other's work, organizing conferences together, publishing a journal (*Erkenntnis*, which ran from 1930 to 1938) and a monograph series (the *Encyclopedia of Unified Science*).¹¹ Within this network, there was a lot of debate over the role of history in philosophy, and over philosophy of history. In what follows, I will highlight these debates by looking at the exchanges about the nature of history between two key figures in the network.

In the first part of the paper, I focus on Hans Reichenbach's struggles over historically oriented philosophy within his practice as editor of *Erkenntnis* and as a leading figure of the Berlin Society for Empirical Philosophy (*Gesellschaft für empirische Philosophie*).¹² In the second part, I deal with similar problems experienced by Otto Neurath as an organizer of the Unity of Science conferences and editor of the *Encyclopedia of Unified Science*.¹³

Reichenbach's practical struggle with historical school 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*,¹⁴ sent out a letter to Edgar Zilsel, asking Zilsel to contribute a manuscript to the new journal.¹⁵ 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, and, consequently, of great interest to Reichenbach. First, the paper would "consider historical events as natural events and seek to connect them through statistical laws".¹⁶ Second, it would "treat the rise of the exact sciences and show how the so-called *Geisteswissenschaften* as they are pursued today, are the remnants of a prescientific time".¹⁷ The paper would carry out this historical research "by approaching the presentation of its material as is customary in physics journals".¹⁸ Consequently, Zilsel's paper would participate in both aspects of the issue concerning history mentioned above: it would present an epistemology of history, and it would also present a historical genealogy of this specific epistemology by relating it to the origins of the natural sciences.¹⁹

Reichenbach was very happy with the proposal: it accorded well with the intention of the journal to promote philosophy as 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 in which 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 a division of labor. We hope to present such philosophy of the *Geisteswissenschaften* in this journal as well.²⁰

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. He hoped that this manuscript would fulfill Reichenbach's wish "to have a contribution

from philosophy of history and sociology”.²¹ After five months, however, Reichenbach rejected Zisel’s paper, because it was too long.²² A week after this initial rejection, Reichenbach went back on 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 Zisel to remove the examples that were “sprinkled into the text”: “a philosophical journal is only concerned with the principal ideas”.²³ Zisel refused to comply with Reichenbach’s advice, because his examples were not an accidental feature of the paper. Zisel’s motivation highlights how he 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 purposeful. [...] These days there is a large amount of work in philosophy of history that uses a meta-physical strategy of argumentation. Moreover, there are many 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. Such a contribution would, however, lack any scientific fruitfulness and remain unconvincing to any expert.²⁴

After this refusal by Zisel to cut out his examples, the paper lost any chance of publication in *Erkenntnis*. Reichenbach considered the final rejection of the paper regrettable, and he apologized for the late decision.²⁵ As a result, a contribution both on the historical origins of modern science and on the nature of historical knowledge was never published in *Erkenntnis*. Even though Zisel’s philosophical program was aimed at exactly the kind of contribution from the *Geisteswissenschaften* that Reichenbach wanted, he would not reserve more space for Zisel’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 Zisel wanted to break.²⁶ Zisel’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. As Zisel realized, a full articulation of this kind 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 during the last decades”.²⁷ 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”.²⁸ Remedying this situation would benefit both students from the natural sciences and of philosophy. Therefore, through the memorandum, the *Society for Empirical Philosophy* requested

new philosophy chairs for natural philosophy and, if there were insufficient 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 (including Hilbert, Einstein, Planck, and Haber). Most of them gave their approval to the petition. Only one German philosopher received the draft: Ernst Cassirer.²⁹ Reichenbach believed that Cassirer was a supporter of his cause: “I know that you, like us, see fault in the existing one-sided occupation of philosophical chairs”.³⁰ 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”.³¹

In 1931, Cassirer, the former rector of Hamburg University (1929–1930), was one of the foremost 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 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.³²

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.³³

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 a scientific approach, when one asks that the existing one-sidedness in the occupation of philosophical chairs be discontinued.³⁴

He wrote to Cassirer that this clause would not be welcomed by some scientists who had already given their signature, especially David Hilbert.³⁵ “You cannot imagine how widespread 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”.³⁶ For Cassirer, there was no conflict at an intellectual level between philosophy 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 in the petition was solely on the practical level of creating more institutional space for philosophy oriented to the natural sciences. 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”.³⁷ 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”.³⁸ 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 the 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”.³⁹ Similarly, Hahn thought the chances of success were very low, while the formulation of the memorandum worked against the unity of science. Neurath fully agreed with Schlick. Finally, Carnap first described himself as neutral, but at the 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.⁴⁰

One year before this rejection of the petition, Reichenbach, Carnap, and Schlick had already seriously discussed a similar issue, and their disagreement had great consequences.⁴¹ 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.⁴² In particular, certain passages seemed to suggest that Reichenbach still considered the content 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.⁴³ 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”.⁴⁴

This reproach by Reichenbach was not received well in Vienna. Carnap wrote back that Schlick had withdrawn from his editorial responsibilities on *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.⁴⁵

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 *Erkenntnis*: “Remarks on Reichenbach’s Book: Goals and Directions of Contemporary Natural Philosophy”. Neurath remarked that Reichenbach maintained a distinction between natural philosophy as a counterpart to cultural philosophy or cultural scientific philosophy, which also implied a parallelism between the

cultural sciences and the natural sciences.⁴⁶ According to Neurath, however, 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?”⁴⁷ 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. These demarcations were dominant in German contemporary philosophy and originated within the debates over historical knowledge.

Reichenbach was furious after reading Neurath’s note and refused to publish it. He even threatened to resign as editor of *Erkenntnis* if Neurath pressed him to publish it.⁴⁸ Carnap convened with Hahn and Neurath, after which Neurath decided to withdraw the discussion note. He would reformulate his questions into a positive contribution.⁴⁹ 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 setbacks 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 New York University:

For some time now I have been 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 conducts natural philosophy in Germany, might be better understood in America than in Germany, where we constantly have to struggle against the dominance of historically oriented school philosophy.⁵⁰

This letter was the start of Reichenbach’s attempt to emigrate to the United States, which concluded in his appointment at UCLA in 1938.⁵¹ 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 relationship 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.

Neurath’s attempts to focus the movement on history

The writings of logical empiricist philosophers did not contain an elaborate engagement with the intellectual issues surrounding historical knowledge. In the years after the

disagreement between Reichenbach and the Viennese philosophers, this lack of engagement reappeared in the correspondence between other actors of the logical empiricist network. Several interactions surrounding Otto Neurath during his years in Holland are very good indicators of the disagreement.⁵² In 1935, Otto Neurath and Carl Hempel, who was Reichenbach's former PhD student, corresponded over these issues. 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's and Oppenheim's use of the distinction between the natural sciences and the cultural sciences. He wrote that some readers would reify Hempel's and Oppenheim's use of a distinction between two types of science while such a distinction was unnecessary. He observed sarcastically that "there are authors who use 'nomothetic' and 'idiographic' as true Windelbandites and who dream of differential quotients, and speak of set theory, as if they have been fed such distinctions with their mother's milk".⁵³ Neurath advised not to analyze science by starting from certain rigid distinctions, e.g. between the natural and the cultural sciences:

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 conducts 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.⁵⁴

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 it as gradual".⁵⁵ These ideas of Neurath were already 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.⁵⁶

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 in a historical manner, i.e. to determine an author as the representative of a tendency; to this end one should draw out the complete situation of science alongside its origin".⁵⁷ Neurath said that "many predecessors of Einstein and Mach were more modern than their contemporaries exactly because of their historical readings".⁵⁸ It is, consequently, good to know how a problem in science arose that is still of actual importance. Neurath had consistently employed this historicizing method in his own work, *Empirische Soziologie* of 1931, in which he had traced the historical origins of empirical sociology in national economy and historiography.⁵⁹ The lack of a historical view on knowledge in Hempel's work concerns the second aspect of the issues surrounding the historical sciences: what does it mean to have a historical perspective on knowledge? Through his comments on the manuscript, Neurath showed that he did not want to exclude 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".⁶⁰ 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 extensive literature on the topic.⁶¹ 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.⁶² Hempel, however, 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:

I dare not engage in 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.)⁶³

Neurath replied that he considered Hempel's 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:⁶⁴

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.⁶⁵

Hempel never engaged with this plea from Neurath and 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.⁶⁶ For him, it was a crucial aspect for the future of the 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 theoretical position. 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 recently published book, *Logik der Forschung*, Hempel wrote multiple letters on the topic. Clearly, Neurath's plea was not having its desired effects on Hempel.

Neurath mentions Schlick's paper on Windelband and Rickert in three different letters as a "disconsolate contribution to *Erkenntnis*", and wondered what Hempel thought about it.⁶⁷ 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".⁶⁸ 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.⁶⁹ 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”.⁷⁰ On his view, Schlick was writing about sociology and history in an utterly uncritical way.

In his paper published in *Erkenntnis*, Schlick had claimed that a modern historian could not principally inquire into her subject matter differently than Thucydides.⁷¹ 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 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.⁷² 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);⁷³ 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*).⁷⁴ Schlick partly follows Windelband’s story: since the nineteenth 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 was only one type of knowledge: the historical sciences do not warrant “a major revolution at the level of world view”.⁷⁵ 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, as the production of the worldview in the understanding (*Verstand*) necessarily relied on general concepts.⁷⁶ 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 psychological life (*Gesetze des Seelenlebens*) could perform such a function, and these could be reduced to the natural sciences.⁷⁷ 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.⁷⁸

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 ancient historian Thucydides as an example of good historical practice. In Neurath’s eyes, this was not scientific philosophy, but poor metaphysics. In 1935, Neurath could not persuade Hempel to engage with historiography or to train himself in a historical perspective on knowledge,⁷⁹ and Neurath could not agree with Schlick’s position on the relation between history and philosophy. Between 1935 and the outbreak of the war, Neurath attempted to enlist other allies in his quest to articulate a proper position on history within an empiricist view of unified science.

One of these 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*.⁸⁰ 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.⁸¹ Kaufmann proposed writing a contribution for *Erkenntnis* that would investigate how Carnap's and Neurath's physicalist position on the social sciences differed from his own views.⁸² Neurath welcomed such a contribution and wanted to review Kaufmann's book in *Erkenntnis*.⁸³ They decided to send in some kind of discussion piece for *Erkenntnis*, with Neurath's remarks and Kaufmann's answers. Carnap was alerted as editor of the journal and agreed on the idea.⁸⁴ Specifically, Kaufmann believed that "social theoretical analyses of neighboring movements should also be published in *Erkenntnis*".⁸⁵ Even though Neurath "had ploughed through the book many times" and was set on writing a response, he never got to finish the work.⁸⁶

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 *Encyclopedia* an idea that Kaufmann applauded.⁸⁷ Six months later, however, 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?"⁸⁸ 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 unraveled. In 1938, Kaufmann was preoccupied with his emigration 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 Kaufmann's book: it was a preliminary discussion of several passages of the work, apparently written during the summer of 1936.⁸⁹ Kaufmann would never 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".⁹⁰ 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".⁹¹ Even though Neurath constantly renewed his intentions, they were never actualized: he died on 22 December 1945. Neurath's interaction with Kaufmann, however, is a vivid testimony to Neurath's agenda to promote 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's 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".⁹²

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 reported 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.⁹³ When von Mises informed Neurath that his planned publisher, Springer, was no longer willing 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.⁹⁴ As Neurath now acted as editor of the text, he did not want to influence von Mises's content. However, he could not restrain himself from making some remarks on the manuscript, especially concerning von Mises's 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.⁹⁵ Von Mises, consequently, made some changes to his manuscript to better account for Neurath's position.⁹⁶ 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.

Neurath, however, was also concerned with dealing with the second aspect of the issues surrounding historiography: namely, to develop historical views on knowledge. To this end, Neurath solicited several authors to write an *Encyclopedia* monograph on the history of science. Initially, Neurath invited 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.⁹⁷ Sarton was a Belgian scientist who had migrated to the United States in 1913 and become a lecturer in the history of science at Harvard University.⁹⁸ 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.⁹⁹ In a second attempt, Neurath managed to 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".¹⁰⁰ 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 advertisements in their respective journals.¹⁰¹ At Neurath's request, Sarton also agreed to become a member of the advisory committee of the *International Encyclopedia of Unified Science*. After Sarton's acceptance, Neurath asked him to write a volume on the history of science, specifically on "the manifold importance of the history of sciences for an encyclopedic organization of knowledge".¹⁰² 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".¹⁰³ 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.¹⁰⁴

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.¹⁰⁵ Neurath's long paean on the history of science to Sarton did not grant him success: even though Sarton would "gladly write the article", he declined the monograph proposal due to a lack of time.¹⁰⁶

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 the University of Southern California in 1935.¹⁰⁷ 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' cannot treat of both sociology and the specific methods of actual historical research)".¹⁰⁸ 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".¹⁰⁹ On Gomperz's 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.¹¹⁰

Because Neurath had always considered Gomperz's influence in Vienna as anti-meta-physical, Neurath welcomed any potential cooperation.¹¹¹ Consequently, Neurath decided to consult his co-editor of the *Encyclopedia*, Charles Morris, about the possibility of adding a volume on interpretation. 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.¹¹² Gomperz decided to attend the conference; his paper on interpretation was eventually published in the seventh volume of *Erkenntnis*.¹¹³ Just before the conference, Gomperz explicitly wrote that he wanted to avoid all polemic.¹¹⁴ 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.¹¹⁵ When Carnap visited Gomperz in the United States, his judgement on Gomperz's paper was neutral; Gomperz reported that Carnap said: "no one can say anything against that".¹¹⁶ So there was a general sense that Gomperz's ideas on interpretation could be a valuable intellectual addition. By that time, Neurath had already, like von Mises, offered Gomperz a book publication in the complementary series *Einheitswissenschaften*, which would be published by Van Stockum & Zoon in The Hague.¹¹⁷

During their correspondence over Gomperz's publication, they both 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.¹¹⁸ 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’.”¹¹⁹

Neurath believed that Gomperz, with his logic of interpretation, meant a “historicizing behavioural study of scholars” (*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?”¹²⁰ 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.¹²¹ Gomperz intended to “analyze what historians really do”.¹²² He thought this was not in opposition with the Vienna circle, but with the abstract normative positions of Mill and Zilsel, who would have historians aim for the formation of laws similar to those in the natural sciences.¹²³ Gomperz’s idea to start his analysis from historical work was very close to Neurath’s own remarks to Hempel three years before. Consequently, Neurath judged, in the end, that they were in fact close in their beliefs, even though he disagreed with Gomperz’s negative attitude towards the Vienna Circle’s physicalism and its relation to the historical sciences.¹²⁴

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?”¹²⁵ 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 Jørgensen, 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.¹²⁶

Neurath explained that his use of “we” referred to the scientific culture of his movement in opposition to an older, “sectarian” philosophical culture.¹²⁷

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.¹²⁸

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.¹²⁹ 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.¹³⁰

Conclusion

With the benefit of hindsight, it is possible to wonder whether Gomperz was right in the end. Neurath's movement was divided on both aspects of Windelband's challenge: the epistemology of historiography and the role of a historical perspective of knowledge. Viennese philosophers disagreed with Reichenbach, Neurath disagreed with Hempel and Zilsel, and Schlick had his own specific position. In general, Neurath and Zilsel were the only philosophers who thought both issues should be addressed properly by analyzing historical works, not theoretical views on historiography. Although Sarton, Kaufman, Gomperz, and Cassirer played a role at the margins of the network, they had no impact on the direction of logical empiricist philosophy. By 1946, of all the aforementioned actors in the network, only Hempel, Carnap, and Reichenbach remained: these philosophers did not have much interest in historiography as a discipline or in a historicized view of knowledge.

In the 1930s, Hempel, Carnap, and Reichenbach had been confronted with both aspects of Windelband's challenge, but mainly refrained from articulating a position. None of them shifted attention in their work to these issues, and, by 1946, all of them worked in a U.S. academic climate that was entirely different from the German, historically-oriented school philosophy, as Reichenbach had described to Hook with terror. Neurath's attempts to bring history into the theoretical concerns of his movement failed: Hempel was not persuaded, Sarton could not be brought to contribute, and Gomperz's efforts remained mute to the other members.¹³¹ Thus, the issues concerning history were felt in the network of logical empiricism and they had their effects, but, by the end of the Second World War, they were completely dissolved.

The contrast between the practical efforts of Reichenbach and Neurath shows that Reichenbach was only concerned with discipline-formation: carving out a place for scientific philosophy in academic institutions, which, for Reichenbach, implied a struggle with historically oriented philosophy on the practical level only. Otto Neurath, however, was concerned with the creation of a movement, in which theoretical problems about history had to be addressed. When the whole network dispersed after 1933 and gradually found ground in the United States in the 1940s, Reichenbach's project of discipline formation found fertile soil, as the disciplinary climate of philosophy was very different. Neurath simply died before he was able to convince others to address the intellectual issues over history. As a result, all incentive to deal with history as a philosophical problem dissipated and Windelband's intellectual challenge was forgotten when academic, U.S. philosophy of science came of age.

Notes

1. Windelband, “Nach Hundert Jahren”.
2. Windelband, “Nach Hundert Jahren”, 11.
3. Dilthey, *Die Einleitung in Die Geisteswissenschaften*, vol. 1, XVII; Windelband, “Rectorial Address, Strasbourg, 1894”. Windelband was not the only, or even the most prominent, philosopher to engage with the philosophical problems over historiography as science. His 1904 text, however, articulates these problems as central to the agenda of future philosophy in the twentieth century. In that sense, it is a good starting point to understand what was at stake in German academic philosophy at the turn of the century. For further contextualization and historicization of Windelband’s agenda, see Beiser, *The German Historicist Tradition*.
4. Windelband, “Nach Hundert Jahren”, 20.
5. Dilthey, *Die Einleitung in Die Geisteswissenschaften*, vol. 1, XIX.
6. Horkheimer, “Der Neueste Angriff Auf Die Metaphysik”.
7. Dahms, *Positivismusstreit*, chap. 5.3.
8. Cassirer, *Zur Logik der Kulturwissenschaften*.
9. Nemeth, “The History and Sociology of Science”; Uebel, “Philosophy of Social Science in Early Logical Empiricism”; Uebel, “Logical Empiricism and the Sociology of Knowledge”; Uebel, “Opposition to Verstehen in Orthodox Logical Empiricism”.
10. For an overview of the various problems with the term “logical-positivism” as a means to denote a set of doctrines, philosophical attitudes, and even self-descriptions, see Uebel, “‘Logical Positivism’ – ‘Logical Empiricism’”.
11. For a discussion on the similarities and connections between the Berlin group and the Vienna circle, see Sandner and Pape, “Report/Documentation – From ‘Late Enlightenment’ to Logical Empiricism”.
12. The discussed correspondence below is taken from the Hans Reichenbach Papers, 1884–1972, ASP.1973.01, Archives of Scientific Philosophy, Special Collections Department, Hillman Library, University of Pittsburgh (henceforth ASP). Quoted by permission of the University of Pittsburgh. All rights reserved.
13. The discussed correspondence below is taken from the Otto Neurath Nachlass, which is located in the Vienna Circle Archives at the Noord-Holland archief, Haarlem (henceforth VCA). Quoted by permission of the Wiener Kreis Stichting, Amsterdam. All rights reserved.
14. For more information on the origins of Erkenntnis as a journal, edited by Reichenbach and Carnap, see Hegselmann and Siegwart, “Zur Geschichte Der ‘Erkenntnis’”; Stadler, *The Vienna Circle*, 56–7.
15. Reichenbach to Zilsel, 29 April 1930, HR 013-38-32 ASP.
16. Zilsel to Reichenbach, 2 May 1930, HR 013-38-31 ASP: “erstens weil sie geschichtliche Vorgänge selbst als Naturvorgänge betrachten und durch statische Gesetzmässigkeiten miteinander zu verknüpfen sucht”.
17. Zilsel to Reichenbach, 2 May 1930, HR 013-38-31 ASP: “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”.
18. Zilsel to Reichenbach, 2 May 1930, HR 013-38-31 ASP: “Auch die Darstellungsart wird sich jener annähern, die etwa in physikalischen Zeitschriften üblich ist”.
19. 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. This concrete episode shows how Zilsel performed this interest in practice. For Neurath’s attempt, see section 3. Stadler, *The Vienna Circle*, 10.
20. Reichenbach, “Zur Einführung”, 1–2: “Solange die Naturwissenschaften wie bisher den weitaus grössten Teil an Erkenntnissen in die Philosophie hineinbringen, 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”.

21. Zilsel to Reichenbach, 8 May 1930, HR 013-38-29 ASP: "In den Fall dass es Ihnen angenehm ist, schon in den nächsten Wochen einen geschichtsphilosophisch-soziologischen Aufsatz mit Sicherheit zu erhalten".
22. Reichenbach to Zilsel, 4 October 1930, HR 013-38-25 ASP.
23. Reichenbach to Zilsel, 16 October 1930, HR 013-38-23 ASP: "für unsere philosophische Zeitschrift kommt es ja nur auf die prinzipiellen Gedanken an".
24. Zilsel to Reichenbach, 18 October 1930, HR 013-18-22 ASP: "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".
25. Reichenbach to Zilsel, 20 October 1930, HR 013-18-21 ASP.
26. 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, "Philosophy of Social Science in Early Logical Empiricism", 255.
27. Reichenbach to Minister (draft), undated, HR 025-11-20 ASP: "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".
28. Reichenbach to Minister (draft), undated, HR 025-11-18: "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".
29. For a discussion of the philosophical relationship between Reichenbach's scientific philosophy and Cassirer's historical views on knowledge, see Heis, "Ernst Cassirer, Kurt Lewin, and Hans Reichenbach".
30. Reichenbach to Cassirer, 5 June 1931, HR 025-11-04 ASP: "Ich weiss, dass Sie, ebenso wie wir, in der bisher bestehenden Einseitigkeit bei der Besetzung philosophischer Lehrstühle einen Fehler sehen". 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.
31. Reichenbach to Cassirer, 5 June 1931, HR 25-11-04 ASP: "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".
32. Cassirer to Reichenbach, 11 June 1931, HR 25-11-02 ASP: "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".
33. Cassirer to Reichenbach, 11 June 1931, HR 25-11-02 ASP: "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”.

34. Reichenbach to Minister (draft), June 1931, HR 025-11-33 ASP: “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”.
35. Reichenbach to Cassirer, 15 June 1931, HR 025-11-03 ASP: “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”.
36. Reichenbach to Cassirer, 15 June 1931, HR 025-11-03 ASP: “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”.
37. Reichenbach to Carnap, 10 June 1931, HR 013-41-53 ASP: “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”.
38. Carnap to Reichenbach, 11 July 1931, HR 013-41-52 ASP: “Wir sind in Wien über diese Fragen wesentlichen einer Meinung”.
39. Carnap to Reichenbach, 11 July 1931, HR 013-41-52 ASP: “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 durften die andern ihre Sache in beliebiger Weise betreiben”.
40. Carnap to Reichenbach, 11 July 1931, HR 013-41-52 ASP: “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?”
41. 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, “‘Logical Positivism’ – ‘Logical Empiricism’”, 88.
42. Carnap to Reichenbach, 29 April 1930, HR 013-41-66 ASP: “Sie machen der traditionellen Philosophie hier in einem Grade Zugeständnisse, der mich nach Ihren bisherigen Stellungnahmen sehr verwundert hat”.
43. For this intellectual opposition of the Vienna circle against an autonomous discipline of philosophy, also see Stadler, *The Vienna Circle*, 42; Stadler, “The Road to ‘Experience and Prediction’ from Within”; Uebel, “Writing a Revolution”, 90.
44. Reichenbach to Carnap, 6 May 1930, HR 013-41-65 ASP: “Unser Programm muss ein Arbeitprogramm sein, und nicht das durch eine spezialisierte Terminologie missverständliche Programm einer bestimmten philosophischen Richtung”.
45. Reichenbach to Carnap, 6 May 1930, HR 013-41-65 ASP: “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”.
46. Neurath to Reichenbach, Zuschrift an die Herausgeber, undated, HR 013-41-50 ASP: “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”.

47. Neurath to Reichenbach, *Zuschrift an die Herausgeber*, undated, HR 013-41-50 ASP: "Gibt es neben Naturwissenschaften nur die Literatur, oder neben dieser noch Kulturwissenschaften und Kulturphilosophie, der übergriffe man abwehren müsse?"
48. Reichenbach to Carnap, 22 August 1931, HR 013-41-49 ASP.
49. Carnap to Reichenbach, 9 September 1931, HR 013-41-48 ASP.
50. Reichenbach to Hook, 20 August 1931, HR 014-51-28 ASP: "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".
51. For an overview of Reichenbach's period in Istanbul between his position in Berlin and his appointment at UCLA, see Irzik, "Hans Reichenbach in Istanbul".
52. In the Vienna Circle meetings, Otto Neurath had regularly insisted on the integration of history of science and sociology of science. Stadler, *The Vienna Circle*, 45; Carnap, "Intellectual Autobiography", 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, *Otto Neurath*, 234–96.
53. Neurath to Hempel, 2 February 1935, Nr. 244 VCA: "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".
54. Neurath to Hempel, 2 February 1935, Nr. 244 VCA: "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".
55. Neurath to Hempel, 2 February 1935, Nr. 244 VCA: "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".
56. In 1931, Neurath also published his work *Empirische Soziologie*, in which he argued for a physicalist epistemology for the social sciences, against any possible *a priori* distinction between the social sciences and the natural sciences. His targets were, among others, the philosophical distinction of Windelband and Rickert. Neurath, *Empirische Soziologie*, 56.
57. Neurath to Hempel, 2 February 1935, Nr. 244 VCA: "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".
58. Neurath to Hempel, 2 February 1935, Nr. 244 VCA.: "Es ist äusserst reizvoll zu sehn, dass manche Autoren vor Einstein und vor Mach durch entsprechende historische Lektüre wesentlich moderner waren, als ihre Zeitgenossen!"
59. Neurath, *Empirische Soziologie*, chaps 3 and 4.
60. Hempel to Neurath, 6 February 1935, Nr. 244 VCA: "die empirischen Forscher selbst machen oft ganz falsche Angaben über ihre Methoden, man muss ihre Einzelarbeit analysieren".
61. Hempel to Neurath, 6 February 1935, Nr. 244 VCA: "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".
62. Hempel to Neurath, 6 February 1935, Nr. 244 VCA.
63. Hempel to Neurath (private letter), 6 February 1935, Nr. 244 VCA: "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.)”.

64. In *Empirische Soziologie*, Neurath had already similarly lamented that philosophers of the logical empiricist movement “took physics and mathematics as exemplary elements of discussion, while history, national economy and sociology are neglected”. He concluded that this neglect had to be rectified by the younger adherents. Neurath, *Empirische Soziologie*, 142.
65. Neurath to Hempel, 8 February 1935, Nr. 244 VCA: “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 misbraucht werden”.
66. Neurath also applied this historicizing, “Machian” attitude himself in his early work on political economy and economic history. Nemeth, “The History and Sociology of Science”, 287.
67. Neurath to Hempel, 5 February 1935, Nr. 244 VCA: “den trostlosen Ausführungen Schlicks”.
68. Neurath to Hempel, 5 February 1935, Nr. 244 VCA: “Er scheint wirklich nichts von modernen Untersuchungen zu kennen, und dass die Geschichtsdarstellungen ebenso sich ändern, wie andere Dinge, neue Sachen wichtig fanden”.
69. Neurath to Hempel, 8 February 1935, Nr. 244 VCA: “Ich habe es nicht gern, wenn man beim Exemplifizieren, die Physik in ihrem besten Exemplaren verführt, während man die Geschichte der ‘Taferlklasse’ behandelt”.
70. Neurath to Hempel, 7 March 1935, Nr. 244 VCA: “Und wenn ich mal so anfang, 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”.
71. Schlick, “Philosophie Und Naturwissenschaft”, 391.
72. Schlick, “Philosophie Und Naturwissenschaft”, 379.
73. Schlick, “Philosophie Und Naturwissenschaft”, 381–2.
74. Schlick, “Philosophie Und Naturwissenschaft”, 384.
75. Schlick, “Philosophie Und Naturwissenschaft”, 391.
76. Schlick, “Philosophie Und Naturwissenschaft”, 394.
77. Schlick, “Philosophie Und Naturwissenschaft”, 391.
78. Schlick, “Philosophie Und Naturwissenschaft”, 394: “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”.
79. In 1942, Hempel published a paper on history: Hempel, “The Function of General Laws in History”. However, later in life, Hempel could not remember why he chose that particular topic for the paper. See Nollan and Hempel, “An Intellectual Autobiography”, 20. Moreover, Neurath was extremely displeased with Hempel’s theoretical approach, because it had little connection to actual work in historiography. Neurath to Hempel, 25 November 1944, Nr. 246 VCA.
80. Kaufmann, *Methodenlehre der Sozialwissenschaften*.
81. Kaufmann to Neurath, 10 July 1935, Nr. 255 VCA.
82. Kaufmann to Neurath, 9 October 1935, Nr. 255 VCA.
83. Neurath to Kaufmann, 14 October 1935, Nr. 255 VCA.

84. Neurath to Kaufmann, 30 October 1935, Nr. 255 VCA.
85. Kaufmann to Neurath, 22 October 1935, Nr. 255 VCA: "Ich glaube, dass in der 'Erkenntnis' auch die sozialtheoretischen Analysen den Gebührenden Platz finden sollten". Kaufmann considered his own viewpoint as outside of, but nonetheless related to, the logical-empiricist movement. Consequently, he describes himself as a neighbor [*Gebührende*].
86. Neurath to Kaufmann, 11 June 1937, Nr. 255 VCA.
87. Neurath to Kaufmann, 11 June 1937, Nr. 255 VCA; Kaufmann to Neurath, 16 June 1937, Nr. 255 VCA.
88. Neurath to Kaufmann, 21 January 1938, Nr. 255 VCA: "Ich habe schon alles beisammen. Ich muss nur einmal die Sache niederschreiben. Sie bekommen das bald. [...] Warum kann man sich nicht multiplizieren?"
89. Ad Felix Kaufmann, Methodenstreit, Nr. 212 K.118 VCA.
90. 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.
91. Neurath to Kaufmann, 7 July 1945, Nr. 255 VCA.
92. Kaufmann to Neurath, 27 January 1938, Nr. 255 VCA: "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".
93. Von Mises to Neurath, 9 December 1937, Nr. 268 VCA.
94. Von Mises to Neurath, 30 June 1938, Nr. 268 VCA. This agreement resulted in the publication of publication of the book one year later: Mises, *Kleines Lehrbuch des Positivismus*.
95. Neurath to von Mises, 24 May 1939, Nr. 268 VCA.
96. Von Mises to Neurath, 5 June 1939, Nr. 268 VCA.
97. Enriques to Neurath, 30 September 1938, Nr. 232 VCA. In the 1930s and 1940s, Neurath also attempts to solicit a historical volume from I.B. Cohen. This attempt, however, similarly failed. See Fuller, *Thomas Kuhn*, 286. Neurath also solicited for a monograph 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, "Did Kuhn Kill Logical Empiricism?"
98. Garfield, "The Life and Career of George Sarton", 113.
99. Neurath to Sarton, 15 February 1936, Nr. 298 VCA.
100. Sarton to Neurath, 26 July 1938, Nr. 298 VCA.
101. Sarton to Neurath, 2 November 1938, Nr. 298 VCA.
102. Neurath to Sarton, 23 November 1938, Nr. 298 VCA.
103. Neurath to Sarton, 23 November 1938, Nr. 298 VCA.
104. Neurath to Sarton, 23 November 1938, Nr. 298 VCA.
105. 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, "Die 'Encyclopedia of Unified Science' (IEUS)", 116.
106. Sarton to Neurath, 4 December 1938, Nr. 298 VCA.
107. 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's relation to the members of the Vienna Circle, see Stadler, *The Vienna Circle*, chap. 7.
108. Gomperz to Neurath, 8 November 1937, Nr. 240 VCA: "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)".
109. Gomperz to Neurath, 8 November 1937, Nr. 240 VCA: "Was den sogenannten Physikalismus betrifft, so dürfte sich allerdings wahrscheinlich ergeben, dass er uns in die Geschichte wenig hilft".

110. Gomperz to Neurath, 8 November 1937, Nr. 240 VCA.
111. Neurath to Gomperz, 26 November 1937, Nr. 240 VCA.
112. Neurath to Gomperz, 7 January 1938, Nr. 240 VCA.
113. Gomperz, "Interpretation", 1937.
114. Gomperz to Neurath, 30 June 1938, Nr. 240 VCA.
115. Neurath to Gomperz, 5 August 1938, Nr. 240 VCA.
116. Gomperz to Neurath, 14 December 1938, Nr. 240 VCA.
117. Neurath to Gomperz, 27 August 1938, Nr. 240 VCA. This short monograph actually appeared in 1939 under the title "Interpretation: A Logical Analysis of a Method of Historical Research"; Gomperz, *Interpretation*, 1939. To my knowledge, it had little or no impact inside or outside the network of logical-empiricist philosophers.
118. 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, *Empirische Soziologie*, 70.
119. Neurath to Gomperz, 26 November 1937, Nr. 240 VCA: "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".
120. Neurath to Gomperz, 26 November 1937, Nr. 240 VCA: "Wer von uns sollte etwas dagegen haben, wenn Jemand, der das gut weiss, uns erzählt, wie man solche Untersuchungen jetzt erfolgreich anstellt [...]?" 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, "Philosophy of Social Science in Early Logical Empiricism", 258.
121. Gomperz to Neurath, 11 September 1938, Nr. 240 VCA.
122. Gomperz to Neurath, 18 November 1938, Nr. 240, VCA.
123. 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 as Gomperz was Zilsel's dissertation advisor. Nemeth, "The History and Sociology of Science", 293; Zilsel, *The Social Origins of Modern Science*, xli.
124. Neurath to Gomperz, 2 January 1939, Nr. 240, VCA.
125. Gomperz to Neurath 11 September 1938, Nr. 240 VCA: "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?"
126. Gomperz to Neurath, 11 September 1938, Nr. 240 VCA: "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 Jørgensen, 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".
127. 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, *The Vienna Circle*, 66.
128. Neurath to Gomperz, 1 November 1938, Nr. 240 VCA: "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".
129. Neurath to Gomperz, 2 January 1939, Nr. 240 VCA.

130. Gomperz to Neurath, 18 November 1938, Nr. 240 VCA: “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 natuerlich von dem zur Selbstverstaendlichkeit werdenden alten Kohl auf die noch nicht hinreichend durchgedachten Einzelfragen. Das nennt man eben historische Entwicklung, und es is gut so, denn da es eine vorgaengige Gewaehr fuer Ubereinstimmung ueber alle Einzelfragen nicht gibt, so liesse sich jene Entwicklung nur dadurch vermeiden, dassesaendig der alte Kohl wiedergekaeut und jedes Weiterdenken unterlassen wird”.
131. There already is a considerable amount of research that shows how Neurath’s remarks on developments within the movement during the Second World War did not have their desired effect and that many of Neurath’s ideas disappeared from later developments in U.S. philosophy of science. My discussion shows how the place of (philosophy of) history was one of the issues that was significantly affected by this rift. See Reisch, *How the Cold War Transformed Philosophy of Science*, chap. 10; Reisch, “Against a Third Dogma of Logical Empiricism”; Reisch, “Terminology in Action”.

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