

WAYS OF THE SCIENTIFIC WORLD-CONCEPTION

RUDOLF CARNAP AND OTTO NEURATH

EDITED BY

*Christian Damböck,
Johannes Friedl
and Ulf Höfer*

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Introduction

Christian Damböck, Johannes Friedl and Ulf Höfer

The Vienna Circle used to be seen as a uniform group of naïve philosophers who had unsuccessfully tried to revive dogmatic sense data empiricism in a logicist setting. Decades later, the astonishing plurality and diversity of positions and strategies of these philosophers are widely appreciated and, in fact, no one today believes that the Vienna Circle was ever committed to any form of dogmatic empiricism at all. The common ground shared by the members of the Vienna Circle included a concern for unification and interdisciplinary integration, the linguistic turn, and a non-cognitivist approach to values. Beyond these very general principles, however, there was plenty of room for pluralism and dissent. Perhaps the most striking illustration of the coexistence of commonalities and tensions in the Vienna Circle is the philosophical and personal relationship between Otto Neurath and Rudolf Carnap to whom this volume is dedicated.

The considerable diversity of opinion within the Vienna Circle had already been attested to by its members:

How good was the title WIENER KREIS, partly because it says so little. In this way it was possible to give a name to rather different tendencies within a uniform togetherness, and as I had predicted, it became popular very quickly.¹

What is true of the Vienna Circle as a whole is also true of that part of it which is often referred to by the term “left wing”, a subgroup formed by Otto Neurath, Rudolf Carnap, Hans Hahn, Philipp Frank, and Edgar Zilsel.² The unique selling proposition of this group was supposed to be a more radical focus on unified science, physicalism, internationalism, and language planning, together with a strong tendency toward socialism. But do these alleged representations consistently belong to the group? At the least the case of Hans Hahn is

1 “Wie gut war der Titel WIENER KREIS, zum Teil, weil er so wenig aussagt. So konnten recht differenzierende Neigungen innerhalb eines doch einheitlichen Zusammenseins mit einem Namen versehen werden, der sehr rasch populär wurde, wie ichs prophezeite” (Neurath to Carnap, August 3, 1939, ASP-RC 102-53-04).

2 Cf. Uebel 2004.

questionable.³ Edgar Zilsel was sometimes not even considered a full member of the Circle.⁴ On the other hand, it seems difficult to find any clear disagreements between the alleged “rightists” like Friedrich Waismann and his leftist opponents; Waismann, who was not merely an interpreter of Wittgenstein, had obviously shared as much philosophical substance with the left wing as he had taken, so to speak, from Wittgenstein. It is also striking that left-right-wing talk was hardly used by the proponents of the Circle themselves – some of them explicitly rejected these designations.⁵ However, not only is it difficult to delineate a consistent group of left wingers, but it is also difficult to identify any consistent views that were either shared by all left wingers or rejected by all right wingers. Although it is certainly possible to identify features such as the very general ones mentioned above – a stronger emphasis on the Unity of Science movement combined with a rather explicit commitment to socialism – as unique to the somewhat underdetermined group around Carnap and Neurath, it is simply impossible to unequivocally associate any more specific philosophical positions with the left wing. This becomes remarkably clear when we look at the philosophical disputes between Carnap and Neurath themselves.

The correspondence between Carnap and Neurath began in 1923,⁶ and they first met in person in the following year on the occasion of Carnap’s trip to the International Esperanto Congress in Vienna. From the beginning, the relationship was intense on both the personal and the philosophical level. Philosophical and ideological similarities formed the basis for a deep friendship while the recurring crises on the personal level were also rooted in conflicts of a more philosophical nature. The tense and productive character of their collaboration is expressed with emotion by Carnap himself:

3 This is due not only to his premature death in 1934: Hahn remained sceptical about physicalistic protocols as well as the idea of Unified Science, a concept he made fun of by misspelling the German expression “Einheitswissenschaft” (Unified Science) as “Einheizwissenschaft” (einheizen = to heat something/to light a fire under someone); cf. Neurath to Carnap, June 16, 1945, ASP-RC 102-55-11.

4 Cf. Verein Ernst Mach (1929/2012), p. 103. Also later, Carnap adhered to this classification (Carnap 1932/33, p. 181). Beside this formal restriction, it is worth mentioning that Zilsel, in his contribution to the protocol sentence debate (Zilsel 1932/33), fits badly with the other “left-wingers” concerning his admission of the “ineffable”.

5 This is not only true of publications: in the whole correspondence between Carnap and Neurath there are only a very few mentions to be found. Among those who rejected this classification is – unsurprisingly – Moritz Schlick, who disallowed this terminology flatly, calling it “ridiculous”; cf. Friedl 2013, p. 171, n. 8.

6 The first letter handed down is Neurath to Carnap, October 19, 1923 (RC 029-16-07). The contact was mediated by Franz Roh, a mutual close friend.

For me, the friendship with Neurath is one of the most meaningful and indispensable points in my life. I owe him a great deal, both personally and factually; above all, I owe him for having taught me to see the historical function of what I would otherwise have worked out only as an isolated scholar “on the icy slopes of logic”. My dream for the future is to collaborate productively with you on a much larger scale than before. One day, our names should stand side by side on the same page of history.⁷

At the beginning of their philosophical relationship, the focus was, of course, on *The Logical Structure of the World* (henceforth: *Aufbau*), a book that Carnap had written before coming to Vienna and used as his habilitation thesis at the University of Vienna. This book (along with Wittgenstein 1922/1961) set the stage for the discussions in the Vienna Circle. In Neurath’s view, the great merit of the *Aufbau* was that it established connections between all scientific concepts in a strictly logical manner, demonstrating that “there is only one domain of objects and therefore only one science”.⁸ The unity of science was no longer a vague methodological principle, but became a reality. But the *Aufbau* did not correspond to Neurath’s philosophical views in every respect. In fact, he found two serious flaws in Carnap’s work: First, “methodological solipsism” which Neurath blamed on “a weakened residue of idealistic metaphysics”⁹ that treated the self as privileged and opposed to the rest. Second, the construction of the *Aufbau* was limited to well-defined concepts and therefore had nothing to offer for the crucial task of dealing with those scientific and semi-scientific concepts that had not yet been fully analysed. The *Aufbau* failed to be applicable to all those ambiguous concepts and “agglomerations” that the Machian Neurath saw as indispensable components of the ever-changing boat of science and culture.¹⁰ At first, Carnap acknowledged only the second part of this critique and took it as a vital stimulus for further work;¹¹ in the 1930s, however, he abandoned methodological solipsism and finally embarked on the

7 “Die Freundschaft mit Neurath ist für mich einer der ganz bedeutungsvollen Punkte im Leben und mir unentbehrlich. Ihr verdanke ich menschlich und sachlich viel. Besonders, daß ich gelernt habe, die historische Funktion dessen zu sehen, was ich sonst nur als isolierter Gelehrter ‚auf den eisigen Firnen der Logik‘ erarbeiten würde. Produktive Kooperation mit Dir in weit größerem Maßstab noch als bisher ist mein Traum für die Zukunft. Unsere beiden Namen sollen einmal auf einem Blatt der Geschichte nebeneinander stehen” (Carnap to Neurath, February 10, 1932, ASP-RC 029-12-63).

8 Carnap 1928/2003, § 4; cf. Neurath 1930/1983, p. 47.

9 Neurath 1931/1983, p. 65.

10 Neurath 1928/1981, p. 296.

11 Carnap to Neurath, October 7, 1928, ASP-RC 029-16-01.

riverbank of Mach-Neurath-style monism when he introduced his own version of physicalism. Nevertheless, there remained a swelling source of disagreement with Neurath's initial points of criticism that could not be eliminated by Carnap's agreement. Although he conceded to Neurath the basic legitimacy of the sociological standpoint, Carnap often felt that Neurath simply failed to commit himself properly to a value-free scientific attitude. Neurath's notoriously pragmatic and spontaneous style often led to an alleged mixing of theory and personal attitude, to the displeasure of Carnap and other members of the Vienna Circle:

[Neurath] went further and often presented arguments of a more pragmatic-political rather than of a theoretical nature for the desirability or undesirability of certain logical or empirical investigations. [...] We in turn insisted that the intrusion of practical and especially of political points of view would violate the purity of philosophical methods.¹²

In principle, Carnap did not reject the sociological approach at all, and even considered the "scientific world conception" to be a fundamentally political undertaking.¹³ Still, he tried to keep theoretical knowledge and practical-political attitude separate. Consequently, he disagreed with Neurath on at least two levels: First, on a more methodological level, Carnap found some of Neurath's writings poorly constructed, unfounded, charged with unnecessary polemics and careless amalgamations of theoretical assertions and mere opinion. These methodological weaknesses could largely be corrected in the editorial process. But there was also a second level of much stronger disagreement. Carnap, who had changed his earlier views under the influence of Tarski, demanded a serious commitment to (semantic) objectivity, a conception that Neurath always viewed with suspicion.

The main components of the philosophical and personal relationship between Carnap and Neurath emerged at the very beginning of their interaction, namely mutual stimulation and criticism, as well as the common goal of promoting the scientific world conception, while at the same time deep differences crystallized around the tension between logic and the sociology of science. This mixture of strong convergences and significant but subtle points of disagreement proved inspiring and, in the end, explosive. During their interaction in Vienna, the volatile setting of their relationship unfolded mostly

¹² Carnap 1963, p. 22–23.

¹³ See Damböck 2022.

positively; they were united in the common fight against the enemies of science. Indeed, Carnap became Neurath's best student during his Vienna years, especially when he published his antimetaphysical writings and developed his physicalism. When Carnap went to Prague in the fall of 1931, Neurath felt somewhat lost, as Olga Neurath (Neurath's second wife) observed: "At present, Otto Neurath has no like-minded opponent with whom to spar."¹⁴

For Carnap, the intense interactions in Vienna, with a multitude of personal encounters and discussions, were followed by a more secluded way of life on the outskirts of Prague. He largely maintained a more "academic" lifestyle, with little or no further involvement in public intellectual discourse, even after his relocation to the United States in late 1935. Neurath, on the other hand, increasingly expanded his activities and promoted "visual education" far beyond Vienna.¹⁵ Facing a difficult time after his forced emigration to the Netherlands in 1934, Neurath managed to re-establish an institute for visual education, modelled on the highly successful *Gesellschafts- und Wirtschaftsmuseum*, which made him an integral part of Red Vienna (and a bogeyman for the Austrofascists). This episode ended abruptly, when the Nazis invaded the Netherlands in 1940. Neurath managed to escape to England at the last moment and was forced to start all over again. Irrepressible as he was, he soon resumed his research and initiated many activities until his untimely death in 1945.

The physical separation did not however mean a loosening of the relationship. In some ways, the collaboration even deepened after Carnap and Neurath had left their common Viennese ground. Their extensive correspondence documents the intense and highly productive, but also somewhat tragic and unfulfilled, relationship of two strikingly complementary personalities.¹⁶ Their joint work covered important organisational matters: work on the *International Congresses for the Unity of Science*, the *International Encyclopedia of Unified Science*, and other editorial work, e.g. for the journal *Erkenntnis*. They coordinated their publications on several occasions, often revising their manuscripts in the light of each other's (always quite direct and sometimes furious)

14 "Otto Neurath hat derzeit keinen gleichgesinnten Gegner, mit dem er sich herumraufen könnte" (Olga Neurath to Carnap, January 12, 1932, ASP-RC 029-12-72).

15 These activities are documented in Sandner 2014, e.g. Neurath's engagement in Moscow between 1931 and 1934 (pp. 227–233).

16 All in all, there are about 500 letters known (plus several postcards and telegrams), now held by the *Archive of Scientific Philosophy* in Pittsburgh respectively the *Vienna Circle Archive* at the *Noord-Hollands Archief* in Haarlem. Publication of a substantial selection (edited by Christian Damböck, Johannes Friedl, Ulf Höfer) is in preparation. For a preliminary survey on the topical main points, see Hegselmann 1985.

criticism. These controversies, however, have hardly been made explicit in their published work;¹⁷ only mild criticism can be found here and there. There are several reasons for this conspicuous lack of public conflict: First, both Carnap and Neurath felt a strategic need to present the emerging Logical Empiricism as a consistent movement. The opponents, including the whole of traditional philosophy, had to be countered by a united force; no inconsistencies or internal disputes were allowed to make the scientific world conception vulnerable to attack. Second, the discussion became particularly heated whenever a new field was entered, in which there was no clear position in sight. Neurath was always quick to present an extremely innovative account, full of potential but also full of ambiguities and inconsistencies. After endless discussions things usually calmed down, and the finally published results, though still often controversial, were no longer seen as provocative. A third reason for not arguing in the public sphere was simply that Carnap, as he often complained, found it notoriously difficult to understand Neurath's writings, especially in their early stages. Carnap often encouraged Neurath to find clearer expressions for his arguments, otherwise it would be impossible to continue discussions. On at least one occasion, Carnap directly asked Neurath to publish fewer papers. While Neurath understandably took offense,¹⁸ he occasionally conceded that there was room for improvement:

And now, overburdened with other work, I am lagging behind because I cannot carve out as well as you or Frank or Schlick. Everything comes out a little rough. *And I know that.*¹⁹

As mentioned above, major differences between Carnap and Neurath are discernible from the beginning. Nevertheless, until 1935, Neurath not only welcomed the *Aufbau* as a foundational text for the new philosophical movement, but he also fully appreciated the “syntactic period” of Carnap's work, which had culminated in *The Logical Syntax of Language* – the achievements of which Neurath regarded as the final building block of Logical Empiricism, leaving behind meaningless Wittgensteinian “elucidations”.²⁰ In Carnap's turn

17 The important exception is the discussion on protocol sentences: Neurath 1932/1983, resp. Carnap 1932/1987.

18 Carnap 2022b, entry from March 22, 1934.

19 “Und nun bin ich, überlastet durch andere Arbeit, im Hintertreffen, da ich nicht so gut ziselieren kann wie Du oder Frank oder Schlick. Alles kommt etwas grob heraus. *Und ich weiß das*” (Neurath to Carnap, October 9, 1932, ASP-RC 029-12-24).

20 Cf. Neurath 1936/1981, p. 697.

to semantics, however, Neurath saw a relapse into “absolutism”, a gateway for a revival of metaphysics in new clothes. In his characteristic style, he diagnosed Carnap as “Tarskized with some Aristotelian flavour”.²¹ Carnap, in turn, empowered by his newly invented “principle of tolerance”, insisted more than ever before that logical construction was an essential creative and innovative task of the philosopher-scientist, where philosophy was simply replaced by “logical analysis of language”. Amplified by divergences being caused by a clash of cultures between the highly esteemed professor at the University of Chicago and the much less successful private scholar at Oxford, the conflict reached its climax after Carnap’s extremely critical reaction to Neurath’s long-awaited contribution to the *International Encyclopedia of Unified Science* (Neurath 1944). In this monograph, all the major weaknesses of Neurath’s writing about which Carnap had so often complained – lack of clarity and comprehensibility – allegedly erupted again with full force. Carnap’s refusal to sign as an editor for this volume caused the deepest crisis in their relationship, which tragically could not be reconciled until Neurath’s sudden death in December 1945.

After a long period, in which scholars talked about Logical Empiricism only in order to criticize the alleged shortcomings of this tradition – radical foundationalism and reductionism that fell short of all kinds of empiricist “dogmas” –, the 1980s brought a wave of renewed interest and led to a thorough historization and development of more balanced accounts of the Vienna Circle, starting with pioneering collections such as Dahms 1985, Rescher 1985, Uebel 1991, and Bell and Vossenkuhl 1992. This reassessment is still in full swing, both in terms of philosophical-historical embedding and systematic discussion of individual approaches. In Europe Rudolf Haller and his collaborators were a driving force in the first phase of the rediscovery;²² the *Vienna Circle Institute*, co-founded by Friedrich Stadler and directed by him for many years, became a centre of research after 1991.²³ On the other side of the Atlantic, J. Alberto Coffa’s major monograph was a kind of initial spark, followed by works by Michael Friedman, Richard Creath, Alan Richardson, and others.²⁴

21 Neurath to Carnap, April 1, 1944, ASP-RC 102-55-08. Neurath identified another source of bad influence (beside Tarski) in Popper.

22 In addition to the publication of the first volumes of Neurath’s *Gesammelte Schriften* (see below): Haller 1986, Haller 1993, and Haller and Stadler 1993.

23 Of Stadler’s extremely numerous publications, we confine to Stadler 1997. The two series *Publications of the Vienna Circle Institute* and *Vienna Circle Institute Yearbook*, founded by Stadler, are still central organs of relevant research today.

24 Coffa 1991, Giere and Richardson 1996, Richardson 1998, Friedman 1999, Hardcastle and Richardson 2003, Friedman and Creath 2007, Richardson and Uebel 2007; also worth

In the meantime, research in this field has flourished and taken on dimensions that make it difficult for the individual to keep track.²⁵ In line with the state of research, most recent publications no longer deal with the Vienna Circle as a whole, but concentrate on individual thinkers or specific problem areas.²⁶ The present volume also follows this approach, focusing on the relationship between Carnap and Neurath. The conception of this volume gained momentum through the editors' work on philosophical editions that had already begun, or at least been planned, some time ago. In the case of Neurath, these are, on the one hand, the long-awaited completion of Neurath's *Gesammelte Schriften*; the early volumes of this edition provided the basis for the rediscovery of this thinker.²⁷ On the other hand, the publication of the correspondence between Carnap and Neurath was already planned in the 1980s by Rainer Hegselmann²⁸ and is now being pursued jointly by the editors of the present volume.²⁹ Another current project is the edition of Carnap's diaries, the first two volumes of which have just been published by Christian Damböck.³⁰ The activities of the editors have not only provided the immediate impetus for the organisation of the conference on which this volume is based, but have also, in the course of these projects, made available the unpublished materials to which almost all the contributions in this volume refer.

The present volume is divided into three parts. The papers in the first part deal with aspects of the prehistory and the influences on both Carnap and Neurath before their first meeting in Vienna. It is now well known that, in addition to the work of Frege and Russell, Carnap was influenced by Neo-Kantian and other German philosophical currents. Recently, the perspective

mentioning are Cartwright, Cat, Fleck and Uebel 1996, Parrini, Salmon and Salmon 2003, Uebel 2007.

- 25 The most up-to-date overview is provided by Uebel and Limbeck-Lilienau 2022.
- 26 Publications focussing on Carnap and/or Neurath: Awodey and Klein 2004, Carus 2007, Wagner 2009, Symons, Pombo and Torres 2011, Creath 2012, Cat and Tuboly 2019, leaving aside publications that deal with non-philosophical aspects of the extremely versatile Neurath, especially his work on visual education (Isotype), economics, and sociology.
- 27 The first five volumes of this edition, long out of print, have been reprinted and supplemented by three newly edited volumes. The whole series is now available as *Otto Neurath – Gesammelte Schriften*, Vienna, LIT Verlag, 2021–2022. Carnap 2019 is the long-awaited first volume of his *Collected Works*.
- 28 Cf. Hegselmann 1985.
- 29 A selection of this correspondence is to appear with Meiner Verlag, Hamburg. A first draft of the complete correspondence is available online in the *Virtual Archive of Logical Empiricism* (VALEP): <https://doi.org/10.48666/872268>.
- 30 Carnap 2022a and 2022b; further volumes are in preparation. Also in preparation is an edition of the entire scientific correspondence of Carnap.

on these philosophical influences has been supplemented by a closer look at the German Youth Movement.³¹ Michael Heidelberger adds another element to these “archeological vestiges”, namely the influence of Herbartianism and Pietism transmitted by Carnap’s grandfather, the influential pedagogue Friedrich Wilhelm Dörpfeld. As Heidelberger shows, these views clearly shaped Carnap’s noncognitivism, but they also left their mark on his philosophy of science. Carnap mentions reading his grandfather’s work *On Ethics* several times in his correspondence. In his diaries, as late as 1964, Carnap notes the discovery of strong convergences between his noncognitivism and his grandfather’s philosophy.³² There can be no doubt, then, that we have here another important, though hitherto almost unexplored, influence on Carnap’s thought.

The influence of Ernst Mach on most of the members of the Circle has long been acknowledged and was often stressed by Neurath himself.³³ It is also manifested in the name *Verein Ernst Mach*. Much less well known is the influence of Richard Avenarius, although Neurath occasionally refers to him. Mach himself described the affinity of his views to those of Avenarius as “as great as can possibly be imagined where two writers have undergone a different process of development, work in different fields, and are completely independent of one another”.³⁴ Lucas Baccarat examines the relation of Neurath’s criticism of the correspondence theory of truth to Avenarius’ criticism of “introjection”, with the result that the former can be seen as a linguistic version of the latter. This is not only a proof of ancestry, but also serves to elucidate the often misunderstood position of Neurath and is therefore a prerequisite for understanding the later debate on truth with Carnap: Neurath accused Carnap of making the “mistake of introjection” in adopting semantics.

Apart from philosophical influences, the main sources of Logical Empiricism were the newest developments in science, especially physics. Recent reconstructions of Logical Empiricism’s borrowings from physics tend to focus on Einstein’s theories of relativity and, to a lesser extent, quantum mechanics. As Jordi Cat’s paper points out, this limited focus loses sight of the various important commitments to Maxwell’s contributions to physics that can be found in both Carnap and Neurath. According to Cat’s insightful paper, since the establishment of connections to Maxwell was significantly different in each case, it

31 Cf. Damböck, Sandner and Werner 2022.

32 Carnap (in preparation).

33 Still in one of his last papers Neurath calls himself “a hard-boiled Machian” (Neurath 1946/1983, p. 237) and states that “[m]any of us, besides myself, have been brought up in a Machian tradition, e.g., Frank, Hahn, von Mises” (Ibid., p. 230).

34 Mach 1906/1959, p. 46f.

makes sense to speak of a plurality of “electromagnetic ways” of the scientific world conception.

Written mainly in 1925,³⁵ Carnap’s *Aufbau* marks the exact state of Carnap’s thought when he entered the Vienna scene. There is little doubt today that this massive endeavour has proven to be flawed in various ways, but the exact nature of the flaws is still a matter of debate. Touching on a crucial point in the debate, Thomas Uebel asks in his contribution to this volume whether a “structuralist” reading can save the *Aufbau* from the charge of reductive failure. Uebel answers the latter question in the negative, arguing that Carnap’s methodological solipsism, on any reading, is based on a faulty assumption about the epistemological order of human cognition. Uebel’s systematic approach corresponds to the historical course of events, since Neurath vehemently rejected methodological solipsism from the beginning.

Part 2 covers the most intense phase of philosophical collaboration between Carnap and Neurath, marked by the concerted publication of substantial papers on physicalism and the protocol sentence debate.³⁶ The popular narrative of the interaction between Carnap and Neurath sees Neurath in the role of the energetic inventor, pushing forward new ideas which were then clarified and elaborated in detail by Carnap. On closer inspection, this narrative appears to be oversimplified, for it obscures important areas of disagreement between Carnap and Neurath. Neurath, for example, tended to use the term “physicalism” to denote a comprehensive attitude (“Gesamthaltung”)³⁷ rather than the single, well-defined thesis of complete translatability of every proposition into a physical proposition. As Gergely Ambrus argues in his paper, for Neurath a purely phenomenal, private language would be meaningless in a strict sense. To support this claim, Neurath develops a “private language argument” (its relation to Wittgenstein’s famous argument is also discussed by Ambrus). Carnap, on the other hand, accepted the possibility of private languages as such, and based his argument on the uselessness of untranslatable phenomenal reports.

This leads directly to the problem of protocol sentences. Not only are they a crucial touchstone of physicalism (in the narrow sense), but the question of the nature and function of protocol sentences “comprises the questions dealt with under the terms ‘empirical foundation’, ‘testing’ and ‘verification’”.³⁸ The focus of Joseph Bentley’s paper is on the unfolding of Neurath’s theory, which

35 For the history of origins of the *Aufbau*, see Damböck 2021.

36 Carnap 1931/1934, Neurath 1931/1983.

37 Neurath to Carnap, June 21, 1935, ASP-RC 029-09-45.

38 Carnap 1932/1987, p. 457.

at its core consists of a seemingly strange, “interlaced” formulation of protocol sentences. It is not surprising that Neurath’s formulation provoked strong reactions from, e.g., Russell and Carnap. However, these criticisms are at least partly based on misunderstandings, as Bentley argues. Closely following the now classic interpretation of Thomas Uebel, Bentley points out that Neurath’s conception is not at all in conflict with Carnap’s mature view, but rather that they form complementary parts of a “bipartite meta-theory”.

The emergence of Carnap’s conception from the *Aufbau* is examined in detail in Johannes Friedl’s contribution. The gradual development is initiated by the departure from the *Aufbau* and fuelled by the contributions of Neurath, Popper and Schlick as well as by the emergence of Carnap’s meta-philosophy. Examining this mature conception, Friedl concludes that Carnap was not entirely successful in getting rid of traditional epistemological problems.

The discussions between Carnap and Neurath on the different conceptions of physicalism and of protocol sentences never led to a state of mutual agreement; nevertheless, these issues were replaced as a hot spot of controversy by the debate on semantics, especially the notion of truth, which lasted from the early thirties until Neurath’s death. Ulf Höfer and Hans-Joachim Dahms examine this debate, which took place almost exclusively in the correspondence and other unpublished sources. Höfer focuses on the exchange surrounding the conference in Paris in 1937 (Carnap’s last visit to Europe for decades) as a first climax of the debate. It was at this time that both positions were first elaborated in some detail, leaving behind the somewhat superficial debate on the name “semantics”. Dahms focuses on the debate on Russell’s *Inquiry into Meaning and Truth*, much of whose exact wording is discussed in the correspondence. This exchange with Neurath as prosecutor and Carnap as reluctant defendant, is unique in its level of detail, and takes on added significance since Neurath’s accusations of Russell’s “Aristotelian metaphysics” could easily be understood as directed against Carnap as well. Dahms concludes by drawing lessons from the current controversy on “fake news”. Like Höfer, Dahms sees the controversy on semantics as revealing a deep discrepancy with far-reaching consequences.

Part 3 of this volume deals with several other contexts and controversies that framed the interactions between Carnap and Neurath from the 1930s onwards. Christoph Limbeck-Lilienau examines the reactions of Carnap and Neurath to the *Tractatus*. It is well known that Neurath was from the outset extremely negative about the “metaphysical-mystical” aspects of the *Tractatus*, but it is another matter to identify the exact points of disagreement. In the case of Carnap, the relationship is more complicated; on his way to *The Logical Syntax of Language*, he tried to hold on to central insights of the *Tractatus* while at

the same time overcoming unacceptable limitations. As Limbeck-Lilienau shows, there may have been some overlap between the “middle” period of Wittgenstein’s thought and Carnap’s attempts to get rid of what Carnap himself called the “absolutist” conception of language.

Another aspect of the relationship with Wittgenstein is explored by Christopher Burke and Günther Sandner. Far from being a mere means of subsistence, Neurath himself saw *Isotype* as complementary to philosophical work, reflecting on the educational, social dimension of the scientific world conception. As such, the close affinities with the theoretical, doctrinal side are explored by Burke and Sandner, including an examination of the similarities and dissimilarities between Neurath’s pictorial language and the *Tractatus*’ picture theory.

Based on a close study of Carnap’s diaries and the Carnap/Neurath-correspondence, Friedrich Stadler sheds light on Kurt Gödel’s role in the Vienna Circle and later in the United States and corrects the widespread view of Gödel as a strange outsider and lone thinker, connected to the Vienna Circle only by chance.

The political and pedagogical dimensions of the scientific world conception are also examined in the article by Adam Tuboly. In 1944, together with Joseph A. Lauwerys, Neurath published ideas on the re-education of Germany after the war. The thesis of totalitarian tendencies in Plato, as put forward by Neurath/Lauwerys, met with rejection from a broad front of the British intelligentsia – with the important exception of Bertrand Russell – in defence of Plato. Tuboly analyses this increasingly surreal debate and Neurath’s efforts to uncover the roots of totalitarianism, an enterprise that had always been a driving motive for this “undaunted fighter who dreamt of a better and more humane world”.³⁹

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39 Popper 1973, p. 56.

borrowed from a paper by Neurath. We hope that the first chronicler of the Vienna Circle would have been pleased to learn that his and his allies' visions are still in flux.

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PART 1
Prehistory



Between Pietism and Herbartianism: Archaeological Vestiges in Carnap's Thought

Michael Heidelberger

Abstract

It is well known that the philosopher Rudolf Carnap (1891–1970) was influenced by his maternal grandfather, the pedagogue Friedrich Wilhelm Dörpfeld (1824–1893). Although Dörpfeld had already died when Carnap was two years old, Carnap's mother Anna née Dörpfeld (1852–1924) who was thoroughly imbued with the views of her father transmitted many of his ideas and attitudes to her son. There is already some relevant literature on the early (philosophical and other) influences on Carnap, but our knowledge of this period of Carnap's life is still not wholly satisfactory. One of the striking facts that has recently come to the fore is that the earliest sources of Carnap's youthful views relevant for his later philosophy rarely seem to belong directly to theoretical philosophy proper but more to practical philosophy, i.e., questions of the way of life, of religion, of ethics, of education and of psychology.

Gottfried Gabriel has recently put forward what one could call the “Jena Thesis”: “Carnap's early philosophy [...] can be regarded as a configuration of influences – a cross-fertilization of modern logic, neo-Kantian constitution theory, and *Lebensphilosophie*” – a configuration that was “highly specific” to Jena in the early twentieth century where Carnap started his studies (Gabriel 2004, p. 6; cp. also Damböck et al. 2022 emphasising the youth movement also considered by Gabriel and by Dahms 2021).

In this chapter, I would like to supplement this claim by a “Ronsdorf Thesis” namely that the roots of Carnap's early philosophy stretch even farther into the past to the Pietism of his religious upbringing¹ but also importantly to the Herbartianism of his grandfather.² The thesis does not contradict the Jena Thesis, it puts it in a broader context resembling archaeological claims because the evidence one can adduce for it is mostly only indirect and relies on certain similarities of Carnap's thought

1 This view is shared by André Carus, e.g., in his 2022, and to some extent also by Gabriel.

2 Gabriel notes Dörpfeld's Herbartian orientation and acknowledges Herbartianism as an important cultural element in Jena at the time but alludes only indirectly, via Gottlob Frege, to an influence of this on Carnap (Gabriel 2004, p. 18 f.; 2017, pp. 54–58).

with the views and values of his upbringing in Ronsdorf (and the nearby Barmen from 1898). Of course, Carnap did not just assimilate Dörpfeld's ideas but accommodated them in the face of his own new experiences – it appears to me mainly to the new logic.

After some information on Carnap's ancestry in the first part, I shall have a detailed look in the second part at the philosophy of Johann Friedrich Herbart to which Carnap's grandfather enthusiastically adhered. In the third part I intend to deal with Dörpfeld's treatise *On Ethics*, which constitutes the most philosophical and original of his writings. The fourth part presents the striking resemblance of some of the views of Dörpfeld and Herbart with central aspects of Carnap's early work. An important point made here is that Carnap's programme of "explication" turns out in the end as an inner-Herbartian move, another one that Carnap learned his non-cognitivism in ethics from Herbart and Dörpfeld.

1 Carnap's Family and Pietist Roots

Carnap's forebears are rooted in the so-called *Bergisches Land* located east of Düsseldorf along the Rhine (Carus 2007, ch. 1). In the 18th century, this area, formerly the dukedom of *Berg*, belonged most of the time to the (catholic) Electorate Palatinate. From 1815/1822 to 1946 it was part of the Prussian province called Rhenish Prussia or Rhine Province. Today it is an area of the State of North Rhine-Westphalia in Germany. The inhabitants of this region were mainly Protestants of the Reformed Church, a denomination ultimately derived from the French/Swiss reformer Jean Calvin.

Carnap was born in the town of Ronsdorf that was founded by a group of Reformed Protestants from Elberfeld in the early 1740s with some of Carnap's ancestors among them (Carnap 1957, A-5 f.; cp. Carus 2007, p. 48). They left Elberfeld, then the biggest town in the area, in the late 1730s in order to flee the sinful "Babel" and to live together in the "New Jerusalem" as a religious and 'communist' unity of brethren. Their decision was also influenced by the wish to get rid of a suppressive and rigid religious (protestant) orthodoxy that rejected their views. Ronsdorf lies about five km to the south of Elberfeld, separated from it by a mountain. In 1929, both towns, as well as Barmen, eventually became absorbed in the city of Wuppertal, which was called the "German Manchester" because of the early and brutal industrial development in the area during the 19th century (Langewiesche 1863, p. 10).

The movement was led by a certain Elias Eller (1690–1750) and his second wife Anna Catharina vom Büchel (1698–1743), a baker's daughter, who had ecstatic visions, revelations and "inspirations". (For the history of Eller's sect

see Engels 1826, Goebel 1860, pp. 448–598 and Langewiesche 1863, pp. 184–199.) The couple claimed that their son was the new messiah and Christ reborn. Although he died about 17 months after his birth, his death did not diminish the zealotry of the sect. In Elberfeld, the group had already formed a so-called “Philadelphian Society” around 1730.³ In Ronsdorf, the members called themselves “Zionites” and were also known as “Ellerians”.⁴ Eller and his wife were worshiped with great devotion by their followers, but their leadership quickly evolved into a dictatorial regime that did not tolerate any disobedience in religious or worldly matters. Eller became the first mayor of Ronsdorf and had also judicial powers.

The first preacher the sect called to Elberfeld in 1729 (and later to Ronsdorf in 1741) was the grandfather of the influential philosopher and theologian Friedrich Schleiermacher.⁵ We will come back to him later in other contexts. Among the “immigrants” from Elberfeld to Ronsdorf was a certain Wilhelm Caspar Carnap (1679–1749), great-great-great-grandfather of Rudolf Carnap, who, probably through the influence of Daniel Schleyermacher (1697–1765), moved to Ronsdorf in 1742 (Kaufmann 1974, pp. 114–118 & pp. 130–134; Carnap A. 1926, p. 8).

After the early death of the *Zionsmutter* (Zion’s mother) Büchel in 1743 Eller himself took over the leadership and the movement started slowly but steadily to disintegrate. Ronsdorf lived in constant and serious conflict with all neighbouring church congregations as well as with the authorities of the Reformed Church and suffered from internal disputes and fights. The

3 The first Philadelphian Society was a theosophical sect in England founded in 1670 by the mystic Jane Leade (1623–1704) and some English adherents of Jakob Böhme (1575–1624). It claimed to be the New Philadelphia according to Rev. 3, 7–13. The Philadelphian movement regarded Christian “true belief” as beyond the confines of denominations and promoted apocalyptic and chiliastic ideas.

4 The movement arguably belonged to “Radical Pietism”, which in comparison with “Pietism” (without epithet) supported “separatism”, i.e., the tendency to separate from and even to breach with the church. The important Herrnhut-Zinzendorf movement of Pietism had its heyday at about the same time as the Zionites. The term “Pietism” is often used in a non-specific, figurative and pejorative sense for a protestant attitude stressing mysticism, excessive piety, revivalism, exaggerated religious enthusiasm, puritanism, chiliasm, bigotry and anti-rationalism. Nevertheless, pietistic attitudes also played an important positive role during the enlightenment: A pietist resists patronisation in religious matters by official creeds. But, alas, it does not by itself protect her or him from sectarianism or superstition.

5 It was only in 1787 that Friedrich’s father Gottlieb informed his 19-year-old son in a letter of the involvement of his grandfather Daniel with the Ronsdorf sect and asked him not to talk to anyone about it except his uncle (Schleiermacher 1985, pp. 88–90). Friedrich Schleiermacher is reported to have said years after that Ronsdorf still haunts him (Krafft 1890, p. 481).

mentioned preacher, Daniel Schleyermacher, gradually became suspicious of the sect. However, it managed to survive for another 25 years because of several factors: Eller as an eminent power seeker had excellent relations with the sovereign powers (bribery seemed to have played a role: Goebel 1860, p. 515, p. 540, p. 560; Langewiesche 1863, p. 193) and obtained a strong autonomy for his religious community (and thus for himself) as well as for the new town. He also managed to infiltrate the area's general synod of the Reformed Church with two members of his sect who received life-time seats and votes. Besides Schleyermacher, an extremely gifted propagandist was recruited as a second preacher in Ronsdorf who managed to receive high offices in the reformed church of the area.

Another reason for the perseverance of the movement was economical. Eller had taken over the ribbon factory of his first wife and brought it to Ronsdorf where it flourished. It formed the core of a rapid and on the whole lasting economic upswing of the town. There were in 1819 1100 looms for the production of silk-bands in operation. Eventually, Schleyermacher broke with the Ellerians in 1749 when their relationship after the death of Büchel culminated in mutual estrangement. He was accused by Eller and the second preacher of being a sorcerer for which he was persecuted by state agencies and had to flee in a shambles (cp. Krafft 1890). The sect was eventually excluded from the general synod in 1752.

There are indications that Wilhelm Caspar Carnap's descendants remained loyal followers of the sect for quite a time; the four younger of his six children were confirmed by Schleyermacher (before his breakup with the sect) and the step-son and successor of Eller, Johannes Bolckhaus, and his brother Jakob acted as godfathers of the great-grandfather of Carnap, Johannes Elias Carnap (1754); there is a high frequency of "Elias" and "Anna" as first names among the Carnaps – Carnap's grandfather (1789–1859) was called Elias Sebulon (Kaufmann 1974, p. 114 f.).

In 1768, after a painful process, the religious community of Ronsdorf was officially united again with the general synod of the Reformed Church of the area. The gulf between the Lutheran and the Reformed Church existing at the time (Langewiesche 1863, pp. 170–180, p. 199) slowly started to close when the Prussian King Friedrich Wilhelm III united the two denominations by law in Prussia in 1817. Yet repeated attempts to implement this unification failed in Ronsdorf (and elsewhere), even until today.⁶ Max Goebel, one of the chroniclers of the history of the Ronsdorf sect, wrote in 1860: "The

⁶ Ronsdorf is unique in still comprising two different religious communities as part of the official united Protestant Church of the Rhineland: a Reformed one and an "evangelical"

chiliastic-Philadelphian *Schwärmerei* [in Ronsdorf] has since then admittedly not any more *erupted*, but it still haunts as a shy spectre in the community to this very day” (Goebel 1860, p. 598; similarly Langewiesche 1863, p. 199).⁷ The Ronsdorf ribbon-weaving industry flourished until the middle of the 20th century. In 1900, up to two thousand looms were being operated by homeworkers and about thirty weaving factories still existed. Until WWI most of the hatbands of the world came from Ronsdorf; also, typewriter ribbons and many other bands were produced.

Carnap’s father Johannes Sebulon⁸ Carnap (1826–1898), born and deceased at Ronsdorf (as all of Carnap’s paternal forebears since the days of Eller), was a prosperous inkle-weaver with a “small but well-established ribbon factory” founded in 1861 (Carnap 1957, A-2; see also Carnap A. 1926, pp. 55–63). He had to quit school at the age of ten to work in the weaving business as did his forebears. He educated himself and became a highly respected personality with an independent mind. He held many important public and church offices in his community, among them Presbyterian of the Reformed Congregation. In 1887 he entered a third marriage with Anna Dörpfeld. Carnap had an older sister (Agnes Kaufmann, 1890–1976, photograph in Kaufmann 1974, p. 128) and twelve half siblings of his father’s side of whom only six survived their father (Kaufmann 1974, p. 132).⁹

Carnap’s mother became a schoolteacher like her father. Sixteen-year-old Carnap wrote in his diary that his mother was *philosophisch angelegt*, “had a disposition for philosophy” (10. III. 1908 = Carnap 2021, p. 70). She devoted her life to expounding the views of her father who had lived many years of his life in her household. In his *Intellectual Autobiography* (cp. Siegetsleitner 2019), Carnap further reported that when he was a child, “my mother worked for years on a large book describing the life, work, and ideas of her late father” (Carnap 1963, p. 3). The book came out in 1897 and contains many letters and

(formerly “Lutheran”) one. Reformed Protestants differ from Lutherans mainly on theological issues like transubstantiation and predestination, among other things.

7 In 1839, the 18-year-old Friedrich Engels, native of Barmen (today Wuppertal), wrote the whole hatred off his chest against the vulgar Pietism of his hometown (and of his father) in his vivid *Letters from the Wuppertal*. They appeared in Karl Gutzkow’s *Telegraph für Deutschland* under a pseudonym and caused quite a stir (Engels 1839).

8 “Sebulon and Josua, Esther and Rebecca, these are the names one still can find among people of Ronsdorf, even if they are today inhabitants of Wuppertal” (Anonymous 1951). Cp. also Carnap A. 1926, p. 13.

9 Carnap himself counted 11 siblings (Carnap 1957, A-2) and Anna Carnap lists 14 (Carnap A. 1926, pp. 11, VII, VIII). For Carnap’s family see also the introduction to Carnap 2021, pp. 31–37.

other documents besides the biography of Dörpfeld. Although it comprises over 600 pages, a second (slightly reworked and shortened) edition became necessary soon after in 1903 (Carnap, A. 1903; photograph of Dörpfeld in the frontispiece. Cp. also the review of Lukens 1897.)

It is interesting to learn from this book that Dörpfeld (born in Wermelskirchen-Sellscheid, 10 km to the south of Ronsdorf) was tutored mainly by his grandparents. His maternal grandmother came from a pastor's family and is reported to have formed his soul and emotional life. The maternal grandfather, hence, Carnap's great-great-grandfather, was a siamose-weaver and simple peasant. (On the Dörpfeld family see also Kaufmann 1974a, pp. 157–176; Goebel 1995, p. 150.) He founded a circle of interested neighbours who came together during evenings to read and discuss philosophical, scientific, mathematical and geographical works “in sparse lamplight and doing ample justice to tobacco smoke”. Young Friedrich Wilhelm was introduced to this circle from early on. It was here that a “searching philosophical sense was awakened in the boy” (Carnap A. 1903, pp. 16–18; see also p. 33 f.).

The Dörpfelds were Lutherans and therefore arguably not directly related to the Ronsdorf brand of Pietism. In a brief overview of the religious history of the Rhine Province, Dörpfeld later stressed the role of 18th century Pietism in revivifying the quarrelling, stiff and even, as he wrote, “drowsy” orthodoxy (Dörpfeld 1860). It is curious that he altogether ignored here the Zionite episode – also in other writings. Dörpfeld pointed out that towards the end of the century “Rationalism” started to gain a strong foothold in theology and Pietism withdrew from the scene. Only after the Napoleonic Wars (1800–1814), he wrote, a milder Pietism resurged in the area without however the liveliness of the earlier movement, remaining somewhat dull and often uninteresting (Dörpfeld 1895, p. 14 f.).

As an antidote, Dörpfeld propagated the “deep mysticism” of the religious writer and local hymnist Gerhard Tersteegen (1697–1769) and the “biblical-theosophical profundity” of the Swabian school of Pietism. He found an attractive reconciliation of these two traditions with each other in the teachings of the lay theologian and physician Samuel Collenbusch (1724–1803) – a local Pietist and harsh critique of Kant's ethics and philosophy of religion (*ibid.* See also Carnap A. 1903, pp. 276–282, p. 287 f.). Yet, he never officially identified himself with Pietism of any form and refused a rigid definition of his own Protestant affiliation propagating an eclectic and independent approach instead (cp. Carnap A. 1903, pp. 276–282, p. 35). He characteristically said that “freedom outranks ‘pure doctrine’” (Carnap A. 1903, p. 344).

From early on, Dörpfeld wanted to become a teacher and was soon assigned by his schoolteacher as an assistant for instructing the lower classes of the

school. He entered a teachers' seminary already at the age of sixteen where he met an inspiring instructor. During this time, he came into contact with the philosophy of Friedrich Eduard Beneke (1798–1854), which he studied eagerly (cp. Dörpfeld 1875, p. 144; Carnap A. 1903, pp. 341–347). Only four years later he himself became a teacher at another seminary. That was the time when, through the writings of the social pedagogue Karl Mager (1810–1858), a compatriot of his, he encountered the work of another philosopher who accompanied his thought for the rest of his life: Johann Friedrich Herbart.¹⁰ He thus became one of the first followers of Herbart's pedagogy and later one of the most influential ones.

The pedagogical movement of Herbartianism started as a reform programme around the middle of the 19th century and, at least until WWI, turned into a virtual mass movement among German teachers, which somewhat diminished after the turn of the century. (It also had many followers in the US, up until at least WWI.) It was directed against arbitrariness in teaching and mere rote learning in the schools. Scientific psychology was thought to furnish a scientific foundation for teaching. One of the leading American Herbartians, Charles de Garmo, aptly observed that the “burning questions” of Herbartianism pertain, “first, to the selection and sifting of suitable subject-matter in the various studies; then to its rational articulation or coördination; and finally, to the truest and best methods of teaching it to the child” (De Garmo 1895, p. vi). This movement of “Herbartianism” should be distinguished from Herbartian philosophy proper in a narrow sense and also from Herbart-inspired *Völkerpsychologie* (approximately: psychology of ethnic communities) – although there are some overlaps. These movements took up different elements of Herbart's thought and were considerably heterogeneous for their part. All three Herbartian movements started to gain importance beyond local boundaries only after Herbart's death.

In 1851 Dörpfeld married the daughter of a Lutheran pastor, Christine Keller (1824–1871), who bore to him six children – among them the son Wilhelm (1853–1940), hence Carnap's uncle who studied architecture and later became

10 He later wrote: “From then on [i.e., the encounter with Mager] I became a grateful and faithful Herbartian and remained so to this day” (Dörpfeld 1884, p. xxiv). Gabriel (2004, p. 18 f.) erroneously claims that Dörpfeld's “religious pietism [...] estranged him from Herbart's specifically religious ideas.” However, religion plays almost no role in Herbart's work and is not pronounced at all. Some friends wanted Dörpfeld to distance himself from Herbart because of an alleged incompatibility of his ethics with religion. Dörpfeld countered that he felt on the contrary “fortified” in his religious opinions by Herbart's ethics (Carnap A. 1903, p. 341).

a famous and influential archaeologist in Turkey and Greece after having first continued Heinrich Schliemann's excavations in Troy.¹¹ In 1857, Dörpfeld sen. founded a popular (first regional, then national) journal for protestant teachers in a Herbartian spirit, the *Evangelisches Schulblatt*, and managed it until his death.

Dörpfeld stood out for the deep scientific interest in education and psychology. He had been a board member of the *Association for Scientific Pedagogy* since its foundation in 1868 and committed himself to many associations for teachers, regularly organizing local and regional conferences, also in bible study and religious instruction (Carnap A. 1903, p. 282 f.). His lecture and publication activities grew steadily. He was a typical *Schulmann*, as one said in the 19th century: a largely self-educated pedagogue and instructor emphatically raising his voice in the public on educational matters and being devoted heart and soul to his profession. He was called "king of principals" and "master of the art of teaching". People said that he could easily have filled a university chair for pedagogy.

In 1872, the new school-minister of Prussia under Bismarck, Adalbert Falk, initiated a wide-ranging school-reform ("Die allgemeinen Bestimmungen") to whose preparation committee in Berlin Dörpfeld was appointed as the only elementary school teacher (among many directors of seminaries and school inspectors).¹² At this meeting, which lasted over a week, he strongly (but unsuccessfully) advocated the introduction of *Gesellschaftslehre* (civic studies, social teaching) as a new school subject (Goebel 1995, p. 267 f.). However, the committee was successful in introducing psychology as a subject in the formation of teachers and Dörpfeld subsequently gave several (mostly triennial) courses on Herbartian psychology on a regular basis attracting many teachers from near and afar. Incidentally, he dedicated the first year of the course to logic (Dörpfeld 1884, p. xxiv–xxvi, Carnap A. 1903, p. 269). The fruit of these courses was a popular booklet on *Thought and Memory*, an introduction to Herbartian psychology especially for educators that built on a small pamphlet from 1866 and received fourteen editions until 1915 (Dörpfeld 1884)! The booklet includes a concise treatment of mathematics instruction, especially of the role of intuition and memorization in this field, with an interesting discussion of the nature of numbers as relational concepts (Dörpfeld 1884, pp. 145–151).

11 Rudolf Carnap visited him twice in Greece, in 1905 and 1910. Cp. Carnap 2021, pp. 34, 37, 75, 515 and ill. nr. 15.

12 His account of the meeting and other material related to it is reprinted in Carnap A. 1903, pp. 260–266.

The topic that was dearest to the heart of Dörpfeld was the constitution of the schools. He wrote four books about this question and argued that the schools were primarily neither in the responsibility of the state or the community, nor of the church but of the parents. The legal basis should be anchored in family law and the local school communities. He worked out a detailed self-government organisation scheme by considering the wider philosophical discussion on civil society and the state. His ideas were unfavourably received: the conservatives disliked the loss of power for the church and the liberals frowned upon the blurring of the rules of the bourgeois political power Dörpfeld's model envisaged.

Unfortunately, Dörpfeld had to leave the profession already in 1880 for reasons of health after having spent 31 years as principal of a Lutheran school in Barmen-Wupperfeld in the area. An English Herbartian, Frank Herbert Hayward (1872–1954), who studied some time in Jena with the Herbartian Wilhelm Rein (and most probably had met Dörpfeld in person), called him “the greatest and wisest of Herbart's followers” (Hayward 1904, p. 88; cp. also p. 57 and Hayward 1903, p. 35). Dörpfeld's publications are collected in twelve volumes (1894–1911) and most have seen a second edition; two are on pedagogical psychology, three on special didactics, one comprises his addresses, three are on school organization, two on miscellaneous subjects and one volume on ethics that will occupy us in more detail below.

The foregoing account of the Pietist roots of Carnap's family shows first that having been decisively marked by Pietism was not just an anecdotal and harmless side issue as it appears from Carnap's *Intellectual Autobiography* – also not in a philosophical respect. And second, that for freeing himself from any irrational and anti-Enlightenment currents that must have affected him, and for striking out new paths in thought, Carnap could partly build on groundwork of members of his family, even though they were at the same time also (partially) transmitters of a problematic and sometimes even fanatical tradition.

2 The Philosophy of J.F. Herbart

The philosophy of Johann Friedrich Herbart (1776–1841) is, besides German Idealism and neo-Kantianism, the third dominant philosophical current of the German 19th century. Herbart studied at the University of Jena where he got enthusiastic about the lectures of the philosopher Johann Gottlieb Fichte (1762–1814). He started soon, however, to critically examine Fichte's philosophy and German Idealism in general. He finally came to reject Fichte's central assumption that the I (ego) posits itself:

It was an error of idealism, violent in its creation, and adhered to with equal violence that the Ego opposes to itself a Non-ego (Fichte), as if the negation of the Ego were inherent in objects. In this way a *thou* or a *he* would never originate – another personality than one's own would never be recognized.

HERBART 1816, p. 402, § 198/1891, p. 154 f. The name of Fichte was inserted here by the translator

It holds true what George Santayana wrote in 1889 that “[t]o Herbart belongs the leadership of the reaction against the Hegelian idealism” and we can add: against German idealism in general (Santayana 1889, p. 142).

After his studies in Jena, Herbart worked for some time in Switzerland as a private tutor for the children of a wealthy family. Mainly as a result of this experience and of shortly meeting the Swiss educational reformer Heinrich Pestalozzi (1746–1827), he developed a strong interest in pedagogical and educational matters. He became a *Privatdocent* and later professor for pedagogy at the University of Göttingen and began to devote himself to seriously working out an educational philosophical system. His fame spread and in 1809 he was offered the vacant chair of Immanuel Kant at the University of Königsberg with the new denomination of “philosophy and pedagogy”. He soon ventured there on the founding of a College or Seminar for the training of teachers. In 1833 he was called back to the University of Göttingen where he became one of the most popular and most respected professors. He died there at the age of 65 in 1841. During his lifetime, his philosophy did not radiate much beyond the local centres of his professional activity – but this changed, slowly but steadily. With the Austrian Thun-Hohenstein university reforms after 1848 mainly instigated by the philosopher Franz Serafin Exner (1802–1853), Herbart's philosophy even came to dominate university philosophy in Austria for a long time, and Herbartianism became the ‘official’ orientation of the Austrian educational system (Heidelberger 2004, p. 64).

One of Herbart's students in Göttingen, Karl Volkmar Stoy (1815–1885), founded a pedagogical seminary at Jena University in 1844, also with some kind of a lab or practice school associated with it. The Herbartian educator and university professor Wilhelm Rein (1847–1929) who was for a brief time a teacher at the school in Barmen where Dörpfeld was principal took over this institution in 1886. He developed it into a centre of worldwide reputation to which teachers flocked from all over the world, especially the United States,

in order to learn the most recent Herbartian methods of school instruction.¹³ The institution was still in full bloom when Carnap moved to Jena with his mother in 1909 to study also with Wilhelm Rein at the University.¹⁴ Jena had yet another Herbartian reformatory, an institution for therapeutic pedagogy (*Heilpädagogik*), which was initiated by a close friend of Dörpfeld, Johannes Trüper (1855–1921), in the early 1890s. A further pedagogical seminary with an associated practice school was founded in Leipzig in 1862 by the leading Herbartian Tuiskon Ziller (1817–1883), which lasted until his death.

2.1 *Concepts and Ideas*

According to Herbart, the task of philosophy is the reworking of concepts (*Bearbeitung der Begriffe*; Herbart 1813, p. 38 f., § 4; 1814, p. 324 and many other places), either their form or their content. Logic results from dealing with their form, metaphysics and aesthetics with their content. Metaphysics is the activity of correcting and extending concepts in order to avoid and dissolve inconsistencies. Aesthetics deals with the valuation of concepts, whether one approves or disapproves of them; it therefore includes also practical philosophy. In his metaphysics Herbart taught that the substrate of the world is constituted by so-called “reals” or “simple beings” (*einfache Wesen*) of simple (and single) unchanging qualities – noumena or space- and timeless metaphysical atoms, in some respect resembling Leibniz’s monads. They have the capacity of self-preservation against annihilation with which other reals threaten them. The soul as a real is a noumenon experiencing phenomena. It should be recognized that Herbart’s view of the “soul”, notwithstanding the terminology he used, was anything but traditional. He did not mean by “soul” a substance in the sense of Leibniz and Christian Wolff or of the Aristotelian and Cartesian traditions. On the contrary, his view led to a questioning of traditional mind-body notions. If here and there he used “substance” for the soul, he simply meant this as an alternative expression for a “real” or “simple being”.¹⁵

13 Together with the Botanist Wilhelm Detmer of the University, Rein organised successful international summer schools for continuing education of teachers from 1889 on. The school of 1913 for example counted 866 participants; half of them came from abroad (Werner 2003, p. 53). Sullivan 2016 reports that Gottlob Frege gave a talk “On the Concept of Number” in the 1890 session. On Herbartianism in the US see Graves 1914, pp. 207–220; Dunkel 1970; Cruikshank 1994.

14 Carus 2007, 45; Gabriel 2004, pp. 5, 9.

15 This is overlooked by Landerer & Huemer 2018. An early discussion and questioning of Herbart’s mind-body conception was given by the philosopher and physician Hermann Lotze (Lotze 1852, pp. 153–156).

The varying states assumed by the efforts of the soul at self-preservation are ideas or presentations (*Vorstellungen*). They are equated with concepts (or rather with their tokens, if “concept” is taken in the psychological sense). They are indestructible and, being in constant flux, appear in consciousness as a result of experience and of the soul’s acts of self-preservation. In the course of time an idea can be repressed by another one and sink below the threshold of perception in order to return under more favourable conditions. Ideas can also fuse or coalesce, wholly or in part. Consequently, Herbart was strongly opposed to the widely held “faculty psychology” – the view that the soul is composed of separate powers or faculties as for example the will, the emotions, and the intellect. The major source of this doctrine at the time of Herbart was the psychology of the philosopher Christian Wolff (1679–1754) from the 1730s, his *psychologia empirica*. Herbart thought that accepting such presuppositions turns psychology into a “mythology”: “The faculties will be banished from scientific psychology in the same way as phlogiston had to give way in chemistry” (Herbart 1816, p. 298/1891, p. 3). All the capacities usually attributed to faculties are now to be ascribed to concepts or rather to presentations, their combination, their action and interaction upon each other.¹⁶

However, Herbart observed, most concepts that are inevitable for expressing experiences lead to inconsistencies. They result, at least partially, from the necessity to regard things (including minds) as unchangeable and yet simultaneously as changing, i.e., as assuming different states in the course of time. In order to avoid such contradictions, we must distinguish between two modes of existence of a thing: its existence as appearance for us and its existence independent of it. In order to explain change in a consistent manner we must assume that whatever exists behind the appearances must have a forever unchanging nature and that change has its source exclusively on the level of the appearances. It is the relation of reals to other reals that gives the impression that things undergo changes: Things have thus to be seen as a collection of reals, and properties of things are to be exclusively thought of as extrinsic properties of the reals in relation to other reals (*Methode der Beziehungen*). Therefore, in order to explain why a thing or soul changes, we must assume that it participates in some antagonism in which it tries to retain its identity. Self-preservation is thus the only true change that can be thought of without contradiction.

16 On the general perspective on psychology at the time see Heidelberg 2004, *passim*. Herbart’s psychology is treated *ibid.*, especially pp. 31–35, 41, 64 and elsewhere.

Herbart's metaphysical treatment of souls and objects might strike us as strange today. It can and should be understood, however, as an empiricist attempt to draw a philosophical lesson from Fichte's and other idealists' unbridled handling of the transcendental ego. What Herbart calls "metaphysics" must be seen as the result of an analysis of the given that also considers linguistic considerations. "Without metaphysics concepts lack conceivability (*Denkbarkeit*). [...] The purpose of metaphysics is no other than to make those concepts conceivable [*denkbar*] that are imposed upon us by experience" (Herbart 1813, p. 208, § 126; see also p. 46, § 6). We find out that we are unable to infer from experience alone what really exists. At least in the first instance we must take experience as mere appearance that does not allow a conclusion about the true nature of the being underlying the phenomena. We have, however, to assume that there is a "real" beyond the appearance because otherwise we could not speak of things at all.

As soon as we try to describe the forms of our experience, Herbart argued, we again end up with conflicting concepts like the concept of matter: A material thing is a finite quantity of matter but at the same time a quantity of infinitely many parts that are themselves pieces of matter. Similarly, with the I: On the one hand the I or ego is something that remains identical in all its changing states of envisaged ideas. On the other hand, it is nothing but the sum of the different changing ideas that it knows as its own states. The I is thus both the entity that envisages ideas as well as the collection of ideas envisaged – a process that leads to an infinite regress.

In this situation we must again turn to metaphysics in order to break free from the contradictory world of mere appearance. Metaphysics in Herbart's sense is thus a necessary reform of given concepts in order to enable logical analysis. We must clear up the semantic properties of concepts before we can use them consistently. So, metaphysics in Herbart's sense is *not* intended as a theory on the fundamental nature of reality or the like.¹⁷ Understood in the right way, Herbart maintained, metaphysics does *not* transcend the realms of human experience but makes them *conceivable*.¹⁸ Metaphysics in a different

17 It is another question whether Herbart resolutely pursued this view and kept it up in all his pronouncements.

18 It is tempting to compare Herbart's metaphysics to Ludwig Wittgenstein's philosophy: Wittgenstein can be seen as someone who brushes away Herbart's approach with one stroke already at the beginning of the *Tractatus*: "The world is the totality of facts, not of things" (1.1). In this way there cannot arise any contradictions with "things", neither with material objects nor "souls". Herbart could have responded (quite in the spirit of the later Wittgenstein) that we do use the thing-concept in our way of life and cannot avoid

sense must be rejected. So, Herbart was right when he stressed the empiricist nature of his approach:

If someone attempted to stand on my shoulders in order to see farther than I, he would at least not have to worry that the soil under my feet would collapse. For I do not stand on the sole peak of the ego (as one could believe on superficial examination [and as Fichte did]) but my basis is as broad as experience in its totality.

HERBART 1824, p. x

Herbart's psychology would perhaps not be so interesting and influential (apart from opening a new perspective on the mind-body problem) were it not for a concept that he placed at its centre, the concept of apperception. (For the history of this concept that was highly important for Herbartians see the work of the Herbartian Lange 1894/Lange 1906.) The concept goes of course back to Leibniz who thought that weak perceptions (*petites perceptions*), although playing a significant role especially for most of our actions, lack distinct consciousness and remembrance. Strong impressions, however, or large aggregates of weak perceptions become conscious and remain long in memory – they are called apperceptions. We must distinguish “Perception, which is the internal state of the monad representing external things, from Apperception as the consciousness (*conscience*) or the reflective cognition (*connaissance réflexive*) of that internal state” (Leibniz 1714, p. 156, § 4). Animals can have some kind of apperception according to Leibniz, but human beings alone are capable of a true reflexive capacity. In this way, Leibniz rejected Descartes's opinion that every *perceptio* (perception) is a *cogitatio* (conscious thought), i.e., something “which we are aware of as happening within us” (Descartes 1644, p. 7, § 9).¹⁹

While adopting this, Herbart went a decisive step further than Leibniz by asking (in an educationist manner) how impressions or ideas can be raised to higher consciousness and thus be strengthened in their apperception. In order to answer this, one has, according to Herbart, to investigate the empirical laws describing the behaviour of ideas in different situations. One has specially to consider how new ideas are affected by previous experience. Herbart thus stressed recognition and understanding resulting from memory over mere sense-perception. Apperception is for him the mental assimilation of new

this manner of speaking. We should, therefore, try to make sense of it through metaphysics instead of putting ourselves outside of its use.

19 “Cogitationis nomine, intelligo illa omnia, quae nobis consciis in nobis fiunt, quatenus eorum in nobis conscientia est.”

ideas (outer or inner) with the help of ideas already possessed, the so-called “apperceiving mass of presentations” (*apperzipierende Vorstellungsmasse*).²⁰ If a new idea has an affinity to (part of) the ideas in memory it is attracted by them and repelled if it lacks this affinity. Without memory apperception of knowledge cannot take place.

Herbart is known for his claim that consciousness has a certain narrowness: From all the innumerable ideas that arise in experience only a limited number can be held in consciousness at a specific moment. Only those ideas remain in consciousness that are victorious in their struggle with other ideas. We must distinguish, he maintained, being in consciousness from being an object of consciousness: An idea can be in consciousness, yet at the same time remain under the threshold of being perceived. In this case it is not an object of consciousness (Cp. Herbart 1824, p. 218). One can nevertheless be affected by it as we often realize only after a suitable event has taken place. Herbart stressed the relevance of this for our actions: Very often we are not conscious of the real motive of our action which at best might dawn upon us only later.²¹

Herbart's concept of apperception allows an empirical application of his psychology to teaching and instruction and can thus serve as a bridge between psychology and pedagogy. It opens the possibility to order the teaching content into phases such that each phase is the precondition in memory for improving the apperception in the following one. It can also provide guiding principles for finding the adequate method of a certain teaching phase and of implementing a teaching content in an economical and effective way in order to avoid redundancy but also overload of subjects. In this respect, space is too limited here to give a better impression of the sophisticated work that was done by Herbart and his followers.²²

2.2 *Logic*

Herbart had a strong interest in logic (cp. Vilkkö 2002, ch. 3; Bellucci 2015; Gabriel 2001). After learning about his strong emphasis on psychology, one might expect him to have advocated a psychologistic conception of logic (and

20 “Assimilation” can be taken here quite in the sense of Jean Piaget’s “genetic epistemology”, his theory of the cognitive stages of child development. Piaget studied Herbart’s theory extensively.

21 Herbart’s thought had a strong influence in this and in other respects on Sigmund Freud (cp. Hemecker 1991).

22 It is important to take notice of the fact that Charles Sanders Peirce was heavily influenced by Herbart’s theory of apperception from early on culminating in his *The Law of Mind* of 1892. Peirce also stressed the superiority of Herbart’s apperception over associationism. See the instructive article by Bellucci 2015, pp. 83–91.

many still unfortunately continue to do so). Yet the contrary is true as the following typical quotation unmistakably testifies:

It is true that logic is concerned with representations [*Vorstellungen*]. But not with the act of representing; therefore, neither with the mode and the manner of how we get thereto [to this act] nor with the state of mind in which we thereby find ourselves. Rather only with *that which* is represented.

HERBART 1808, p. 259; cp. BELLUCCI 2015, p. 72 f.

And later he wrote that “all our thoughts can be regarded from two perspectives, partly as activities of our mind, partly with respect to *what* is thought through them.” The former perspective would be psychological, the latter logical. He also said that “in logic, it is necessary to ignore everything that is psychological” and spoke of “the indispensable elimination [*Ausscheidung*] of anything psychological” from logic (Herbart 1813, p. 67 f., § 34. Cp. also Herbart 1814, p. 326 f., 1825, p. 118, § 119).²³

Herbart made it very clear that this is also valid for the use of concepts: we must distinguish between their “logical” and “psychological” sense:

Any thought (*Jedes Gedachte*) [...] is a concept in the logical sense. [...] The thinking subject [*denkendes Subjekt*] is irrelevant; one can apply concepts [to a thinking subject] only in a psychological sense whereas the concept of a human being, a triangle etc. does not properly belong to anybody. A concept in the logical sense generally *exists only once*. This could not be the case if the number of concepts would increase with the subjects envisaging them, or even with the different acts of thinking through which a concept is produced and aroused.

HERBART 1825, p. 119, § 120

And the chapter on logic in the *Textbook on Philosophy* begins with the following:

All our thoughts can be regarded from two sides; partly as activities of our mind, partly regarding *what* is thought through them. In the latter respect they are called *concepts*. This term, in signifying what is *comprehended*,

23 Gabriel 2013 and 2017, pp. 54–71, 112 treats the influence of Herbart’s anti-psychologism on Hermann Lotze, the neo-Kantians (esp. Windelband) and on Gottlob Frege.

demands an abstraction from the manner, in which we might receive thoughts, produce or reproduce them.

HERBART 1813, p. 67, § 34

The discussion on logic in Germany during the 19th century started with the question whether logic was a purely formal subject or whether it was metaphysical, or whether metaphysical, empirical or other factors at least play a limited role in it: “There are two ways of treating logic that are abruptly and drastically opposed to each other: the *formal* and the *Hegelian* one” (Prantl 1849, p. 3). The formal one was associated with Kant and Herbart and the metaphysical one mainly with Hegel. Roughly and schematically spoken, the debate evolved around the relevance of thought to forms of being, i.e., to the ontology of thought and of its object. The philosopher Friedrich Ueberweg (1826–1871) (who incidentally grew up in Ronsdorf like Carnap) gave a vivid description of the situation:

The subjectively-formal Logic²⁴ – that which is promulgated by the schools of *Kant* and *Herbart* – puts the forms of thought out of all relation to the forms of existence. Metaphysical Logic, on the other hand, as *Hegel* constructed it, identifies the two kinds of forms [thought and existence], and thinks that it can recognise in the self-development of pure thought the self-production of existence.

UEBERWEG 1857, p. v/1871, p. XI

Kant wrote that “logic is a science which refers to all thought, without regard to objects which are the matter of thought” (Kant 1800, p. 4/1885, p. 3) or, more pointedly, that it “abstracts from all contents of the cognition of the understanding and from all differences in its objects, and has to do with nothing but the mere form of thinking” (Kant’s *Critique of Pure Reason*, A 54). His programmatic pronouncements about the formal and general character of logic were indeed very influential in the debate, although they were kept short and not sufficiently detailed. It is true that he rejected any non-formal component of (general) logic. Logic is pure, because it does not contain any empirical rules of thinking following from its psychological or physical conditions of thinking:

24 Ueberweg uses “subjectively-formal” and “objectively-formal” in the somewhat unhappy sense of: “what, in Hegel’s logic, would belong to ‘subjective’ resp. to ‘objective logic’”.

Just because it abstracts altogether from objects [...] it cannot be an organon of the sciences. By an organon we mean an instruction how some particular branch of knowledge is to be attained. [...] Logic [...] cannot meddle with the sciences, and anticipate their matter, and is therefore only a universal Art of Reason.

KANT 1800, p. 4/1885, p. 3

That means that logic is formal *only* for the general employment of the understanding, *not* for the special one (see Kant's *Critique of Pure Reason*, B 76–79; MacFarlane 2002, esp. pp. 43–46; Heis 2012, pp. 98–100; Heinemann 2017, pp. 32–34). Herbart, however, as Ueberweg noted, “entirely separates from Logic, and refers to Metaphysics, the question of the significance of the forms of thought in knowledge” (Ueberweg 1857, p. 45 f./1871, p. 60)!²⁵ Ueberweg meant here to say: *in contrast to Kant* who *included it* into his (transcendental) logic.

Consequently, there always remained doubts whether Kant had not thwarted his views on analytic formal logic by expanding it in the *Critique of Pure Reason* to a “transcendental” and thus synthetic logic being anything but formal. Herbart characterised this kind of logic as “an attempt to show the completeness of the categories as alleged primary conceptions of the understanding – which belongs to psychology” and has thus to be rejected (Herbart 1843, p. 611 f.). He also wrote that “so-called transcendental logic pretending to prove originally innate concepts or forms of cognition is nothing else than a failed chapter of psychology” (Herbart 1831, p. 329 f., § 335). The Berlin philosopher Friedrich Harms (1819–1880), in his *History of Logic*, even dared to write in a similar spirit that it was “Kant, who, by discovering [in transcendental logic] the source of ontological concepts in the forms of [synthetic a priori] cognition, became the founder of *metaphysical logic*” (Harms 1881, p. 222).²⁶

Compared to Kant's view Herbart's formalist position in logic was thoroughly unambiguous and straightforward and did not differentiate between

25 Heis 2019, p. 29 takes the same passage as evidence that there were practically no rivals to “Kant's school” of logic at the time. This is true only if logic is taken as referring exclusively to the universal usage of the understanding in the sense of Kant. For Herbart, however, it crucially holds for *any* usage. This crucial difference was generally known at the time. So, it is at best misleading to readily count Herbart as belonging to “Kant's school” as Heis does. Cp. also MacFarlane 2002, pp. 44–46.

26 See Harms 1876, pp. 169–171 for more details. Harms was very influential in this opinion for the South-West German Kantians' understanding of logic. Wilhelm Windelband, Heinrich Rickert and Emil Lask mistrusted Kant's transcendental logic and his deduction of the categories. See especially e.g. Windelband 1884.

general and special employment of the understanding, i.e., between general and transcendental logic. His philosophy stands and falls with the thoroughgoing formal character of logic: In order to be fruitfully applied, there is no need as in Kant for (a part of) logic to be pushed up to a transcendental realm. If contradictions arise in our thinking, they are the result of a misapprehension of *concepts* and can never have an ontological origin (as in Hegel and others). Any identification of logical with transcendental or real forms is for Herbart thus completely out of the question.

The dynamics in the later debate of the century on the “logic question” was upheld by an almost steady generation of intermediate positions between Herbart and/or Kant’s general logic on the one side and Hegel or other metaphysical views on the other (on this debate see also Gabriel 2017, pp. 15–22). These positions tried to argue for *some* relevance of logic for reality (or of actual practice of thought for logic) without, on the one hand, falling prey to Hegelian exaggeration or Kantian transcendentalism or, on the other hand, to the complete neutrality of Herbart’s formalism for content.

In 1840, the philosopher Adolf Trendelenburg (1802–1872) proposed an attractive middle course that elegantly circumvented entrenched frontlines. In a clever move, he called none other than Aristotle to witness that logic serves (and has always served) as an organon of the sciences. He criticized thus *both* formal and Hegelian logic.²⁷ Formal logic (both in Herbart’s and in Kant’s clothing), he held, is incapable of discovering the real and relevant forms of thought because it ignores any reference to the contents of thought. But then again Hegelian logic is unable to deduce reality and its development solely from the self-movement of thought. Whenever Hegel claimed to have achieved this in his writings, Trendelenburg argued, one can always find an illegitimate use of intuition (*Anschauung*). We must thus conclude, he claimed, that cognition is enabled by something that thought and reality have in common. He ultimately found this common element in *Bewegung* (motion, movement, change). Although only very few could follow him on this, his conception was seen as putting logic back into the realm of genuine *Erkenntnistheorie* or *Theorie der Wissenschaft*. The general employment of the understanding thus

27 His criticism also implies an historical claim on Aristotle: Aristotle did *not* demarcate logic by its formality (Trendelenburg 1840, I, p. 18): “Aristotle has nowhere expressed his intention to grasp the forms of thought from within themselves [*aus sich selbst*]. Such a separation is alien to Aristotle and only a newer invention”, namely an invention by Kant (as becomes clear from Trendelenburg 1840, I, pp. 4–8). Cp. MacFarlane 2002, pp. 45, 47, 56. Drobisch, however, rejected Trendelenburg’s claim that logic was not formal for Aristotle (Drobisch 1851, pp. ix-xii).

became much more dependent on the special employment of the understanding than in Kant's scheme. It was (and is) not so clear what that means in the end for Kant's view of general logic (outside of the transcendental claim).

Trendelenburg's proposal has an air of tragedy around it because it was not at all intended as a backward-looking enterprise of revaluing Aristotle and thereby even more increasing the isolation of contemporary philosophy vis-à-vis the developing sciences. No, he wished for just the opposite, namely, to make formal logic (and thereby philosophy) more relevant for the sciences: "I had the wish in mind, to make logic, by taking full account of the individual sciences, in its reasons more experienced, more significant in itself and thereby more fruitful to the outside" (Trendelenburg 1840, I, p. VIII). Whereas for Kant formal logic "has been unable to take a single step forward" since the time of Aristotle (B VIII), Trendelenburg conversely recommended Aristotle as someone who has taken the needed step forward already long ago, thus becoming logic's reformer for the present and the future.

Friedrich Schleiermacher (1768–1834), former teacher of Trendelenburg for a time, also established a relation of the reform of logic with Aristotelian doctrine, albeit a weaker one than Trendelenburg. In rejecting a full-blown identity between the forms of knowledge and the forms of real existence he argued for some kind of a *parallelism* between them. His *Dialectics* (Schleiermacher 1839) on which he had lectured since 1811 and where he developed this view, appeared posthumously just a year before the work of Trendelenburg (on Trendelenburg and Schleiermacher see also Heis 2012, pp. 109–111). Ueberweg aptly and crisply characterised the Aristotelian move in the logic debate and the ensuing interpretation of Aristotle thus:

The Aristotelian apprehension "of the forms of thought [as seen by Schleiermacher and Trendelenburg] holds a middle place between the subjectively-formal and the metaphysical Logics. [...] Aristotle, equally far from both extremes, sees thinking to be the picture of existence, a picture which is different from its real correlate and yet related to it, which corresponds to it and yet is not identical with it."

UEBERWEG 1857, p. v/1871, p. XI

Ueberweg himself had strong sympathies with Trendelenburg's and especially with Schleiermacher's views on logic and consequently propagated "general Logic [...] as a theory of knowledge [*Erkenntnißlehre*]" (Ueberweg 1857, p. viii/1871, p. xii). For the rest of the century, the discussion in Germany on logic mainly turned around the dichotomy between a purely formal logic

and an epistemological one admitting some input from the sciences (see e.g., Windelband 1907, p. 185).

Trendelenburg complained in 1846 that his objections of 1840 to Herbart's conception of logic had not met an answer yet (Trendelenburg 1846, p. 354). Another five years had to pass until the school of Herbart developed effective counterarguments. The Leipzig logician, mathematician and Herbartian philosopher Moritz Wilhelm Drobisch (1802–1896) took the occasion of the second edition of his *New Exposition of Logic* to scrutinize and criticize, especially in the foreword, anti-formalist arguments in Trendelenburg and in general (Drobisch 1851. For Drobisch see Heinemann 2017, ch. 4; Villkko 2002, pp. 39–49, pp. 74–78; Villkko 2009, p. 207 f.; Heis 2019, pp. 44–52). Drobisch had already contributed before, especially through favourable reviews in journals, to promote Herbart's philosophy and make it more widely known and acceptable. In the first edition of the *New Exposition* of 1836, Drobisch had vehemently supported Herbart's formalist conception of logic: "Logic is indeed nothing else than pure formalism, it does not want to be and should not be anything else" (Drobisch 1836, vi). For his critique of formal logic, Trendelenburg had chosen Drobisch as one of two authors as targets (Trendelenburg 1840, I, p. 7).

It is not true Drobisch argued in 1851 that formal logic separates, as Trendelenburg claims, thinking from its subject-matter and thus inappropriately presupposes the existence of "pure thought". On the contrary, he said, logic does not know forms without content but only forms that are independent of *any particular* content that remains *indeterminate* and *accidental* for logic (Drobisch 1851, p. iv; see also p. 5 and later).²⁸ This move, it seems, opened up new vistas for a discussion of the nature of mathematical entities and of their existence.

It is remarkable that Drobisch draws here an analogy between logic and geometry: Basic geometrical forms obtain by abstracting from the sensible properties of observable objects. Yet geometry does not content itself with these abstractions but arrives, through the "combination" of basic forms, at "ideal constructions" (*ideelle Constructionen*) that "partly appear as aliens in

28 This shows that Drobisch's position does not coincide with that of neo-Leibnizians like Christian Wolff (1679–1754) and Alexander Gottlieb Baumgarten (1714–1762) who maintained that logic abstracts from concrete objects but not from highly general and abstract ones. Cp. MacFarlane 2002, p. 45 f. – Drobisch's talk of abstraction and ideal construction is very similar to Richard Dedekind's later views on abstraction and creation in mathematics, when he spoke of number as "free creation of the human mind" (i.e., free from any regard for empirical content except "distinguishability"), by only considering "the relations [of the numbers] to one another" (Dedekind 1888, pp. 360, § 73/1901, p. 68, § 73).

our sensible and familiar world" (p. v; cp. also p. 6, § 6). Geometrical truths, notwithstanding their experiential origin, thus do not in the end depend on experience but only on the relations of these abstractions and ideal constructions to each other.²⁹

This also applies to logic, Drobisch maintained. Although pure logic "is certainly a demonstrative science, it has to draw its beginnings from empirical facts. Before it can proceed to progressively link concepts with each other, it has to regressively abstract the elements to be linked from these facts" (p. viii). Logical truths, however, only depend on the "agreement of forms of thought with each other, of thought with its own principles. *This* agreement and nothing else is what we mean by logical truth" (p. v). This view implies a subtle modification of the radical formalist outlook of the first edition and a sophisticated concession to the epistemological view of logic à la Trendelenburg, without, however, giving up the idea of the creative freedom of the mathematician or logician (it also clearly moves away from logical universalism, e.g. in the later sense of Bertrand Russell: namely logic as understood as maximally general truths, thus preparing the way for Wittgenstein's view of logic as a system of tautologies).

It follows that logical truth has also to be distinguished from "material truth": A sentence is true in the material sense (of Drobisch) if what it says is really the case:

The assessment of the *material* truth of what is given to thought [...] lies outside the scope of logic. Logic can stand up for nothing more than that, *if* the given has material truth, *then* also what is deduced from it has it. This fact, however, does not diminish the value and significance of logic, because it specifies the only secure way to indirectly reach true findings.

DROBISCH 1851, p. 7

Drobisch's turn nevertheless remains a clear rebuff to any stronger claims of an epistemological (or metaphysical) conception of logic (in the sense of Trendelenburg, Ueberweg and others) and thus an effective defence of Herbart's formalist outlook.

²⁹ Drobisch does not mean by *ideell* something perfect (*ideal*) as opposed to something imperfect, but something fictional or inexistent (being actual only as a thought, as an idea) in contrast to something real! It is, however, neither arbitrary nor subjective, in the later sense of Gottlob Frege. It is composed of elementary forms of thinking that are, as in geometry, the result of elementary abstraction!

Most significantly, Drobisch used these considerations for a critique of Kant's synthetic a priori that roughly goes like this: Abstracting from experience does not mean, as we have just seen, to remove *all* empirical content in what is left. Therefore, there are no pure forms of intuition free of experience as Kant has taught. To abstract from empirical content only means to abolish a *particular* content: "To imagine a *pure* form, a form divested of *all* matter, is as impossible as it is to imagine a matter without a form" (p. vi f.). Consequently, there are no synthetic intuitions a priori. Drobisch diagnosed that "Kant's misinterpretation of the fact of pure mathematics has undeniably exerted a seducing influence on him" that was not conducive to his efforts (p. vii).

2.3 *Aesthetics and Ethics*

What remains to be discussed is Herbart's conception of ethics. Herbart thought that ethics (including the theory of natural law in the legal sense) and aesthetics are based on "immutable determinations of value" or "value assessments" (*Werthbestimmungen*) (Herbart 1813, p. 44, § 4). The most general assessments are approval and disapproval, praise and blame. Since ethics and aesthetics are similar in this respect, they must be subsumed under a joint general category, which Herbart chose to call "aesthetics" as well. They are also similar in their not judging objects or states of affairs individually but only *relations* among them. Perhaps because of his pedagogic practice and insight, Herbart thought that simple value assessments or "aesthetic judgments" are based on original and *intuitive* evidence and cannot be derived from other judgments with non-aesthetic predicates. "In the subject matters of logic, ethics and indeed the whole of aesthetics one has to deal with subjects that occur with an *immediate* evidence – a sort of evidence that is alien to the whole nature of metaphysics, in which knowledge can be acquired only through the elimination of error" (Herbart 1813, p. 221, § 131, note to 4th ed.).

Value judgments are of a twofold nature for Herbart: First, the subject of the judgment must be comprehended or recognized as such without any help or addition of elements of preference or rejection. This is called the "theoretical idea" of the subject. Any value judgment presupposes such an independent idea. As soon as a predicate is added to this subject expressing approval or disapproval in an "immediate and spontaneous way" a judgment of an aesthetic nature is formed (Herbart 1831, p. 80). It is hard to say whether Herbart wants the "theoretical idea" to which a value is added to be a *statement* in its own right asserting the existence of a certain subject or whether he just wants to say that our idea of the subject must be clear and stand-alone without having the valuing predicate considered yet. In an early text, Herbart maintained that the difference between an *Erkenntnis*, i.e., a judgment of recognition, and a

Geschmacksurteil, a judgment of taste, is tantamount to the difference between the question for “being” and the question for an “ought” (Herbart 1807, p. 334).

Herbart admitted that there is the great danger of connotations or biased opinions interfering and thereby obscuring and distorting the immediate evidence of value and rendering it impure (Herbart 1813, pp. 105–109, § 72–76). Such interferences are one reason why aesthetic judgments are commonly thought to be wholly subjective and arbitrary. There is an important parallel to logic for Herbart in this respect: In the same way as logic must find its objective subject matter underneath a psychological jumble, so aesthetics has the task of identifying the simple reactions to the “beautiful” and “ugly” (in the general sense that includes “good” and “evil”) among the admixtures of metaphysics and psychology. According to Herbart, the reason for the existence of objective ethical judgments not being as commonly accepted as the objectivity of the logic of judgments lies in the lower maturity in the current state of the philosophical discussion of aesthetic judgments compared to the one in logic. He thought that the scientific character of logic has already been accepted for a long time whereas the struggle for a scientific conception of ethics is only of recent origin and needs more persuasive philosophical effort.³⁰

Another parallel between aesthetics and logic is for Herbart their extensionality: The value (whether aesthetic or logical) of a complex sentence depends only on the values of its simple ingredients. The immediate evidence, absolute validity and relentless rigidity of an aesthetic judgment that Herbart assumes only applies, however, to elementary judgments. Since works of art are highly composite, elementary aesthetic judgments fuse into indeterminate feelings. This leads also to the appearance of uncertainty, variability and subjectivity of aesthetic judgments and thus to the seeming difference from logical ones.

Ethics differs from aesthetics (in the narrow sense) by its subject matter: it judges the quality of *volitions* (or rather of relations between them). Yet it does not judge them according to their capacity to fulfil certain goals (especially not the goal of happiness or welfare), but rather according to the different relations into which wanting beings can enter with each other. Both ethics and aesthetics are disinterested – in this rejection of eudemonism Herbart shared Kant’s view. For this reason, “beautiful” is for Herbart not to be confused with “pleasant” or “useful”: these are properties relating to our emotions but not to intrinsic qualities of volition.

30 Herbart is thus someone like a “hyper-anti-psychologist”: not only formal logic is anti-psychologistic for him, but also ethics (cp. Herbart 1825, p. 118, § 119)!

The approval or disapproval expressed in an elementary aesthetic judgment “possesses original evidence in virtue of which it is clear without having to be learned or demonstrated.” Yet, the evidence is often distorted by interfering ideas, or it is only felt and not clearly discerned. For this reason, one must highlight and demonstrate the “original purity and precision” of what is approvable and what is not (Herbart 1813, p. 105, § 72). This demonstration can be achieved by recurring to five basic ethical circumstances or “practical ideas” (prime examples or *Musterbegriffe* of ethics) possessing original evidence: the idea of perfection or efficiency, the idea of good-will (benevolence), the idea of justice, of equity or retribution, and of inner freedom (i.e., the harmony between a concrete volition on the one side and conviction and moral insight on the other). These ideas are, except for the last one, abstract expressions for aesthetic judgments obtaining in relations of wanting beings with each other. They take the place of the categorical imperative in Kantian ethics. Virtues and obligations arise only from applying practical ideas to the concrete circumstances of human life. The five original ideas yield for their part five derivative ideas.

Herbart’s ethics is thus of a non-cognitive nature because value judgments cannot be reduced to factual judgments and hence are not empirically verifiable as they are. Yet it also has a sort of a cognitive character since elementary value judgments can be correct or incorrect, at least as a matter of principle. Their ethical form, so to speak, must be detected in a laborious process of going beyond any psychological or metaphysical deformation and disguise on the surface. To detect the ethical form resembles the process of finding the logical form of a judgment below its immediate appearance, except that it is much more difficult and contested as in the case of logic. One can see here the educator at work: It would be a complete failure of education to teach children only the non-cognitive side of ethics. Note that Herbart does not invoke any supernatural foundation for ethical insight. Moral education, or indeed education as such, is for him the development of the power of ethical judgment and orientation. In this he also differs from Kantian ethics.

The most important aspect of Herbart’s ethics, however, both from a systematic and an historical viewpoint, is its rejection of Kant’s metaphysics of free will. For Herbart it is deeply inadequate and completely meaningless to regard, as Kant does, human beings as belonging to two separate realms, the sensual realm of experience and the intelligible realm of things in themselves, and to locate free self-determination of humans only in the latter. From the beginning of his philosophical career Herbart was convinced that education is impossible if one takes Kantian ethics in this sense seriously. Kant is for him a fatalist because his conception makes formation of character through

education impossible (e.g., Herbart 1813, p. 54, § 16; 1825, p. 331). Drobisch puts Herbart's views succinctly: "Indeterminist freedom [in the intelligible realm] is neither logically conceivable nor factually justified nor is it compatible with the interests of morality" (Drobisch 1876, p. 31). It would go beyond the scope of this overview to expand further on this.

3 Dörpfeld's Herbartian Philosophy and Religious Outlook

Carnap showed a deep admiration of his grandfather:

There were two relatives, both highly revered by my mother, whom I regarded from childhood as models of men, admirable for the fact that in their scholarly field they did not simply follow traditional ways but searched for their own new paths. One was my maternal grandfather, the other my uncle [Wilhelm].

CARNAP 1957, A-7

He explicitly related central features of his own philosophy to his mother's influence and thereby to the influence of his grandfather, especially his ideas on tolerance, humanism and ethical non-cognitivism (Carnap 1957, A8-A15). He read the Dörpfeld biography at least in 1922 with his mother, as his diary testifies (September 12, Carnap 2022), but we can undoubtedly assume that he was familiar with it already much earlier. The biography was a sort of a life project for his mother occupying and dominating her thoughts until her death in 1924. The book contains a wealth of material on Dörpfeld's Herbartianism and Pietism, although the Zionite episode (of which Carnap was aware as already reported above) is once again strangely absent.³¹

We will now turn in detail to the book "On Ethics" (Dörpfeld 1895) that among Dörpfeld's writings arguably exerted the most important influence on Carnap. A leading question will be how Dörpfeld combined Herbartianism with his religious orientation. Carnap reported that his mother had already read this book to him and his sister when he was still a child. He admitted that he is "not sure whether we children quite understood these ideas" (Carnap

³¹ Anna Carnap briefly refers to it in an apologetic tone in Carnap A. 1926, pp. 5–7. She complains that Eller's movement was called a "sect" and therefore disparaged. She also stresses the positive correlation of the "autonomous, authentic and vivid religious life" of the Zionites with Ronsdorf's "spiritual life and life's outer [i.e., economic] relations" (p. 6 f.).

1957, A-9). It is highly significant that he requested a copy of the book from his mother in 1920,³² at a time when he began his “own research in philosophy” (Carnap 1963, p. 10).

“On Ethics” is the writing of Dörpfeld that stands out most for its philosophical orientation – perhaps it is even the most philosophical one of them.³³ Dörpfeld himself attributed to it a scientific character (Dörpfeld 1895, p. xii) and judged it as the “most revolutionary” of his writings: “There is arguably no part of theology that is not unsettled by it,” he said (Carnap A. 1903, p. 560). He worked on it for 30 years and attached great importance to it (Carnap A. 1903, p. 443).

The text was planned as a large-scale treatise but was never finished. A large part of it was published posthumously in 1895 by Dörpfeld’s son-in-law, the clergyman Gustav von Rohden (1855–1942) who married a younger sister of Anna and Wilhelm, Agnes (1858–1907). The final intended structure of the book is not entirely clear. It seems that the part entitled “The Secret Shackles of Scientific and Practical Theology: A Contribution to Apologetics” (Dörpfeld 1895, pp. 1–183) presents the main argument, as also the editor assumes (*ibid.* p. xviii). It comprises three chapters: “The evidence of ethics” (philosophically speaking the most important part), “The significance of ethics for religion and theology” and “The mixing of ethics and dogmatics, or: the false dogma of the moral and obligatory nature of faith.” During the last years of his life, Dörpfeld gave a précis of these chapters in more colloquial terms in a lengthy letter to his son Wilhelm that is reprinted in the biography (Carnap A. 1903, pp. 444–465; see also pp. 396–399 and p. 558 f.). It is unknown whether Dörpfeld collaborated with anyone in working on his book. Apart from Herbart, hardly any other author is mentioned in the text. It is probable, however, that there was one or the other academic among his circle of acquaintances who gave him advice.

The central claim of the work is that ethics and not religious belief forms the *basis* both of theology and of religion and that ethics is independent of religion or any *Weltanschauung* (Dörpfeld 1895, p. 51, p. xxviii). Ethics neither flows from the doctrinal part of theology, i.e., from dogmatics, nor from the bible or from the concept of God, but the other way around: dogmatics crucially depends on ethics (Dörpfeld 1895, p. 47, also p. 50 f.). Dörpfeld basically put forth two arguments to support this view: First, in following Herbart, he maintained that

32 In a letter to her of February 20, 1920. Hillman Library, University of Pittsburgh (RC 025-85-32).

33 Dörpfeld’s *Denken und Gedächtnis* (Dörpfeld 1884) can perhaps in this respect compete to some extent.

ethics is a “rational science”³⁴ independent of all other knowledge and of all doctrines, especially psychological ones, and that it is “evident” (we will shortly deal in detail with what that meant for him). And second, he thought that religion can become a matter of conscience (*Gewissenssache*) only through ethics. Without ethics, religious doctrines would merely be a matter of the intellect (*Kopfsache*) (Carnap A. 1903, p. 444). This brief overview already shows, or so I shall argue, that Dörpfeld’s philosophy is to be characterized as a fusion of Herbartianism and Pietism.

Dörpfeld contended further that received theology does not think this way and relies instead on the “scholastic method” or the “method of authority” in its understanding of ethics as an outgrowth of the bible, of theology or of tradition. He thought that theology places itself into fetters by following these methods. “[E]thics and not dogmatics is the fundamental discipline of theology. From this it follows further that ethics must be developed and taught [...] in a rational way as deriving from its own source of knowledge” (Dörpfeld 1895, p. 50; cp. also p. xxvi). The “rational” and “genetic” method (which he also thought characterizes logic) is the only reasonable one for ethics, Dörpfeld argued, also for a scientific pedagogy.³⁵ Dörpfeld emphatically invoked the name of Herbart in all this and regarded him as the first to have shown the possibility of a fully elaborated rational *science* of ethics – “science” in the same sense as logic is a science (Dörpfeld 1895, p. xxxi).³⁶

In addition, he thought, one must realize that the scholastic approach bears an eminent risk: If someone has doubts about an article of religious faith and cannot but reject it, this will, under the premises of the scholastic method, inevitably result in a loss of trust in religious authority. But when trust is lost and ethics is believed to be dependent on authority, not only dogmatics is abandoned in the end but also ethics. This is the reason, according to Dörpfeld, why so many educated people turn their back on religion and neglect ethics in the way as it is taught by the church (Dörpfeld 1895, p. xxvii). If, however, ethics is

34 What Dörpfeld meant by this is that ethics is not based on outer facts like the natural sciences or on doctrines or revelations like theology. In this sense logic is also rational. And it is a science in being based on knowledge – in this case on knowledge of attitude (*Gesinnung*), i.e., on something spiritual (Dörpfeld 1895, p. 17).

35 “Genetic” does not mean here that the natural psychological development is investigated, but the epistemic order of ethical expressions among one another, or “their order relative to *epistemic primacy*”, as Carnap put it for the propositions of his constitutional system (Carnap 1928, p. 64, § 54/2005, p. 88). Dörpfeld does *not* relapse into psychologism here.

36 As it becomes apparent from a note in his estate, Dörpfeld planned to write a book dealing with all the objections against Herbart’s ethics and with all the defences of Herbart, as far as they were known from the literature (Dörpfeld 1895a, p. 90).

seen as being independent of dogmatics, people can retain their ethical outlook even if they have doubts about the doctrinal side of faith. So, ethics can become a “neutral ground” on which to discuss questions of human existence outside of religion. If ethics is not only abstractly known but really taken to heart, Dörpfeld further argues, it raises the awareness of the moral nature of human actions.

Acknowledging an autonomous ethics also means acknowledging the existence of sin. Admitting the latter inevitably leads to a moral dilemma. As a result, Dörpfeld thinks, a yearning for liberation from inner conflict develops in people and the conviction arises that redemption cannot come from the individual alone, but only from an outside authority. Expressed in religious terms, this amounts to an “inner revelation of God”, even if the person in conflict is not aware of a divine inspiration (Carnap A. 1903, p. 445). At this point theology has a chance to win back the religious sceptics without patronizing them or instigating fear in them or the like. Religious “dogmatics” can now come into play in a much less doctrinaire way, as knowledge of the “outer revelation of God” in creation and in salvation. This is not a revelation of “doctrines, theorems, words” but of “deeds, works, events” (p. 446).³⁷ It must be really called tragic for Dörpfeld that his grandson Rudolf would not develop an inner conflict in Dörpfeld’s sense, or if he did, he did not resolve it in the way Dörpfeld had predicted.

Carnap gave a condensed summary of his grandfather’s outlook in his autobiography (just before he conveyed in the above quote that his mother had read her father’s ethics to him and to his sister):

Her father had always strongly emphasized that in the education of a child’s character, the moral principles should be based only on the child’s own conscience and not on God’s will. He criticized the church severely for making ethics dependent upon theology, because once young people would begin to doubt the dogmas they would also be in danger of losing their moral ground.

CARNAP 1957, A-8 f.

37 It should be noted that Dörpfeld did not only criticize traditional church ethics but also Eudemonism. One can admit, he said, that Eudemonistic Ethics applies the rational method, but its principles are derived from other doctrines so that it lacks an independent and thereby a scientific character. “The theory of Eudemonism is no moral theory” because it is “nothing else but a doctrine of egoism” and not of ethics (Dörpfeld 1895, p. 51; see also p. xxviii). Dörpfeld opposed Social Democracy because of its supposed proclamation of and adherence to Eudemonism (p. xxxii). Apart from that he resolutely fought for the solution of the “Social Question” from early on (so the title of Dörpfeld 1866) and refined the social pedagogy of Karl Mager (see Rudloff 1922).

It is time now to deal in more detail with Dörpfeld's views on the nature and foundation of ethics as well as his ideas of its justification. This is a part of Dörpfeld's work that became highly relevant for Carnap. Dörpfeld does not tackle ethics directly but makes a most interesting detour by asking about the "evidence" of the *sciences in general* ("science" in the broad sense of German *Wissenschaft*).³⁸ This affords us to make a detour as well in this presentation. Dörpfeld first notes that mathematics is usually regarded as the science with the highest degree of evidence. Its certainty is either based on axioms, i.e., on sentences that are self-evident³⁹ (he leaves aside for a moment whatever that may mean), or on concepts that are "precisely determined" by the mathematician without worrying about the existence of corresponding objects in the world (Dörpfeld 1895, p. 1).

In obviously echoing Drobisch (see above p. 39f.), Dörpfeld makes it clear that these concepts are originally also abstracted from experience but then idealized independently of it (Dörpfeld 1895, p. 2; cp. also Dörpfeld 1895b, p. 130). The task of the mathematician is then to draw the possible consequences from these concepts, i.e., to deduce from their abstracted features any implications. The evidence of the new truths obtained in this way lies in the fact that they logically follow from the essential features of the concepts – in other words, that the contrary assumption is impossible, i.e., logically incompatible with the essential conceptual features. Mathematical research is thus characterized by its starting point and by its method: It starts from imaginary or invented concepts (*gedachte Begriffe*), not from real objects, and proceeds by "inference, syllogism or deduction". "But now", Dörpfeld continues,

38 *Evidenz* in German is not quite the same thing as "evidence" in English. If a fact is (empirical) evidence for a proposition, i.e., support for its truth, this would be rendered as *Beleg*, *Befund* or *Beweis* in German, although one also increasingly finds *Evidenz* because of the growing influence of English on contemporary German. The German adjective *evident* is more akin to "immediately apparent", "obvious", and "intuitive" or "not in need of proof" than to "supported by evidence". It describes more the psychological process of being (immediately) convinced of something. There is no grammatical analogue for the English "self-evident" in German (as e.g., *selbstevident*). Dörpfeld also uses *evident* in the sense of "certain" (*gewiss*). The term *Selbstgewißheit* exists in German, but normally only in the sense of "self-assurance". Dörpfeld here redefines *Selbstgewißheit* for his purposes as a "belief that intrudes with necessity against wish and will", whereby the necessity is the inevitability of natural law (1895, p. 7). So, some caution is required in reading Dörpfeld's (and Herbart's) texts in respect to "evidence".

39 This term here correctly translates Dörpfeld's use of *selbstgewiß*, literally "certain in and for itself".

the axioms used by mathematics come from logic, and their whole deduction is based on the theorems that logic has established about concepts, judgments, inferences. The evidence of mathematics thus depends on the evidence of logic, and mathematics can be evident only if logic is. From this it further follows that, if we rank the sciences according to the degree of their evidence, then it is not mathematics that deserves the highest place, but logic.

DÖRPFELD 1895, p. 2

This is a short, but nevertheless full-fledged statement of logicism, perhaps somewhat clumsily formulated (compared to later standards) and rather sweeping, yet clear enough, at least for Dörpfeld's purposes.

Dörpfeld's discussion is deeply Herbartian: Logic is taken as a formal science; it is also assumed that logic is only of formal relevance for the sciences. And it is argued for an "if-thenism" (as Musgrave 1977, p. 109 and *passim* called it) especially in mathematics and logic (Dörpfeld 1895, p. 2, fn.). This is a view – usually attributed to Bertrand Russell – that has been advocated by Herbart from early on (Herbart 1808, p. 262; 1813, p. 79, § 53, also 1829 p. 59 f., 204; 1831, p. 316, § 216 and 1843, p. 612): Categorical judgments, Herbart claimed, are in reality, in their true sense, of an hypothetical nature: The truth of the judgment "If concept C has such and such features E, then it also has other features F" does not depend on the existence of the subject with features E, which C talks about. Consequently, categorical judgments like "All As are B" do not have existential import, i.e., they do not imply (as they do in Aristotelian logic) that there is at least an A. "Every judgment *has to be, as such, hypothetical*. 'A is B' does not mean that A is – but that, *if A is posited (gesetzt), then B is also posited (mitgesetzt)*, in order to be united in one thought" (Herbart 1808, p. 262).⁴⁰

⁴⁰ Herbart also said that the difference between categorical and hypothetical judgments is only a matter of the linguistic form (*Sprach-Form*) (Herbart 1808, 264). Herbart noted later that Christian Wolff had a similar view in his *Latin Logic* (Herbart 1813, p. 80, § 53, in the 3rd and 4th ed. of 1834 and 1837). Cp. Wolff 1732, p. 229, § 226: "Propositiones categoricae æquivalent hypotheticis et ad eas reduci possunt" (categorical judgments are equivalent to hypothetical ones and can be reduced to them). Wolff also wrote (on the page following this) that if a subject is expressed without a particular conditional, a proposition has a "categorical form even if it is in truth hypothetical" (Kant rejected this view explicitly in Kant 1800, p. 163, § 24). For Herbart, the copula acknowledges that the predicate B applies to the subject A. Since Frege, A and B are of course understood as predicates (functions) applied to objects (their arguments): "All As are B" is thus to be read as "For all objects x: if x is A, then it is B". Cp. also Gabriel 2001, p. 157 f. For Drobisch's if-thenism cp. Drobisch 1836, pp. 25 and 52.

Thus, to state that a rectangular circle is impossible (or that it is green or whatever) does not imply that a rectangular circle exists (in any sense whatsoever) (Herbart 1813, p. 79, § 53). It was possibly for the first time in history that someone applied the equivalence of categorical and hypothetical judgments to the question of existential import of judgments. This argument excludes from logic the problem of the nature and existence of the entities as described in the hypothetical premises and restricts its task to the structural side of our thought as Dörpfeld observes:

It is the *task of logic* to find the forms of correct thinking, i.e., the formal marks of the correct concepts, correct judgments, correct inferences etc. In other words, it must find the formal characteristics, which thought-structures [*Denkgebilde*] of all realms of knowledge carry with them when they are *convincing* for anybody.

DÖRPFELD 1895, p. 2

It is quite natural that the concepts from which mathematics starts are originally abstracted from experience, but then they are ideally [*ideell*] conceived. That means one refrains from asking whether they can be so perfectly represented in reality. This is the reason why mathematics always says: *if* such and such a concept (e.g., a triangle) is dealt with, *then* etc.

DÖRPFELD 1895, fn. on p. 2⁴¹

Dörpfeld's characterization of mathematics and its relation to logic sounds very modern indeed and one must search for long among his contemporaries, even among professional mathematicians, to find a voice talking in a comparable way. In addition, it is also original since no-one at the time, as far as I know, seems to have tried to argue for logicism by means of the concept of evidence. And hardly anyone discussed the role of logic within the total system of knowledge including its application in non-logical fields.

This is not yet all what Dörpfeld had to say about logic because all he has shown so far is that the evidence of mathematics is grounded in the evidence of logic. But what is the evidence of logic and where does it come from? It starts, according to Dörpfeld, as all search for truth does, with naturally occurring

41 Dörpfeld's use of "if-thenism" is obviously related to Drobisch's view of mathematics as "ideal construction": The mathematician conceives of mathematical entities as abstract forms without being bothered by experience or intuition and looks for the consequences of postulating them. See above p. 39f.

evidence of belief and ends in the objective confirmation of this evidence. Dörpfeld thus sees two “marks” or “indicators of truth” (*Wahrheitszeichen*): The first one is the natural occurrence of a conviction or belief in thought according to natural psychological law. It serves as a *signal* of the presence of truth in a certain formation of thought. The second mark tells us what this truth objectively consists in, what the reason for it is. Science as well progresses by moving from the first to the second mark of truth: Whenever we encounter a strong belief (as a natural fact in consciousness), we ask for an objective characteristic of the concept correlating with this belief and discuss it hypothetically. Dörpfeld calls this “inductive reasoning”.⁴² The inductive search must be repeated many times with the aid of many examples so that it can increasingly converge on a certain special property of the situation responsible for the naturally occurring belief. The resulting hypothesis must then also be evaluated against cases where the conviction is absent. At the end, we can say that the relevant belief emerges whenever such a characteristic is present and vice versa.

In logic, the process from the first to the second indicator of truth results in certain formal characteristics of an argument:

This is the place where one has to look. From then on logical research took the route that all empirical sciences [...] had to take, namely of comparing and abstracting and the subsequent examining of new examples, in short of induction.

DÖRPFELD 1895, p. 3 f.

We can summarise Dörpfeld’s vision of the founding of logic in five steps:

1. Conviction through evidence: occurrence of a “subjective feeling of logical satisfaction or belief” (p. 5) accompanying certain thought (argument)
2. search for an objective factor that is common to all the cases where the same feeling or belief occurred: “comparing and abstracting”
3. setting up a hypothesis as to the “objective marks of the correct formations of thought”
4. examination of this hypothesis through further examples: “putting it to the test”

42 In 1884, Dörpfeld invoked William Whewell’s *History of the Inductive Sciences* (1837) in this respect, available in German since 1840, and claimed to have combined inductive reasoning with Karl Mager’s “genetic” method (1884, p. xxiii). Inductive method is genetic in Dörpfeld’s sense, if it follows the *actual* historical path that the method has embarked upon originally (or the most probable one if the actual development is unknown) (*ibid.*).

5. transmission of evidence from the belief to the mark of the detected truth: “The fact of being convinced [of the theorems of logic] has impressed on them the seal of evidence” (p. 5).

In this context Dörpfeld makes a remark that shows why a pedagogue is so much interested in such a topic: He observes that one does not have to know what the theorems of logic are in order to think correctly and successfully:

Recent psychology, at least the one of Herbart, can provide exact information how it is possible and how it works that natural thought [being ignorant of the explicit theorems of logic] can be successful [in its logic], provided that the relevant illustrative material [*Anschaungsmaterial*] is unmistakably and clearly presented.

DÖRPFELD 1895, p. 6

It is the aim of pedagogy to develop didactical methods and to provide visual aids that enable a child to find by herself or himself the *objective* characteristics of a belief as quickly as possible and to eliminate faulty ones.

At first sight it might seem that Dörpfeld’s view does not allow for a difference in the way logic and the empirical sciences proceed. There is, however, one circumstance that makes it impossible for the empirical sciences to reach the fifth step above (we could call it the stage of “complete” evidence) namely lacking *direct* evidence in certain cases. To show this, Dörpfeld introduces the distinction between “explaining sciences” (*erklärende Wissenschaften*: his term for empirical sciences) investigating the causal relations of the phenomena and “norm-seeking sciences” (*normsuchende Wissenschaften*: logic, aesthetics and ethics) looking for the norms of ideal objects (p. 10 f.). Both types of sciences try to reach evidence for their results; they both look for *Erklärungsgründe* (reasons of explanation) by referring to basic matters of fact.⁴³ The two types of sciences differ in the end, Dörpfeld maintains, in the type or strength of evidence that is available for their objects: The objects of the norm-seeking sciences are, as just explained, *directly evident* (because directly given in experience), whereas the objects of the explaining sciences like “force, matter,

43 It is possible that Dörpfeld took the distinction between the two groups of sciences from Drobisch who once praised Kant for agreeing with Herbart in assuming in his ethics that “practical philosophy is neither a describing nor an explaining science but a *norm prescribing* one [... and that] the validity of these prescriptions is based exclusively [...] on their *absolute* [...] *value*” (Drobisch 1876, p. 25).

atom, ether etc.’ are only indirectly given and are not inferable from sensual perception. They can therefore only be hypothesized upon.⁴⁴

As a result, empirical sciences can only show that the entities they postulate can serve as *logically possible* explanations for the phenomena, but they can never claim *evidence* for them, as the norm-seeking sciences can (mind you: “evidence” in the special sense of Dörpfeld!). This means that, at a certain point, “experience and with it induction run out” for the empirical sciences, as Dörpfeld says, but not for the norm-seeking ones. Dörpfeld’s view must thus arguably be seen as a case of Metaphysical Antirealism combined with Scientific Realism (for a successful treatment of this combination see Alai 2023).

After the deliberation of the sciences in general and their basic differences, Dörpfeld subsequently analyses, in so many words, the theological and other consequences of his views. I think we can opt out here from the rest of Dörpfeld’s discussion and try to assess the general nature of his outlook in his work on ethics. The first question, which arises is whether Dörpfeld, by seeing logic as being ‘grounded’ in (psychological) evidence, in the peculiar way explained, commits himself to psychologism. I think definitely not and in order to substantiate this I will fall back on Lewis Carroll’s “What the Tortoise Said to Achilles” – incidentally also from 1895 (Carroll 1895). In this article, Carroll showed that nobody can be forced by reason alone to accept a valid logical inference.⁴⁵ Dörpfeld can now be interpreted as saying that in order for logic to work, i.e., to move the mind of a reasoner (or, in Carroll’s terms, for Achilles to force the Tortoise to accept a valid argument), there must be at least *some* occasion where the Tortoise has encountered *evidence of validity*, i.e. has (psychologically) *experienced* an inference before as valid, which Achilles can appeal to. We could say that the reasoner (or the Tortoise) must be converted by an *evidential involvement* to a practice. I hold that this argument touches neither the objectivity of logical relations nor their being based on meanings nor on empirical-psychological facts. The argument says that logical rules *alone* are of no significance – but *people must make them their own* (must

44 Dörpfeld expressed this view already in 1884, p. xix. He stated there that scientific concepts like “force” etc. are “a piece of genuine, true-hearted, i.e., natural metaphysics that is not yet philosophical, i.e., scientific.”

45 As a quick reminder, here is a short outline of Carroll’s argument: Achilles proposes a valid inference of premises A and B and conclusion Z (e.g. *modus ponens*). The Tortoise declares accepting A and B and invites Achilles to force him to accept Z. Achilles notices that if A and B are true then Z is true. The Tortoise calls this (complex) proposition C, accepts it, and asks Achilles again to force him to accept that if A and B and C are true then Z is true. And so on ad infinitum.

commit themselves to them, must be affected by them) in order that they can actually unfold their force.

What goes for logic also goes for ethics and gives all of Dörpfeld's Herbartian "norm-seeking sciences" an unmistakable "Pietistic" touch! For Pietism doctrines alone do not count but *innere Gesinnung* (pious attitude, inner spirit, authentic conviction, true affection), or, more precisely, doctrines only count as far as they are filled with life as they undergo a commitment and show themselves in "deeds, works, events". One must be committed to logic as well as ethics in order to be able to apply it to anything. "The ethical is a property of the mind, or, more closely, of *Gesinnung*, of the will" and manifests itself in action (Dörpfeld 1895, p. 13); cp. also the statement that "morality is the doctrine of the correct *Gesinnung*" (Dörpfeld 1884, p. 168).

I want to illustrate the Pietistic nature of Dörpfeld's ethics with a letter of the blind Samuel Collenbusch – one of Dörpfeld's, as we have seen, favourite pietistic theologians – to Immanuel Kant in 1795 after his reading of Kant's ethics, probably the *Groundwork of the Metaphysic of Morals* (1785) (on Collenbusch see Renfordt 2014). It is improbable that Dörpfeld knew this letter, but it nevertheless gives a significant and precise illustration for Dörpfeld's view on ethics. Collenbusch in effect thinks that the mind of someone in respect to ethics can only be moved if the person sets his or her *hope* in it, i.e., commits oneself to it, develops an ethical *Gesinnung*. He insinuates thereby that the categorical imperative is not sufficient for this purpose. The validity of the argument is independent of its partly being framed in religious terms:

My dear Herr Professor!

Hope rejoices the heart. I do not sell my hope for a thousand tons of gold. My creed has astonishingly high hopes of God. [...] This hope rejoices my heart.

This summer I had your moral and religious writings several times read to me, and I cannot convince myself that you are serious about what you have written there. This is a faith free from all hope and a morality purified from all love – this is a strange phenomenon in the republic of letters.

The final aim to write something like this is perhaps the pleasure to delight oneself; in the inclination of people who are in the habit of being amazed by anything strange. I myself keep with a hopeful faith that is active through love bettering oneself or one's next one. [...]

It is impossible that my reason [*Vernunft*] and my will can exchange this promising [*hoffnungsreich*] faith with a faith that is purified from all hope.

I am sorry that I. Kant does not augur well from God, neither in this nor in the coming world, I expect a great deal from God. I wish you the same state of mind [*Gesinnung*] and remain in high esteem and love for you
 your friend and servant
 Samuel Collenbusch

COLLENBUSCH 1795, pp. 28–30⁴⁶

Kant did not respond to this letter. Collenbusch was born in the same year as Kant and died one year before Kant.

Note that, when logic, ethics and aesthetics share the same status of a norm-seeking science with one another, as Dörpfeld claimed, their degree of evidence is *ipso facto* the same! Dörpfeld titles one chapter as “Of the evidence of ethics standing on the same level as the evidence of logic” and writes that “indeed also aesthetics stands on the same highest level of certainty [as do logic and ethics]” (p. 11 f.). Dörpfeld followed again Herbart in this respect who claimed, as we have seen above, that to disagree on ethical and aesthetic principles is mainly the result of misplaced psychological and metaphysical interferences that must be surmounted.

After the treatment of ethics, I want to introduce here still another text by Dörpfeld, this time dealing with concept formation. In 1866 Dörpfeld wrote an article on the “Appropriate Formation of Concepts” that later turned into a separate booklet (Dörpfeld 1894; 9th ed. 1917) – a “logico-psychological-pedagogical investigation” (p. 27), as he put it. He obviously attempted to translate and systematize Herbart’s sparse and scarce comments on concept formation (and to some extent apperception) into a more modern language and a more compact form so that teachers can apply it easily. The investigation is “psychological”, he says, as far as the natural formation of concepts in children is discussed, i.e., the way concepts develop without a methodological or reflected intervention by parents or teachers and the effect such intervention has. It is “logical” or “scientific” (*wissenschaftlich*) as far as it deals with the rules or norms that must be followed when the concepts are “properly or proficiently formed” (p. 11; cp. also p. 3). And it is also pedagogical in that it concentrates on the most advantageous implementation of proper concept formation in schools. School instruction can help children; first, to become conscious of proper concept formation, that is to acquire “*clear and scientifically valid*

46 Collenbusch’s letter was reprinted in a collection of letters of German authors in Switzerland 1936. The letters were selected and edited by Walter Benjamin (yes, precisely him) under the pseudonym Detlef Holz. It is reported that Benjamin’s favourite letter of the collection was Collenbusch’s.

concepts”, and second, to “*shorten*, i.e., to speed up, the process of concept formation” (p. 12). Dörpfeld also makes the difference between the “conceptual content” of a concept, that is “the complex of its essential features”, and “a *concrete* idea, an exemplar of the many things that are subsumed under the concept” (p. 11).

4 Traces of Herbart’s Philosophy and of Pietism in Carnap’s Work

We now come to the question where to find the vestiges of the young Carnap’s critical engagement with the philosophy of his grandfather. I suppose readers who have some familiarity with Carnap’s philosophy have already found here and there in the foregoing exposition surprising and unexpected matches with Dörpfeld’s thought. Influence of Dörpfeld on Carnap is hypothetical and tentative because there is little or no direct evidence for it available. Perhaps the publication of Carnap’s correspondence with his mother will change this situation one day. Yet as contemporary Scientific Realism insists on the admissibility of employing inferences to the best explanation when theoretical entities in science are at stake, we are entitled to do something similar in respect to philosophy and its history and infer an influence of Dörpfeld on his grandson from available evidence.

It is advisable to start the confrontation between Carnap and his grandfather with their respective ethics and their relation to religion because this presents perhaps the field where the two come closest to each other and where their philosophical proximity becomes most evident (for a helpful and detailed overview of the development of Carnap’s ethics see Siegetsleitner 2014, ch. 5). In the unpublished part of his *Intellectual Autobiography*, Carnap expressly emphasised how important his mother’s views on ethics were for him:

While I grew up, my mother often explained to me that the essential point in religion was not to believe certain things, but to live the right life. And for the decision in all moral questions regarding right and wrong, she referred not to any authority, either the parents or the word of God, but rather to one’s own moral insight, the ‘voice of conscience’. This was one of the cornerstones of her brand of Lutheran Protestantism.

CARNAP 1957, A-8

These words make the brand of Anna Carnap’s faith also decidedly pietistic: the conduct of life is more important than any doctrine, and the voice of conscience serves as the highest authority. Note, however, that what is expressed

in the first sentence of this quotation is not exactly the view of Dörpfeld. For him, as we have seen, ethics is *separate* – and must be kept separate – from religious doctrine and is thus not an “essential point” in religion as such, although an essential *precondition* for it. One can argue, however, that by rejecting the widespread opinion that ethics plays only a subordinate role to the doctrine and by granting it an autonomy, Dörpfeld strongly revalues it and makes it in a way the real driving force of religion. The quotation from Carnap shows his mother’s and grandfather’s Pietism as potentially serving the enlightenment namely that there is no higher authority in religion than one’s own moral insight.

In a talk Carnap gave to the *Freischar* on “Religion and the Church” in 1911/12 he saw religion, like in the autobiography some 50 years later, as “something universal to humans, which depends neither on belief in a god [...] nor on any particular sort of ideal.” Religion is rather determined by the *Gesinnung* – the fundamental attitude, the “stance of heart” of someone “to whatever is highest” to the believer. In this way also patriotism and the like can be a religion (Carnap 2018, p. 476). Similarly to his grandfather, Carnap vehemently rejected the view that religion is defined by any *Lehrsätze*, by teachings or confessions of faith. “Religion not only does not consist in these [doctrines ...] but can neither be supported nor undermined by them, as it has no connection with them whatever” (p. 477).

The proclamation of this view foreshadows his later take on philosophical problems when he distinguishes between “knowledge statements” and “faith statements”, that is, between statements “about things that can be grasped by the understanding” and statements that express our “stance” towards these things and therefore only depend “on an insight of the conscience.” He drew a parallel here to the distinction of Heinrich Rickert, one of his professors in Freiburg at the time, between “statements of existence” and “statements of value” (p. 477).

We find Carnap’s view again in 1935 in a first canonical formulation of his non-cognitivism: “[A] value statement is nothing else than a command in a misleading grammatical form. [...] The propositions of normative ethics, whether they have the form of rules or the form of value statements, have no theoretical sense, are not scientific propositions. [...] They] have, here as elsewhere, no theoretical sense. Therefore, we assign them to the realm of metaphysics” (Carnap 1935, pp. 24–26), which means to the realm of the senseless (this did not mean that there cannot be a *descriptive* ethics as part of psychology or sociology. Its propositions are empirical and true or false and therefore of a cognitive nature).

If we now try to assess Carnap's ethical approach, we can, I think, clearly see its roots in his grandfather's views on ethics as independent of "dogmatics" and not derivable from any doctrine, religious or otherwise. Ethics belonged for Dörpfeld to the norm-seeking sciences whereas he regarded theology as part of the explaining sciences. In the hands of Carnap, this view underwent, however, a shift, or better, Carnap made explicit what has already been prepared and insinuated by his grandfather. What Dörpfeld called "ethics" is now understood as "religion" by Carnap. I claim however that Carnap's step in this respect is not as new as it might seem at first sight, because religion or ethics depends, for Dörpfeld and for Carnap alike, on the fundamental attitude, on *Gesinnung*, on the conscience. "Intellectual intuition does not suffice to form a moral concept; something else has to come into play [...]: the participation of the *heart and will* through practical energy" (Dörpfeld 1895, p. 43). Without this, religion or ethics does not have any "evidence".

André Carus claims that Carnap's ethical non-cognitivism has a religious origin (Carus 2022). This is true if by "origin" the context is meant in which the concept arose for Carnap. It is, however, not the whole truth. We have seen that there is also a heavy Herbartian strain in the type of non-cognitivism that has come down to Carnap. This context is neither religious nor theological. To understand this fully, it is helpful to distinguish two theses in non-cognitivism (using a terminology that comes close to that used by Herbart and Dörpfeld): 1. Elementary aesthetic judgments (with "good" or "beautiful" or their contraries as basic predicates) cannot be derived from any non-aesthetic factual judgment; 2. Elementary aesthetic judgments possess a normative (non-descriptive) character. It is clear that Herbart understood ethics as consisting of "value judgments" that are primal and cannot be derived from anything else (unambiguously in Herbart 1813, p. 105, § 72, but also in many other places). Whether Herbart really shared the second thesis is not so clear (though probable; see above). It is obvious, however, that Dörpfeld did so when he established the difference between "explaining sciences" dealing with the empirical realm and the "norm-seeking sciences" treating the norms of an ideal. Since he explicitly claimed that an ethical concept involves an "ought" (*Sollen*), it can only belong to the latter realm. The former is reserved for concepts involving an "is" (*Sein*) (Dörpfeld 1895, p. 44 f. For Herbart's rejection of deriving an "ought" from an "is" see Herbart 1825, p. 118, § 119 and Herbart 1828, p. 167, § 96). So, in the end the main difference in ethics between Carnap and Dörpfeld is Carnap's insistence that "value judgments" are not judgments at all because they are not scientific propositions. Dörpfeld (and Herbart) could have responded to Carnap that he thereby illegitimately gives way to psychologism in relation to

value judgments. They are also scientific in the same sense as logical propositions are!

Coming back to the theological context, Carnap's view in his 1911/12 talk (and to some extent also Dörpfeld's view) is not so much different from liberal protestant theology as it existed since about the start of the 19th century. In surveying the role of ethics in different religions, Carnap brings up Friedrich Schleiermacher, one of the important founders of this modern movement, and notes that he is "fully in accordance" with his own conception: "for him religion consists not in knowledge statements, but [...] in a person's stance toward that which is the highest for him" (Carnap 2018, p. 480 f.). Schleiermacher in fact tried to mediate between Pietism (which he was fraught with through his grandfather, as we have seen, as well as through his father) and Enlightenment rationalism and saw religion as characterized by an immediacy of "feeling" or awareness of "absolute dependence" (*Gefühl schlechthinniger Abhängigkeit*). In this he opposed Hegel who insisted on reason as the organ of religion. In addition, he thought and taught that a religion without a personal god is perfectly conceivable.

Carus would perhaps insist that Dörpfeld's (and *a fortiori* Carnap's) ethical non-cognitivism has a religious origin whose remnants were subsequently secularized by Carnap. I would in the first place rather allocate it to an *emancipation from* religion and thus to a strand of Enlightenment and not to religion itself. No wonder then, as Carus writes, "that Carnap's at first religiously motivated non-cognitivism survived its first big hurdle" – the major change of his outlook during the war – and "remained entirely intact in spite of the loss of its former religious foundation" (Carus 2022, p. 153). This is true, or so I claim, because it was *not religious* all along. In the hands of Dörpfeld, non-cognitivist tendencies were from the start oriented towards the *liberation* from, as we would express it today, an oppressive, destructive and authoritarian ideology. This is not to say at all that the Ronsdorf legacy has, so to speak, been magically overcome in all its dimensions by that move.

Despite all the praise of the emancipatory and liberating character of Carnap's (and his forerunners') views on ethics we should not overlook a fundamental limit of this ethical position. The central role that *Gesinnung* or attitude plays in Dörpfeld's and Carnap's ethics immediately reminds one of Max Weber's (1864–1920) distinction between *Gesinnungsethik* ("ethics of attitude", sometimes also translated as "ethics of ultimate ends") and *Verantwortungsethik* ("ethics of responsibility") that he formulated under the impression of the First World War: "[T]here is an abysmal contrast", Weber wrote, "between conduct that follows the maxim of an ethic of ultimate ends – that is, in religious terms, 'The Christian does rightly and leaves the results with the Lord' – and

conduct that follows the maxim of an ethic of responsibility, in which case one has to give an account of the foreseeable results of one's action" (Weber 1919, p. 120). According to Weber, political action must always find a compromise or balance between the two ethical maxims, in adjustment to the individual situation. To follow just one maxim is always detrimental. Weber observed that "Protestantism, especially, legitimated the authoritarian state. Luther relieved the individual of the ethical responsibility for war and transferred it to the authorities" (Weber 1919, p. 124). It is not to be expected that with its peculiar doctrine of predestination the reformed branch of Protestantism scores better in this respect than the Lutheran one. If this protestant attitude were reason enough for Carus to still insist on the religious origin of Carnap's ethics (see above), it would be incorrect to say that non-cognitivism has ever lost "its former religious foundation".

It would, of course, be unjust to blame Carnap for not being a politician, but an ethics, which reduces moral conduct to (a seemingly correct) attitude and fails to contribute to the conciliation of the two maxims is, or so I want to insist, philosophically unsatisfactory. Consequently, one cannot really speak of a political orientation of the Vienna Circle (at least as far as it was dominated by Carnap), despite of the talk of a "left" or "right" wing of the Circle, to which we have accustomed ourselves for some time already. A theory of normative ethical propositions is not enough to build up an ethics in the full sense of the word. And expressions of sympathy with socialism do likewise not suffice to make Carnap's agenda political in the true sense of the word. "All of us in the [Vienna] Circle were strongly interested in social and political progress. Most of us, myself included, were socialists" (Carnap 1963, p. 23).

In a certain sense, Carnap realized this himself when he wrote in 1963 that before WWI "the general trend of our [i.e., of his and his friends'] political thinking was pacifist, anti-militarist, anti-monarchist, perhaps also socialist. But we did not think much about the problem of how to implement these ideals by practical action" (p. 9). Apart perhaps from some texts written towards the end and shortly after the war, there has been little change in this, neither in Carnap's philosophy itself nor in the view of philosophers in the wake of the Vienna Circle. Compared to Carnap, his grandfather was much more political in his writings, on the "social question" for example, and especially on the constitution of the schools.

After having taken note of Dörpfeld's and Herbart's influence on Carnap in ethics, one might be tempted to downplay its overall importance for Carnap: Since Carnap's philosophical strength and prestige lies predominantly outside ethics, one could argue that Dörpfeld's influence (and Herbart's with it) on him is just a limited nostalgic reminiscent of his youthful idealism and

early religious attitude. It is true that outside ethics Herbartian traces are not as pronounced in Carnap's writings, at least not at first sight, but that does not mean that they do not exist.⁴⁷ They are, however, admittedly more difficult to find in his theoretical philosophy than they are in ethics. In the following I simply want to list some similarities I can see of the philosophical thought of the early Carnap with that of his grandfather and with Herbart and Herbartianism by drawing on the material presented above.

We have seen that for Herbart philosophy starts with concepts: philosophy has the task of reworking them. Logic aims to render concepts clear (*klar*) and distinct (*deutlich*): they must be distinguishable from other concepts and have differentiable characteristics. Logic also investigates the different relations in which concepts can stand to each other. The simplest case is the connection of concepts in a judgment as subject and predicate, but also hierarchical subordination and the like. And finally, logic has of course to deal with inferences.

If we ask whether experience, as presented in ordinary understanding, is reliable, Herbart responds that many of the experiential everyday concepts turn out to be inconsistent. The problem of finding true knowledge in the given seems to be thwarted from the start because we do not know where to begin and where to end in our endeavour to find safe ground. A closer inspection, however, reveals, so Herbart, that the inconsistencies in our conceptions are inherited from three faulty basic empirical concepts: the concept of an object with several characteristics, the concept of change and the concept of the ego:

47 Ulrich Lins reports that, at least in 1908, Carnap met the Herbartian Otto Flügel (1842–1914), one of the most prolific writers on and enthusiastic followers of Herbart's philosophy. Carnap seems to have taught Esperanto to Flügel who was well acquainted with Dörpfeld as well as with Carnap's mother and sister (Lins 2022, p. 57. To Flügel and Carnap cp. also pp. 65, 74). (The *Staatsbibliothek zu Berlin* keeps two letters of Dörpfeld to Flügel of 1884 and 1886.) From 1872 on Flügel was the co-editor of the most orthodox journal on Herbart's philosophy, the *Zeitschrift für exacte Philosophie* (1861–1896) and, from its beginning, of the *Zeitschrift für Philosophie und Pädagogik* (1894–1914), which was in a way the successor of the former journal. After Kehrbach's death he took over the editorship of the chronological edition of Herbart's works with volume 11 in 1906. In 1909, Carnap received a booklet on logic as a gift, most probably from Flügel himself, edited and mostly written by Flügel, although it appeared under the name of Allihn (Allihn 1901) (Carnap 2021, p. 433). Friedrich Heinrich Theodor Allihn (1811–1885), Flügel's father-in-law, was also a Herbartian of the first hour and for some time co-editor of the *Zeitschrift für exacte Philosophie*. According to Carnap's reading list for 1908–1919 in Carnap 2021, pp. 432–482, this is the first book Carnap read on logic. The second one was: Frege's *Begriffsschrift* (1910/11) and the third Ernst Schröder, *Vorlesungen über die Algebra der Logik* (1917) (Carnap 2021, pp. 143, 149, 436; 351, 465).

“We will see”, Herbart wrote, “that these concepts, while being forced upon us through experience, cannot really *be thought*; that we cannot *retain* the given as the given in the way it is presented to us and that, as a result of it, we have to rework it in thought and submit it to a necessary adjustment”.

HERBART 1813, p. 147, § 96

The philosophical activity of adjusting concepts in the desired way is called “metaphysics” by Herbart. So, in a way, everyday concepts must be replaced by metaphysical ones before they can be used in a philosophically satisfying way. Dörpfeld was basically on the same path as Herbart as far as the necessity of adjustment is concerned but he took his distance from Herbartian metaphysics in a narrow sense and rephrased the programme in a subtle but far-reaching way:

All concepts related to the world of experience can arise in a natural way [*naturwüchsig*], without intention and without instruction. Their development gets easily stuck half-way or even earlier. The result is confusion or obscurity if not downright falsity. [...] Concepts that have naturally arisen and thus are half-baked are called ‘psychological’; the completed ones that are formed in a workmanlike or appropriate way are called ‘logical’ or ‘scientific’.

DÖRPFELD 1894, p. 11

The last sentence already quite clearly points to the transformation of the programme in the hands of his grandson: concepts do not have to be entrusted to metaphysics in order to be “completed”, but to be fitted into the edifice of unified science. The aim is “to establish a ‘constructional system’ [*Konstitutionssystem*], that is, an epistemic-logical system of objects or concepts” (Carnap 1928, p. 1, § 1/2005, p. 5).

Maybe Dörpfeld and Carnap would not agree with Herbart that our everyday concepts are as inconsistent as he feared they are, but they would all three share the conviction that philosophy is tantamount to a *reform* of inherited concepts – especially concepts directly based on experience. This characteristic is already enough to speak of a unified philosophical tradition from Herbart to Carnap that is sufficiently set apart from other ones. In the final chapter of his book on *Carnap and Twentieth-Century Thought*, Carus aptly referred to the idea of the *coupure épistémologique* of the French philosopher Gaston Bachelard as a simile for Carnap’s distinction between “informal” and “formal” languages (Carus 2007, p. 274): There is an “epistemological discontinuity or

rupture” between the everyday mode of thinking and philosophically enlightened thought, or, as Carnap also called it, between “word-languages” and “artificially constructed symbolic languages”, or “natural” and “artificial” languages.

The 1961 preface to the second edition of his main work *Der logische Aufbau der Welt* shows particularly well Carnap’s affinity with Herbart and Dörpfeld in this respect:

The main problem [treated in the book] concerns the possibility of the rational reconstruction of the concepts of all fields of knowledge on the basis of concepts that refer to the immediately given. By rational reconstruction is here meant the searching out of new definitions for old concepts. The old concepts did not ordinarily originate by way of deliberate formulation, but in more or less unreflected and spontaneous development. The new definition should be superior to the old in clarity and exactness, and, above all, should fit into a systematic structure of concepts. Such a clarification of concepts, nowadays frequently called ‘explanation’, still seems to me one of the most important tasks of philosophy.

CARNAP 1928, p. X/2005, p. IV

This quotation also hints, somewhat covertly, at the differences that can exist in the choice of the “systematic structure of concepts” (*systematisches Begriffsgebäude*) in executing the programme. And indeed, Herbart, Carnap and Dörpfeld differ in this respect even if they agree to the systematic reform of concepts and its dependence on a special system! Herbart intended to incorporate the reformed concepts into a structure of metaphysics (*in his special sense*), but at the same time also into a psychological and pedagogical one (keyword “apperception”). This idea was continued by Dörpfeld who aimed for a more integrated approach to logic, psychology and pedagogy (taking precautions not to fall into the traps neither of psychologism nor of the excesses of Herbartian metaphysics). Carnap’s project finally was to order all concepts of a unified science in such a way that certain concepts expressing the given are taken as basic or primal and all remaining concepts are shown to be explicitly definable by them, with “definition” in the sense of the new logic of Bertrand Russell and Alfred N. Whitehead.

It might be objected that to squeeze Herbart and Dörpfeld with Carnap into one and the same philosophical programme is inappropriate and illegitimate and does not do justice to Carnap’s seminal break with metaphysics and the philosophical tradition. But surely, one cannot infer from Herbart’s use of the term “metaphysics” that he necessarily meant by this the same thing as Carnap does. Carnap once made a distinction between “all those propositions which

claim to represent knowledge about something which is over and beyond all experience” and the effort to arrange the “most general propositions of the various regions of scientific knowledge in a well-ordered system.” Only the former theories are metaphysical, Carnap claimed. The latter actually “belong to the field of empirical science, not of philosophy, however daring they may be” (Carnap 1935, p. 15 f.).

According to this distinction, Herbartian “metaphysics” constitutes a very daring system, or comes very close, if you insist, to a sort of metaphysics in Carnap’s sense. But in order to really do justice to it, one has to investigate the *motives* that impelled Herbart to turn to metaphysics. It is definitely *not* the mindless continuation of a tradition that originated “in confusions due to languages our species has evolved over millennia”. Leitgeb and Carus (from which this is taken) recently also wrote that “[l]anguage choice, for Carnap, was not an end in itself but was rather in the service of freeing ourselves from the distorted perspective on the world imposed on us by our inherited natural languages” (Leitgeb 2020, sects. 1.4 & 1.1). Herbart’s choice of “metaphysical” language was a choice *faute de mieux* for exactly the same liberation: He did not see a better alternative for a reform of our language in the desired way than by letting in some well-dosed metaphysics that could make sense of our experience (i.e., could remove the inconsistencies of our natural language) and serve as a rational basis for our educational system, nay, for our culture in general! Metaphysics is for Herbart the minimal set of propositions over and beyond experience that is absolutely necessary for creating a “well-ordered system” in the sense of Carnap. The Herbartians themselves were the first to criticise the concrete choice of this set undertaken by Herbart and to lively discuss alternatives. Seen from this vantage point, Carnap’s move is the proposal of an *inner-Herbartian* reform: in order to free us from the distorted perspective of the world, Carnap said to his Herbartian fellows, it is enough to use the language of science and no metaphysics is needed.

It is not surprising that a further unifying feature of the Herbart-Dörpfeld-Carnap-programme is the particular role logic plays in it. For Herbart and the Herbartians, the formal conception of logic primarily provided a means to keep any ideological noise at bay: the noise of half-baked psychology, but especially of German Idealism and also of large portions of Kantian philosophy. For Dörpfeld it was above all an effective instrument for wresting ethics from the reach of theology and the church. And for Carnap, of course, it became a weapon against any metaphysical tendencies (now in his sense), especially those coming from the philosophical tradition itself. He strongly expected that philosophers would eventually “not be able to avoid using this penetrating and efficient method [of the new logic] for the clarification of concepts and

the purification of problems" (Carnap 1928, p. xviii/2005, p. xvi, preface to the 1st ed.).

Some of Dörpfeld's ideas carry with them a baffling and almost naïve simplicity, yet at the same time a certain noblesse and breath-taking foresight: I am thinking especially of Dörpfeld's logicism. It is of course possible that he learned about it by reading Gottlob Frege's *Grundlagen der Arithmetik* of 1884 (§ 87) or by some kind of an intermediary between Frege (1848–1925) and him (one could perhaps think of a Herbartian mathematics teacher with a certain philosophical streak like Leo Sachse; see Gabriel 2017, pp. 54–58). Both possibilities do not seem very probable because of the different vocabulary used by Dörpfeld and Frege and by the different scope they see for logic: whereas for Frege only *arithmetic* can be reduced to logic, Dörpfeld (and later Carnap) extends this without further ado to the *whole* of mathematics. In addition, Dörpfeld's defence of logicism is embedded in and dependent on his discussion of evidence – a topic entirely alien to Frege's text. Finally, Frege was a Platonist for mathematics which implies the rejection of a mere if-thenism. At any rate, we must assume that Carnap first learned about logicism through his grandfather *before* he encountered Frege's work.

Dörpfeld also astonishes with the anticipation of the "problem of theoretical terms" as it is called – a problem that seems to have been fully realised by Carnap (and perhaps remembered by him that his grandfather had already treated this problem before) only around 1939 (at least in print). It is the problem that certain concepts of the advanced empirical sciences (the so-called theoretical ones) refer to entities that are not directly observable and thus are not reducible to the given. One is left with the dilemma either to reject these advanced sciences altogether or to weaken the criteria for the scientific character of an advanced theory.

Already in 1884, Dörpfeld used this insight for an ingenious defence of Herbart's psychology that goes approximately like this (Dörpfeld 1884, pp. xviii–xxiii): Herbart's psychology is accused to be unscientific because it rests on a shaky and unscientific metaphysics. Yet metaphysics in the peculiar sense of Herbart (call it M_1) must be distinguished from the fact that certain fundamental concepts in his theory refer to unobservable objects and are thus metaphysical in a second sense (M_2). Now, Herbart's psychology obviously fulfils its explanatory function quite well independently of M_1 . That is, one can adhere to Herbart's psychology even if one rejects M_1 . So M_1 does not put the scientific character of Herbart's theory into doubt. The fact that the theory is metaphysical in the sense of M_2 is also no reason to reject it as unscientific: Many other theories are empirically successful although they also work with hypothetical concepts in the sense of M_2 . They are regularly regarded as scientific, in the

case of physics even paradigmatically so. If this situation does not change, it follows that the dependence of Herbart's psychology on M_2 does not deny its scientific character either. So, the accusation that Herbart's psychology is unscientific because of its metaphysical character does not work. Far from damaging Herbart's psychology, M_1 is even bound to raise its epistemological value: M_1 gives Herbart's psychology a solid philosophical grounding – an advantage that physics still lacks.

In a final remark I want to point out that one can even find not only in Carnap's ethical but also in his theoretical work a remnant of Dörpfeld's peculiar theory of "evidence" and thus an almost faded trace of Pietism. I think that Dörpfeld's "evidence" turns up again at least in Carnap's view of "explication" (Carnap 1950, ch. 1), but perhaps also in the *Aufbau*. An explication of a concept is roughly a process leading to the concept's replacement with a better or more precise one. Carnap calls the original concept that is to be replaced the "explicandum" and the replacement the "explicatum". In a certain manner any purported reform of concepts and their replacement by better ones is an explication. In this sense, also Herbart and Dörpfeld take part in this Carnapian activity. In the *Aufbau* Carnap called this process a "rational reconstruction" and wrote that "[t]he constructional system [as described in the *Aufbau*] is a rational reconstruction of the entire formation of reality, which, in cognition, is carried out for the most part intuitively" (Carnap 1928, p. 139, § 100/2005, p. 158). In 1950 he spoke of the "transformation of an inexact, prescientific concept" into a new, exact and scientific one. As Carus aptly describes in the chapter on "the ideal of explication" of his book already referred to above, Carnap detected something like a "paradox of explication" (my expression, in analogy to the well-known "paradox of analysis" formulated by G.E. Moore): How can we justify the introduction of a new concept to replace an old one? Obviously, the new concept must have a certain close relationship with the old one; otherwise, one could not say that it is better than the old one. At the same time, it must be different from the old one otherwise we would not gain anything new by its introduction. One can express this situation also in another way: The replacement of a concept X by another one Y can be neither an *elimination* of X nor an *explicit definition* of X through Y. In the first case, the replacement would be arbitrary because there is nothing left which Y could be compared with. In the second case, the replacement of X would be a pseudo-replacement because it would only be replaced by itself. Carnap's solution to this is to say that the new concept should fit into a preconceived systematic structure of concepts narrowing down the possibilities of the replacement – at least in 1950 the system to achieve this is "science". We can then decide by comparison whether or not the new concept fits the system better than the old one.

Remember the discussion of Dörpfeld's conception of "evidence" above: He described how a naturally developed subjective conviction or belief is taken as a "mark" or "indicator of truth". The belief or conviction carries with it compelling evidence. The scientific process of "induction" then turns this mark into a second indicator of truth: the objective conditions correlated with the original evidence. Translated into Carnap's scheme of explication this means that the naturally evolved concept from which we depart is replaced with a concept gained by the application of a certain scientific method. But Dörpfeld said even more than Carnap: he claimed that the replacement process is also a replacement of subjective evidence with objective criteria for it, an insight that does not really occur in Carnap. Only in the discussion of 1950 it seemed to dawn on Carnap that something like this takes place and that we cannot dispense with the original "evidence" – an insight that his grandfather had from the beginning. Without the original intuition (*Intuition*, in German) (see the quotation of the *Aufbau* § 100 above), the machinery of explication could not get going: we *always* must compare the new, more objective concept with the old natural and intuitive or "evident" one. In 1950, Carnap wrote that it is indispensable in explication to have an independent understanding of the original concept to be replaced. Compare the following passage that is also quoted by Carus:

Philosophers very frequently violate this requirement. They ask questions like: 'What is causality?', 'What is life?', 'What is mind?', 'What is justice?', etc. Then they often immediately start to look for an answer without first examining the tacit assumption that the terms of the question are at least practically clear enough to serve as a basis for an investigation, for an analysis or explication. Even though the terms in question are unsystematic, inexact terms, there are means for reaching a relatively good mutual understanding as to their intended meaning.

CARNAP 1950, p. 4. Cp. CARUS 2007, p. 278

This is exactly what Dörpfeld meant when he said that ultimately, even in formal logic, we must rely on "evidence", on the "voice of conscience"; that we must start with a creed, a feeling, a certain *Gesinnung*. This is what I see as (part of the) pietistic heritage of Carnap's theoretical philosophy.

Let me summarise the most important aspects in which I see an influence of Herbartianism and Pietism on Carnap: First, the Herbart-Dörpfeld-Pietism ethics syndrome is non-cognitivist (in the current sense) as far as it sees "value determinations" devoid of theoretical and descriptive sense and resting on irreducible acts of valuation originating from *Gesinnung*, i.e., on immediate evidence of approval or disapproval. In this sense it allows for a clear

distinction between “factual questions” and “pure value questions” that is for Carnap the main advantage of non-cognitivism (Carnap 1963, p. 81). Second, Herbart-Dörpfeld’s “reworking of concepts” corresponds to Carnap’s ‘concept engineering’ in explication relying on pre-theoretic intuitions in the sense of Pietism. Third, the programmatic proposal to reform concepts by metaphysical analysis (in Herbart’s sense!) is replaced by the call to reform through logical analysis. Fourth, Carnap shares with Herbart and the Herbartian tradition a sceptical attitude towards the philosophy of German Idealism. Fifth, Herbart-Dörpfeld’s logico-psycho-pedagogical ordering of concepts mirroring apperception is tightened by Carnap to a logico-epistemological ordering (in the *Aufbau*), thus again corresponding to Herbart’s concept-centred approach. Sixth, the high valuation of formal logic, the rejection of a Kantian transcendental logic and the analytic conception of mathematics based alone on logic seems to have been taken over by Carnap from the Herbart tradition.

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In this article I wanted to show that we can find traces of Carnap’s early philosophical heritage (before his Jena years) both in his practical as well as in his theoretical philosophy. I call this the “Ronsdorf thesis” because it was in Ronsdorf (and a little later in the nearby Barmen) that he was especially exposed to these influences. As I said at the beginning, I do not want to argue thereby for a certain dominating impact or superior position of one strain over another. All I wanted to say is that Pietism, Herbart, Dörpfeld: they all left their clear traces in Carnap’s work. I also did not wish to claim that these influences are acceptable without further ado by present-day standards or that they are consistent in themselves. But I wanted to show that one of the most “analytic” philosophers like Carnap, whatever that may mean, is deeply rooted in “continental” traditions, whatever that may mean – in this case at least in the philosophy of Herbart and Herbartianism and in a Pietistic orientation. History of philosophy is not a squiggled ornament around the real stuff, so to say, but a means to create and raise awareness that we are what we are because of our history. Only this insight can translate new thought into comprehensible terms and secure a place for it in tradition.

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Neurath's Anti-correspondentism and Avenarius

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Abstract

Neurath's criticism of the correspondence theory of truth is still a highly controversial topic. Even if new scholarship has been able to somehow rectify a wide-spread image portraying him as a coherence theorist, his stance on the concept of truth continues to be disputed and the exact content of his rejection of any reality talk has not yet thoroughly been explained. In this chapter I seek to clarify Neurath's criticism of the correspondence theory by shedding light upon Avenarius' influence on him.

1 Introduction

Up until recently, it was widely assumed that Neurath's criticism of the correspondence theory of truth eventually led him to adopt a coherentist stance on truth matters. This would mean that his radical rejection of any kind of reality talk implies the adoption of an alternative substantialist account of truth, in which a statement is true if and only if it is coherent with the total set of statements. Recent scholarship,¹ however, has put this assumption into question and drawn attention to the deflationist leanings of Neurath's holistic naturalism². In what follows, I intend to further develop this line of thinking, and argue that Neurath's criticism of the correspondence theory of truth and anti-metaphysical stance amounts to a kind of linguistic version of Avenarius' rejection of introjection, which does not imply any kind of coherentism.

2 Neurath's Anti-correspondentism

Anti-correspondentism may be taken as one of the main features of Neurath's anti-metaphysical stance, it also constitutes one of the most controversial and

1 For a general overview of the new scholarship on the Vienna Circle see Stadler 2015 and Uebel 2007, chapter 1.

2 See Uebel 1991 for the interpretation of Neurath as a naturalist.

intriguing aspects of his thinking. Neurath's somehow vague and radical rhetoric puzzled both his contemporaries and many of his interpreters. In general, however, his clumsy remarks about what it means for a statement to be true were taken as an expression of an undeveloped coherence theory of truth.³ It seems that, in his eagerness to do away with metaphysics, Neurath's radical rejection of any kind of talk about reality eventually led him to an alternative conception of truth and knowledge, that came very close to idealism. Unwittingly, Neurath replaced a metaphysical notion of truth as correspondence with an equally metaphysical alternative.

Such interpretation has recently been disputed. Authors such as Uebel, and Carus⁴ have convincingly shown that one would be much more in line with Neurath's own intentions and writings if one took his statements on truth not as a defence of a substantial theory of truth, but as an undeveloped theory of epistemic justification. However, much remains to be explained about his idiosyncratic and radical anti-correspondentism, such as its underlying assumptions and contextual background.

In what follows, I would like to clarify the roots and content of Neurath's rejection of any reality-talk. My aim is to show that Neurath's anti-correspondentism is closely related to ideas developed by Richard Avenarius about the relation between mental and physical phenomena. Moreover, I argue that an account of Avenarius's influence on Neurath provides further arguments against the idea that he held a coherentist theory of truth.

3 Neurath and Avenarius

Avenarius is often quoted as one of the main influences on the Vienna Circle. One of the founding fathers of empiriocriticism, he is often mentioned as a forerunner of the scientific world conception. However, despite some sort of historical recognition, we rarely see in-depth analysis of the concrete philosophical aspects in which Avenarius exerts influence upon the members of the circle. Whenever he is mentioned, one does not get much more than historical handwaving, along with some very general remarks on how empiriocriticism informed the political and anti-metaphysical attitude of the Viennese group.

3 See Grundmann 1996; Hofmann-Grüneberg 1988 and Woleński 2018. See Schlick 1934 is probably the main historical source for the reading of Neurath as a coherentist.

4 Uebel displays this interpretation in several different papers, in particular see Uebel 2004 and 2007, chap. 4; see also Carus 2019. Mormann 1999 also provides an interesting interpretation of Neurath's takes on truth, but does not explicitly reject the coherentist reading.

Avenarius' work, therefore, seems to have influenced the political and scientific spirit of the group, whilst being less relevant from an epistemological standpoint.

This image, I argue, is incomplete. Especially for Neurath, Avenarius' influence was not epistemically trivial. In fact, accounting for the reception of Avenarius' work is instrumental for understanding some of Neurath's key philosophical contributions. Specifically, I argue that Neurath builds upon Avenarius' work to reject the possibility of comparing statements and reality, thought and object.

Curiously enough, despite the lack of secondary literature on the issue, Neurath himself made it explicit how important Avenarius' views were to him. In *Sociology in the Framework of Physicalism*, for instance, right after rejecting both the possibility of a phenomenalist evidential base for the sciences and the comparison between statements and reality, Neurath admits that his own position has an obvious connection to Avenarius:

In a certain sense the view advocated here starts from a given state of everyday language, which in the beginning is essentially physicalist and only gradually becomes intermixed with metaphysics. Here is a point of contact with the 'natural concept of the world' of Avenarius. The language of physicalism is nothing new as it were; it is the language familiar to certain 'naive' children and peoples. Science is at times discussed as a system of statements. Statements are compared with statements, not with 'experiences', not with a 'world' nor with anything else. All these meaningless duplications belong to a more or less refined metaphysics and are therefore to be rejected.

NEURATH 1983, p. 66

Two different aspects of this quotation are extremely important: (a) Neurath explicitly states that his own thoughts on the status of everyday language resembles Avenarius' idea of *Natural World Conception*; (b) He rejects the comparison of statements with "experiences" and/or "world" as a meaningless "duplication" (*Verdopplung*). This is also a clear reference to Avenarius. As we shall see, the rejection of meaningless duplications is one of the main consequences of Avenarius' critique of introjection – a central part of his psychological work – that lies at the very core of his anti-metaphysical project.

In the following section, we shall present the ideas from Avenarius mentioned by Neurath: (a) The natural world conception and (b) the error of introjection. Avenarius discusses such notions in his 1891 book *Der Menschliche*

*Weltbegriff*⁵. As we shall see, (a) and (b) are intrinsically intertwined and play a key role in his critique of metaphysics: in Avenarius, it is precisely the error of introjection that makes us leave the natural world conception and fall into metaphysical interpretations of experience.

4 Avenarius and Introjection

Avenarius' critique of introjection plays an instrumental role for his general project of grounding psychology as an autonomous empirical science. His *Menschliche Weltbegriff*, mentioned by Neurath, is devoted to the explanation of the necessary starting point of every psychological investigation that does not result in metaphysical errors. This starting point, named by Avenarius the natural world conception, refers to all that is immediately found in experience, or, in his own terminology, the *Vorgefundene*.

In examining the *Vorgefundene*, Avenarius highlights that its basic content displays two components: The Ego and the surrounding environment. These two components hold a relation of mutual functional dependency⁶ to one another. In such primary experience, there is no qualitative distinction between these components. Both the ego and the environment are on the same level, and the interdependency they maintain does not allow us to promote any kind of fundamental dichotomy according to intrinsic properties.⁷ One might be able to talk about a duality in the *Vorgefundene*, but never a dualism:

If the natural concept of the world, as it is presented in experience, includes the relative comparability of thing on the one hand, and after-image and thought on the other, then it excludes with it the absolute incomparability and, therefore, the absolute dissimilarity, the absolute heterogeneity between thing and thought. Here, too, there is a duality within the unvaried natural concept of the world, but no dualism in the principal philosophical sense.

AVENARIUS 1905, p. 13, my translation⁸

5 The Critique of Introjection is also developed in Avenarius 1888–1890 and 1894–1895

6 See Avenarius 1905, p. 10.

7 See Avenarius 1905, p. 9.

8 “Schließt, wie es erfahrungsgemäß ist, der natürliche Weltbegriff die relative Vergleichbarkeit von Sache einerseits und andererseits Nachbild und Gedanke ein, so schließt er damit die absolute Unvergleichbarkeit aus und eben damit die absolute Ungleichartigkeit, die absolute Heterogenität zwischen Sache und Gedanke. Auch hier

Already here Avenarius distinguishes himself from naive representationalism, as defended in modern philosophy (specially Locke). In starting from a purely phenomenal field, he doesn't take subject-object dualism as a primary fact. Only the unitary and whole experience of the *Vorgefundene* – which has components in constant interaction – can count as a primary element of analysis. The derivative distinction between subject and object, as we shall see, is neither fundamental nor necessary. Moreover, this natural world conception does not correspond to the concept of experience itself, but represents the naïve epistemological starting point of inquiry. Providing a reasonable account of the concept of experience is the end goal of his epistemology, not its starting point.

In looking specifically to the basic component “environment”, Avenarius states that it is constituted by two elements, (a) objects that are different from me and hold relations among themselves and (b) other people that behave and express themselves in a manner that resembles my own way of acting and that also hold relations to the other objects. He states:

I, with all my thoughts and feelings, found myself amidst an environment. This environment consisted of a multiplicity of components, which held a multiplicity of dependence relations to one another. Belonging to this environment there were also fellow-people with their multiple assertions; and, to the most part, what they said stood also in a dependence relation to the environment. Furthermore, these men spoke and behaved like me: they answered my questions just like I answered theirs; they sought out various components of the environment or avoided them, they would also either change such components or try to preserve them unchanged; and what they did or failed to do, they described in words and explained their reasons and intentions for deed and omission. All of it in the same way as I usually did, therefore I could not think anything different other than that those fellow-people were beings like me, and I myself was a being like them.

AVENARIUS 1905, p. 4–5, my translation⁹

besteht also innerhalb des unvariieren natürlichen Weltbegriffs wohl eine Dualität, aber kein Dualismus im prinzipiellen philosophischen Sinne.”

9 “Ich mit all meinen Gedanken und Gefühlen fand mich inmitten einer Umgebung. Diese Umgebung war aus mannigfaltigen Bestandteilen zusammengesetzt, welche untereinander in mannigfaltigen Verhältnissen der Abhängigkeit standen. Der Umgebung gehörten auch Mitmenschen an mit mannigfaltigen Aussagen; und was sie sagten, stand zumeist wieder in einem Abhängigkeitsverhältnis zur Umgebung. Im übrigen redeten und handelten die Mitmenschen wie ich: sie antworteten auf meine Fragen wie ich auf

The fellow-people, the passage makes it clear, are taken to be a specific class of the components of the environment because of the set of relations that they hold to the Ego. Avenarius designates them by the letter T, thus completing the triad of components of the Natural world conception (M (ego), E (environment) and T (fellow-people)), all of which can only be relatively distinguished, and not categorically or ontologically differentiated.

From the recognition of the fellow-people as a class of components of the natural world conception, Avenarius identifies the existence of two different elements in primal experience: (a) an empirical element, that is, all the objects that constitute the environment and (b) a hypothetical element, that is, the assumption that the movements and assertions of the fellow-men are somehow like mine, these are not merely mechanical changes, but attain some kind of significance.

Understanding how (a) and (b) interact and, above all, to provide an interpretation of this hypothetical element, says Avenarius, is of extreme relevance.¹⁰ In fact, his whole theory of psychology is based on how to understand the meaningful acts of the fellow-people. According to him, a defective reading of the hypothetical element is the gateway to all kinds of metaphysical errors and could derail the possibility of a science of psychology.

Avenarius claims, that a paradigmatic example of a wrongful interpretation of the hypothetical element, is to take the meaning of the acts of the fellow-people to be connected to something that happens within them. Postulating a kind of inner sphere as the cause of the movements of the fellow-people leads us to a dead end and alienates us from the natural world conception. According to Avenarius, in such interpretation one ends up introjecting the experience into an inner domain of the fellow-people, that was never a part of *Vorgefundene*. The experience of other men is no longer a part of my empirical world, but something hidden and inaccessible. This is the famous error of introjection, that ends up promoting a kind of metaphysical dualism and an array of unsolvable epistemological problems. In particular, it raises the insidious problem of coordinating the private experiences of different subjects.

die ihren; sie suchten die verschiedenen Bestandteile der Umgebung auf oder vermieden sie, veränderten sie oder suchten sie unverändert zu erhalten; und was sie taten oder unterließen, bezeichneten sie mit Worten und erklärten für Tat und Unterlassung ihre Gründe und Absichten. Alles, wie ich selbst auch: und so dachte ich nicht anders, als dass Mitmenschen Wesen seien wie ich – ich selbst ein Wesen wie sie."

10 I owe much of my interpretation of Avenarius to Prof. Chiara Russo Krauss, especially for the way she presents Avenarius' major concern. See especially Russo Krauss 2013, chapters 2 and 3, for a detailed exposition of why providing a reasonable interpretation of other people's meaningful acts was the central problem in Avenarius' mature work.

In order to further scrutinize the introjection error, I shall make use of Russo Krauss' reconstruction of it.¹¹ According to her, Avenarius views the error of introjection in the following train of thought: (i) the acts of the fellow-people are meaningful; (ii) this meaningfulness is not a purely mechanical process (iii) the meaning of their acts should be derived from something that happens in them; (iii) both me and the fellow-people act in the same way; (iv) therefore my movements and statements must also be related to what happens inside of me; (v) every object is primarily given in my internal sphere, which, in turn, is different from the intersubjective world (vi) I only have direct and immediate access to my own representations.

The fundamental error, as we can see, happens when, in trying to account for the meaningful behaviour of other human beings, I try to move from the perspective of M (the ego), to the perspective of T (a fellow-person), while, at the same time, maintaining the point of view of M, therefore creating two egos in the environment.¹²

The main consequences of such reasoning are the following: a) it constitutes a rupture in the domain of the primal experience, that, which was previously unitary b) it gives rise to a series of meaningless duplications that are on the very basis of metaphysical interpretations of experience, both realist and idealist. Avenarius claims that the distinctions between subject and object, internal and external world, phenomena and thing in itself, are all more or less direct consequences of the introjection error, which goes beyond the *Vorgefundene*, and therefore fundamentally deviates from the unitary experience of the natural world conception. Avenarius claims:

11 See Russo Krauss 2019, p. 23–30; see also Russo Krauss, 2013, part 1, chapters 1 and 2.

12 “Now, Introjection can be decomposed in the following moments: 1) M with his ‘thinking’ moves himself to the standpoint of the individual T in sense of its assertions; 2) However, M leaves – what wouldn’t be a risk, since this shift takes place only ‘in Thoughts’ – the individual T and his system C, i.e. CT, on the ‘standpoint’ as it was before; 3) Thus, both in thought and in outcome the standpoints and the System C of the individual T are fused with his ‘thinking’. Result: The System CT, in some way, now has ‘the thinking’” (Avenarius 1905, p. 69, my translation).

“Nun zerlegt sich aber die Introjektion in die folgenden Momente: 1) M mit seinem ‘Denken’ versetzt sich ‘in Gedanken’ auf den ‘Standpunkt’ des Individuums T im Sinne von dessen Aussagen; 2) M beläßt aber dabei – was ohne Risiko ist, da die Versetzung eben nur ‘in Gedanken’ geschieht – das Individuum T und mit ihm das System C desselben, also CT, auf dem ‘Standpunkt’, auf dem es war; 3) So vereinen sich ‘in Gedanken’ die ‘Standpunkte’ und im Ergebnis das System C des Individuums T mit dem ‘Denken’. Resultat: das System CT hat jetzt in irgend einer Weise das ‘Denken!’”

Via introjection, the natural unity of the empirical world is divided into different directions: into an outer-world and an inner-world, into an object and a Subject. Of both comparisons, the first element is still an experiential thing of M; whereas the second element belongs to T: the *inner-world* is the world insofar as it is put inside of T, the Subject is the inner self of T.

AVENARIUS 1905, p. 29, my translation¹³

As an alternative to this view of how and where the experience of the fellow-people occurs in my own experience, Avenarius elaborates a complex system, in which the experience of other people is constituted by a set of relations between processes happening in their brain, the linguistic content their statements and the environment. Explaining how it works, however, is way beyond our present scope. For our purposes, it suffices to see how, according to Avenarius, we should understand language and, more importantly, the meaning of the statements of other human beings.

Avenarius proposes the following interpretation as an adequate understanding of language: (i) my statements have meaning, which is connected to some processes happening in my brain and to the other objects in the environment; (ii) the fellow people express themselves and interact with the environment in the same way as me; (iii) the statements they utter have meaning in virtue of a similar set of relations, i.e. their meaning derives from the relation between observable processes happening in their brain and the objects in the environment.¹⁴

Such an interpretation of the hypothetical element of experience allows us to account for the meaningfulness of the acts of other human beings, without postulating new domains of experience that are not accessible. In conceiving the representation of other people as sets of relations between objects that are empirically accessible to me and the expressions and brain states of the fellow-people, which are also components of my experience, Avenarius is able to give an account of psychological objects, which avoids any appeal to an inner sphere of the representational subject.

13 "Durch die Introjektion ist die natürliche Einheit der empirischen Welt nach zwei Richtungen gespalten worden: in eine Außenwelt und in eine Innenwelt, in das Objekt und das Subjekt. Von beiden Gegenüberstellungen ist das erste Glied noch immer die Erfahrungs-Sache des M; und das zweite Glied gehört dem T zu: die *Innenwelt* ist die Welt soweit sie in T hineinverlegt wurde, das Subjekt ist das *Innere* selbst des T."

14 See Russo Krauss 2019, p. 26.

In my view, this description of language allows Avenarius to work around the problem of coordinating multiple private experiences and also shows how we are able to have empirical access to the psychological states of the fellow-people, thus making scientific psychology possible. It also prevents metaphysical interpretations of the idea of reality like realism and idealism, since it does not lay down ontological dualities whose justification are impossible, given our cognitive limits. The rejection of introjection also implies the denial of the traditional view of knowledge and truth as correspondence with reality since Avenarius rejects the existence of a metaphysical reality that transcends the realm of experience.

In place of the correspondentalist view of knowledge, Avenarius articulates a naturalist conception of knowledge in which the development of thoughts and ideas is contiguous to the biological adaptation of the human brain to the environment. Psychological contents are maintained or eliminated according to their biological adequacy. The adaptation process of the nervous system to its surroundings, moreover, follows a kind of dialectical collective history in which we have: (a) Firstly a natural world conception, which is basically correct, but is not yet well articulated, (b) secondly a metaphysical conception of the world, which is marked by the error of introjection and (c) a scientific conception of the world, which is basically the same as the natural conception of the world, but which has been improved by the insights provided by the history of our metaphysical errors.

Here we can see, that Avenarius not only poses an argument to justify why the correspondence theory is fundamentally wrong, but he also tries to formulate a new conception of knowledge that would not trespass that which is empirically controllable.

Now that we have examined Avenarius' argument against introjection, let us turn to the role it may play in Neurath's rejection of the correspondence theory of truth and knowledge.

5 Avenarius' Relevance for Neurath's Anti-correspondentist Stance

There are several different points here that could be explored. At first glance, Avenarius touches upon a multitude of topics that are very dear to Neurath. Epistemological naturalism, which is the idea that metaphysical speculation is to be eliminated by a correct interpretation of language, the attempt at reformulating our conception of Knowledge and Truth; as well as scientifically

oriented approach to philosophy, are all ideas that come across both authors.¹⁵ However, we will now only focus on Neurath's argument against correspondence and how it resembles Avenarius argument against introjection.

In rejecting any talk about correspondence, Neurath, and in similar fashion Avenarius, admits the existence of a fundamental starting point, which constitutes the limit of all possible human cognition, namely, ordinary language.¹⁶ Unlike Avenarius' *Vorgefundene*, however, Neurath's ordinary language is historically contingent and not foundational.¹⁷ Another fundamental difference between both arguments is that, in taking ordinary language as a primitive and unavoidable medium, Neurath assumes that all cognitively relevant experience is somehow shaped by language, whereas Avenarius speaks of non-mediated experience. However, if we take those differences into consideration, I believe it wouldn't be totally off the mark to say that Neurath's argument against correspondence is a sort of linguistic version of Avenarius rejection of introjection.

In posing ordinary language as the fundamental social and intersubjective medium of cognition, Neurath takes the concept of a reality in itself, which is supposed to exist independently from linguistic practices as utterly nonsensical. In his 1934 reply to Schlick, Neurath made a crystal-clear statement of rejection of any transcendental grounding of knowledge and/or meaning:

Schlick, however, must cling precisely to "the reality" because he operates with the metaphors "prosecutor, defendant, eternal judge", so to speak, without noticing that these metaphors would only be admissible if not all three persons were represented by himself, but if some transcendental superbeing guaranteed.

NEURATH 1983, p. 108

The same goes for the claim of the existence of some sort of private language in which statements are meaningful in virtue of some kind of inner experience of the linguistic actors.

Here Neurath's reasons for rejecting either phenomenalism or the classical ideal of correspondence between thought and object seems to lie, in something very similar to what Avenarius called the introjection error. Neurath's argument, seems to have even the same structure as the one presented by

15 Neurath's naturalist tendencies become quite clear already in Neurath 1931b and specially in his response to Ake Petzäll, cf. Neurath 1936b.

16 Neurath expresses the idea that ordinary language constitutes a universal insurmountable medium in several papers. See especially Neurath 1932 and 1936a.

17 See Neurath 1983, p. 91.

Avenarius. The fundamental error of anyone defending correspondentism consists in: instead of taking everyday language as our starting point (despite the fact of it being socially and historically constrained) and analyzing the meaning of my statements and the ones of other people in accordance to intersubjective control practices which are linguistic and therefore, within my cognitive limits, one introjects the meaning of statements in a mystical inner experience of other men or in an extra-linguistic reality, both of which trespass the limits of empirical controllability. Neurath says:

Language is essential for science; within language all transformations of science take place, not by confrontation of language with a “world”, a totality of “things” whose variety language is supposed to reflect. An attempt like that would be metaphysics. The one scientific language can speak about itself, one part of language can speak about the other; it is impossible to turn back behind or before language.

NEURATH, 1983, p. 54

In another passage, Neurath recognizes that this way of conceptualizing, which tries to articulate everything on a worldly plane, reverts to Avenarius who had already tried to conceptualize cognition and psychology in a totally unmetaphysical/behaviourist manner:

The idea of starting from ordinary statements in the observable field is, in a certain sense, the fulfillment of the program formulated by Avenarius: to choose as one’s point of departure the “natural (initial) notion of the world”, that is “the general conception formed by men who are capable of expressing it”. According to him, this concept of the world is altered on the one hand by ‘psychoses’ and on the other by ‘philosophies’. Here we have an empiricist and even quite a behaviouristic point of view.

NEURATH 1983, p. 150–151

Neurath, like Avenarius, tries to articulate an idea of a universal medium that constrains all of our possible cognitive efforts and, in so doing, tries to dissolve the metaphysical dispute between realism and idealism into language. The subject object distinction, it seems, is not taken to be something fundamental in Neurath, just as it is not in Avenarius, but one distinction that occurs within language according to practical conveniences. In admitting such an Avenariusian starting point, Neurath rejects any philosophical attempt to ground meaning or truth in a domain that would trespass our *Linguistic Vorgefundene*. Both the coherentist idea of correspondence of thought with

itself and the correspondentist idea of correspondence between thought and object, lose any meaning in this context. All that we are left with is our everyday language and possible articulations of it according to practical proposes that allow intersubjective control.

6 Conclusion

In my view, this way of reasoning by Avenarius is essential to understanding what Neurath means when he claims that “statements can only be compared with statements” or “one cannot go back and beyond language”. The main point of his argument is that there is no transcendent sphere that provides grounding for the meaning of propositions or statements, be it some kind of inner experience or an extra sensible reality. In the same way, to utilize any of these transcendental spheres as a criterion or conditions of truthfulness of a statement is equally wrong and, just like the introjection error, gives rise to useless duplications which are way beyond our cognitive limits. Neurath’s anti-metaphysics, in a way, consists in pleading for epistemic modesty since our cognitive starting point does not allow for anything else other than the intersubjective control of linguistic proposals.

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Carnap and Neurath's Electromagnetic Ways to the Scientific World-Conception

Jordi Cat

Abstract

This chapter challenges the predominant historiographic position that accords a philosophical role in the rise of logical empiricism almost exclusively to considerations of relativity theory, and secondarily quantum mechanics, by virtue of their historical character as either modern or revolutionary. It is argued, instead, that the epistemic authority of scientific practices and results, even when rhetorically linked to their historic character, includes electromagnetic theory. Considerations of electromagnetic phenomena and theory, especially Maxwell's equations, help track and understand the specificity and evolution of the views and projects of different members of the logical empiricist movement such as Carnap and Neurath, and also their interactions, surrounded by affinities and marked contrasts. In each case, considerations of electromagnetism tracked their respective disciplinary choices, their epistemological priorities, their methodological standards and, in particular, their conceptions of unity. Also in each case, the role of attention to electromagnetism is embedded in a variety of explicit considerations of the historical character of evidence from scientific practice, whether as a matter of the authority of most recent physics, of its revolutionary character, of the historical self-awareness of participants, of historical change or as part of the widespread historical domain of different scientific practices. In addition, this chapter points to the role of a connected history of the philosophical significance of electromagnetic theory, influential among philosopher-scientists such as Mach, Hertz, Boltzmann, Duhem and Poincaré, all prominent references in the scientific philosophy of logical empiricists.

1 Introduction

Writing on the role of physics in the rise of logical empiricism, Thomas Ryckman has expressed the prevalent view: "Logical empiricism was conceived

under the guiding star of Einstein's two theories of relativity."¹ Early works by Schlick, Reichenbach and Carnap, he continues, "were principally concerned to show how the philosophy of natural science should be necessarily transformed in [relativity theory's] wake."²

I want to suggest that this picture of the relation of philosophy to science and of the path to logical empiricism does indeed some justice to the importance of relativity theory. But it also leaves out the way electromagnetic theory, with its historical formulations, applications and developments, played a role. In particular, it leaves out the role it played in the paths different thinkers such as Carnap and Neurath took to the logical empiricist movement, both in their shared and their differentiating problems and positions.

Prior to the works of Carnap, Frank, Schlick and Neurath, Maxwell's electromagnetic theory had also been the subject of philosophical consideration and criticism in the hands and minds of a number of influential scientist-philosophers such as Hertz, Mach, Boltzmann, Duhem and Poincaré. Their appeal to it by members of the movement of logical empiricism reflected and extended the theory's dual life, its evolving philosophical and scientific significance.

2 Scientific and Meta-scientific Lives of Maxwell's Equations

I begin with the historical background, with an emphasis on its dual theoretical and methodological dimensions.

Maxwell's equations were introduced by James Clerk Maxwell in 1856 to provide a unified mathematical field theory of electrical and magnetic phenomena. In a subsequent development, in 1861, he used them to suggest the identity of light with electromagnetic waves. The result was an even more significant turning point in the history of physics. With it, Maxwell's theory was opening the door to the possibility of reducing optics to electromagnetism. In a more methodological twist, he also linked different mathematical representations of electric and magnetic phenomena to particular mechanical models of the ether. Maxwell called this cognitive and methodological heuristic the method of physical analogy.³ In this way, Maxwell's equations had entered, then, both the history of physics and the history of scientific epistemology and methodology, in fact raising controversies in both.

1 Ryckman 2007, p. 194.

2 *Ibid.*, p. 195.

3 Cat 2001.

In the 1880s, Helmholtz's student Heinrich Hertz provided new experimental evidence supporting the existence of Maxwell's electromagnetic waves and their contiguous propagation of action. Hertz declared his difficulty forming a consistent physical conception of Maxwell's ideas. What was Maxwell's theory? Testing Maxwell's theory alongside Continental alternatives required following Helmholtz's approach and comparing only their mathematical equations and characteristic parameters. And this methodological move required the epistemic decoupling of Maxwell's theory from its supporting mechanical models, and so in his book *Electric Waves*, Hertz famously declared: "Maxwell's theory is Maxwell's system of equations".⁴ This criterion would become part of his parsimonious, reductionistic foundation of mechanics in a positivistic and axiomatic form.

Hertz also gave the physical theory a broader epistemological significance when in the *Principles of Mechanics* he identified a physical theory with a consistent system of appropriate clear symbols or pictures that would denote facts, make observable predictions and be appropriate in the avoidance of arbitrary terms or symbols such as 'force' denoting nothing.⁵ Hertz's foundational ideas and scientific example had wider scientific and philosophical implications. For instance, they resonated in Vienna with Ernst Mach's anti-metaphysical and economy-centered philosophical analysis of science, prompting both thinkers to refer to each other.⁶ Also in Vienna, they had a significant influence on Ludwig Boltzmann and Ludwig Wittgenstein, especially philosophical. Boltzmann, who had followed in the footsteps of Maxwell's researches on molecular theory of gases, had published the first lectures in Germany on Maxwell's electromagnetic theory.⁷ In subsequent, more philosophical discussions that included references to Hertz and Mach, Boltzmann endorsed a restricted standard of models as mental pictures, after Maxwell's analogies, without Mach's or Hertz's phenomenological or monistic axiomatic constructions.⁸

In Germany, Hertz's views also might have provided a key catalyst in David Hilbert's formulation and pursuit of his formalist axiomatic program, first in geometry and subsequently in physics.⁹ In Hertz's and Hilbert's views,

4 Hertz 1892/1893, p. 21.

5 See Hertz 1894, introduction.

6 Cf. Mach 1883, 1885 and 1896.

7 Boltzmann 1891 and 1893.

8 Boltzmann 1899/1974.

9 See Corry 2006 for the suggestion.

Maxwell's equations alone would enter an axiomatic system for physical theory.

Maxwell's theory extended its dual scientific and methodological life in the hands, among others, of French conventionalists such as Poincaré and Duhem. Poincaré led the way with a technical study of Maxwell's theory in 1890 that prompted a more critical one by Duhem in 1902.¹⁰ The critique continued in their more philosophical works. Both Poincaré and Duhem defended a privileged role for intersubjective, historically continuous and invariant mathematical structures. Poincaré pointed to persistent elements in the evolution of mathematical theories of light from the theory of motion to the theory of electricity. Duhem simply denounced its historical discontinuity. Both also appealed to allegedly French methodological standards of simplicity, precision and logical structure to reject Maxwell's disunified collection of provisional and independent but often contradictory constructs. Duhem notoriously declared that model-building was the product of neither reason nor experiment, only of imagination, the proverbial weakness of the English mind.

Not surprisingly, Duhem acknowledged Hertz's separation of Maxwell's equations from Maxwell's models. The interpretation helped Maxwell's electromagnetic theory meet Duhem's logical and, especially in Helmholtz's classificatory theory, also historical standards.

By the end of the century, electromagnetic theory had reached new heights of theoretical and methodological significance. In 1895 Hendrik Antoon Lorentz made a crucial contribution by introducing the so-called Lorentz-Maxwell equations for the electrodynamics and optics of moving bodies. With the equations taken axiomatically, Lorentz's theory broke away from the mechanical world-picture and provided a broader unification of electromagnetism and optics with a new electron theory. It postulated new relations between locations, times and motions and a hypothesis of contraction of electrons when moving through the ether. The growing success of Lorentz's theory explaining new phenomena prompted him in 1900 to speculate on the possibility of reducing gravitation to electromagnetism. As a result, at a conference in Lorentz's honor of the same year, Wilhelm Wien announced officially the project of an electromagnetic world-picture unifying all matter and forces. The proposal was promptly and widely endorsed and enlisted new theorists and experimenters.

Any credibility left for the mechanical world-picture was further shattered by Planck's statistical proposal of the quantum of radiation, also in 1900. The

10 See Poincaré 1890 and Duhem 1902.

quantum, Einstein noticed, also challenged the electromagnetic project. To deepen the crisis, Einstein also showed that the electromagnetic behavior of moving bodies and the kinematic behavior of light conflicted with the laws of mechanics. Following the example of Mach, Hertz and Lorentz, he sought safety and generality in positivism and axiomatics. Specifically, with new axioms he could protect electromagnetic theory from the conflict with mechanics and the burden of Lorentz's hypothesis of physical contraction. It is worth noting that, unlike the earlier axiomatic systems, Einstein's axioms were not the field equations.¹¹ One was the axiomatic principle of relativity, or the invariance of the equations for all inertially moving bodies and observers (that is, independence from uniform motion), and the other was the principle of the absolute constancy of the velocity of light. From them followed a new space-time structure for any theory of matter and radiation, that is, any form of energy (elsewhere I examine the origins of the canonical postulationist presentation).

Along the way, Einstein's electromagnetic path to the theory of relativity ended up placing Maxwell's equations further at the core of modern physics, only now alongside a new image of space and time whose job was to protect them.

3 Logical Empiricism

Where do logical empiricists enter the picture? The older scientifically-trained generation of logical empiricists included Philipp Frank, Otto Neurath and Moritz Schlick. Maxwell's equations and electromagnetic theory were in the focus of Schlick's doctoral thesis of 1904 under Planck in Berlin, and Frank's habilitation of 1908 in Vienna. Frank had studied with Boltzmann in Vienna and with Klein and Hilbert in Göttingen and received a doctorate with a thesis on dynamics. Schlick's dissertation in optics investigated the application of Maxwell's equations to inhomogenous media without ether models.

Frank, Neurath and the mathematician Hans Hahn, among others, met regularly in Vienna between 1907 and 1912 to discuss current issues in science and philosophy. Their readings included recent foundational work in logic, mathematics and physics and the philosophy of science of Mach and French conventionalists such as Duhem, Poincaré and Rey. As Frank reported, much of their discussions revolved around the crisis in physics associated with the failure of the atomistic mechanical worldview and the proliferation of alternative

¹¹ I am grateful to Iulian Toader for this qualification.

systems such as the electromagnetic worldview around electron theory and energetics. In a similar spirit, they paid attention also to the foundational significance of Hilbert's new geometrical paradigm in the axiomatic formulation of mathematics. The group's reception of French new positivism and conventionalism and its more local attention to Mach and Boltzmann cannot and should not be separated from the scientific and methodological attention those philosopher-scientists had given to Maxwell's equations and electromagnetic theory.

In an extended version of this paper, I offer a more detailed discussion of Schlick's and especially Frank's attention to the significance of Maxwell's equations and electromagnetic theory.¹² Here, I focus on Neurath and Carnap.

4 Otto Neurath

The association of Neurath with electromagnetism is more surprising than Carnap's. So, I begin with his particular electromagnetic path to the scientific world-conception. Neither his interest in political economy nor his contributions to logical empiricism can be fully understood in isolation from considerations of physics and technology. The Vienna discussion group only added to his earlier science education.

One context for the relevance of physics and its applications to economics is the history of political economy itself. The so-called Industrial Revolution had taken place on the basis of a new organization of labor and the use of machines and engines. Marx and others promptly analyzed its significance for economic theory.

Neurath even considered valuable the economic role for machines supplementing labor shortages in war economies.¹³ In the same spirit, he didn't omit references to the economic relevance of the steam engine, but he drew special attention to the social and political significance of the use of electricity (already noted by Lenin), especially noting its superior socializing effect and its power to set up networks of production.¹⁴

In the history of economics, also physical as well as organic analogies were dangerously common. Throughout the 1910s, Neurath began introducing mechanical, thermodynamic and engineering analogies to illustrate the distinctive holistic, modal and constructive features of his own model of planned

12 Cf. Cat 2021.

13 See Neurath 1910/2004, p. 169.

14 See Neurath 1925/2004, p. 449; also Neurath 1973, pp. 8–9.

administrative economy. Engines illustrated the challenge of tracking the causal complexity of quality of life. Engineering illustrated the modal consideration of possible designs and the active and constructive dimension of planning: utopias, declared Neurath, are the business of social engineers.¹⁵

The consideration of possibilities was for Neurath part even of the historical methodology of political economy. His view resulted from engaging two different but related debates over unity still ongoing at that time: one, over the relation of the historical and cultural sciences, including economics, to the natural sciences; and the other, over the different economic methods and perspectives. During the same period, Neurath extended his critical investigation to the methodological unity and cooperation in history and chose the case of the history of optics.

He wrote two overlapping papers, 'On the Foundations of the Theory of Optics' and 'On the Classification of Systems of Hypotheses'.¹⁶ It is in these works, especially in the second, that electromagnetic theory and Maxwell's equations play a role. To begin with, Maxwell's theory put an end to the isolation of optics. By the mid-19th century, as Neurath observed, optics entered a unification with electrical theory introduced by Maxwell to order phenomena of electricity and magnetism.¹⁷

Neurath proposed an objective, unifying method of classification in history of science that he modeled after the method of analysis and synthesis in physical theory, also chemistry, even after the algebraic logic of the political economist and mathematician Stanley Jevons – in addition to what he had learned and critiqued from Ernst Schröder's *Algebra der Logik*.¹⁸ Jevons' technique offered a combinatorial mechanical approach to composition applied to duals of conceptual components and their negations. As a result, it could systematically explore and classify realized and unrealized possible combinations. Neurath adopted as elementary notions, periodicity, polarization, interference and diffraction.

But what was the required form of the analyzed theories? Neurath introduced a weighted criterion of physical theory as a system of hypotheses. One criterion, which he attributed to modern physicists including Duhem and Poincaré, gave almost exclusive priority to mathematical form, with a role in logical argument. The alternative granted superior educational and methodological value to the role of imagery and analogy.¹⁹ For the methodological

15 Neurath 1919/1973, p. 151.

16 Neurath 1915/1973 and Neurath 1916/1983.

17 Neurath 1916/1983, 16–17.

18 Cat 2019. I am grateful to Ulf Höfer for the reminder.

19 See Neurath 1915, pp. 102–103, 1916, p. 25.

purpose of looking to actual science, Neurath endorsed the second, which was also Maxwell's own.

Without explicitly endorsing Maxwell's method of analogies or dismissing Duhem's criticism, he declared the heuristic value of analogies to present, guide and extend the imaginable systems of relations; and this, he added, must be done 'purely logically' and by deducing further consequences.²⁰ Here Neurath provided several explicitly Maxwellian examples: (1) Mechanical analogies for electric and magnetic phenomena;²¹ (2) analogies between the large and the small such as the application of Maxwell's equations for electrical fields to the field of electrons;²² and analogies between different fields and their kinds of phenomena such as the ones that led to the successive unification of light, electricity, magnetism and radiating heat, the very achievement attributed to Maxwell's theory.²³

For Neurath the significance of the formal criterion of theory was historical: it tracked changes in the history of science – and not just in the 20th century –, a method he also attributed to Duhem and Poincaré and used to discuss with Frank and others. Accordingly, he also mentioned Hertz explicitly on two accounts, as having developed Maxwell's theory of light²⁴ and as having identified the theory with its mathematical field equations. On this occasion, Neurath also made sure to note that Hertz had justified the identification in Neurath's own Duhemian historical way, on the basis of the convergence and continuity of results.²⁵ He quoted Hertz accordingly, beyond the famous identity statement: "To the question, 'What is Maxwell's theory?' I know of no shorter or more definite answer than the following: – Maxwell's theory is Maxwell's systems of equations. Every theory which leads to the same system of equations, and therefore comprises the same possible phenomena, I would consider as being a form of or special case of Maxwell's theory."²⁶

The collective efforts of the Vienna circle manifesto would give expression to the goal of unity. The manifesto emphasized a rigorous linguistic framework prominently featuring the axiomatic method and logical analysis, and an emphasis on intersubjective, neutral constructed systems of formula with precise symbolic relations.²⁷ Hertz's interpretation of Maxwell's theory met

20 Ibid., p. 25.

21 Ibid., pp. 26–27.

22 Ibid., p. 27.

23 Ibid.

24 Ibid., p. 17.

25 Ibid., p. 29.

26 Ibid.

27 See Carnap, Neurath and Hahn 1929/1973, p. 306.

the new standard and thereby gave it new philosophical significance. Frank made just this point explicit the following year at the first international presentation of the Circle and its movement at the Königsberg Congress on the Epistemology of the Exact Sciences.²⁸

In the wake of the manifesto, Neurath would still make occasional reference to electromagnetic theory, but now to serve the purposes of illustrating and supporting his own views, and marking out differences from the manifesto's ideals.

His well-known proposal was an anti-metaphysical, materialist account of unified language characterized by the interconnected doctrines of syntacticism and physicalism.²⁹

The unified language of empirical science would have to be intersubjective and, from the empirical standpoint, inter-sensory. And such features depended, according to Neurath, on relations of order,³⁰ for instance, in statements of spatio-temporal data, that is, of spatio-temporal order – or 'space-time linkages'³¹ –, so that protocol statements would consider only material things or events in space and time. Neurath sought to enforce the social and scientific requirement of objectivity and to challenge Carnap's reliance on subjective experience in the epistemology of the *Aufbau*. His brand of physicalism also provided a new solution to his old problem of unifying the natural and the human sciences.

To illustrate and support his physicalist doctrine of empiricism, in 'Sociology in the Framework of Physicalism'³² Neurath considered the use of the everyday term 'blue' to report an experience. One way to provide a physicalist, inter-sensory and intersubjective formulation, Neurath suggested, was to have recourse to electromagnetic theory, namely, the physical concept of 'the number of oscillations of electromagnetic waves'.³³ Carnap had introduced the same correspondence in 1923. The appropriateness of the choice was obviously based on Maxwell's theory's reduction to electromagnetic theory of optics and the associated concepts for qualities such as color. The statement 'here is a blue cube' could then be replaced, according to Neurath, by 'a physical formula in which place is defined by coordinates.'³⁴

28 See Frank 1941, p. 10.

29 Cf. Neurath 1931a/1983 and 1931b/1983.

30 Neurath 1931b/1983, p. 62.

31 Neurath 1931a/1983, p. 49.

32 Neurath 1931b/1983.

33 Neurath 1931b, p. 63.

34 Ibid.

But Neurath insisted that the substitution was neither required nor unavoidable, since behavioral descriptions, for instance, could similarly provide acceptable physicalist alternatives.³⁵ In the purified form, electromagnetic theory could now contribute to the physicalist project of empiricism. But, unlike Carnap's, Neurath's use of the example expressed also his anti-reductionistic approach to unity that preserved the value of the physicalistic but otherwise autonomous human sciences.

Also one later appeal to electromagnetic theory in 1936, in 'Individual Sciences, Unified Science, Pseudorationalism,' illustrated his anti-reductionistic approach to unity.³⁶ Now it did so from an extended Duhemian standpoint of methodological holism across different disciplines, not just different hypotheses. In addition to the familiar example of the forest fire, here Neurath mentioned electromagnetic theory much in the way Einstein had introduced it in 1905. The theory, stated Neurath, cannot be empirically 'controlled in isolation,' without predictions integrating statements of different disciplinary sources: "The theory speaks of electric currents that originate when closed conductors and magnetic fields move relative to each other in a certain way whereas a prediction has to speak of a dynamo in a certain laboratory and of the behavior of an experimenter."³⁷

5 Rudolf Carnap

Carnap intended and presented his pre-*Aufbau* works as contributions to the theory of science. In particular, he applied recent formalist, axiomatic and psychological perspectives. The goal was to investigate the sources of physical knowledge in terms of the construction and organizations of concepts, and, derivatively, the evaluation of theories.

In 'On the Task of Physics' ('Über die Aufgabe der Physik'),³⁸ for instance, Carnap investigated the decisions he considered involved in evaluating and selecting physical theories according to principles. Extending the scope of the conventionalism he had encountered in Poincaré and Dingle, he now argued that the relevant decisions concerned, first, three stipulations: a system of space, a system of time and an action law fixing the dynamics and the description of

35 Ibid.

36 Neurath 1936/1983.

37 Neurath 1936/1983, p. 133.

38 Carnap 1923/2019.

the state of the world.³⁹ Second, decisions were required to choose to which of the three stipulations one must apply the conventionalist principle of maximal simplicity and, accordingly, what specific form of the principle should be applied. He then introduced the elements of what he called the ideal physical system or completed construction of physics.⁴⁰ The first is an axiom system that includes the space and time postulates and the action law. The second is an empirical dictionary that translates the descriptions of qualities in the domain of perception and the descriptions of the objects of physical theories associated with the choice of axiom system. The third element, also fixed by the axiom system, is the description of the corresponding physical state of the world at any two points in time.

I want to draw attention to the fact that Carnap required the three elements to accommodate the concepts and laws of electromagnetic theory. Why? Without them, the elements of Carnap's ideal of physics and thereby his own account lacked a necessary credible scientific image of the physical world, one with the epistemic authority of actual science. In the case of the axiom system, he considered three possible kinds.⁴¹ All included Maxwell's equations either as axioms or required theorems, including those systems with Einstein's space-time equations.

If in the first element of the ideal of completed construction of physics, the equations of electromagnetic theory illustrated and grounded the formal structure of the unified ideal of physics, in the second element, they also illustrated and grounded the empirical, phenomenological dimension. Carnap pointed to the case of colors, which would be recognized only within an ordered color system – he mentioned the example of Ostwald's. The corresponding physical object or process is electromagnetic, but it would vary according to the chosen axiom system for physical theory. Thus, for the case of blue, also Neurath's choice, he observed that the color would correspond in the second kind of system to a periodical movement of electrons denoted by the frequency of oscillation.⁴² Similarly with smells – despite the caveat, he noted, of the lack of a clear classificatory system – and sensations of warmth; Carnap associated them, within a system of the second kind, for example, with different properties of electron complexes.⁴³

39 Ibid., p. 211 and 239.

40 Ibid., pp. 221–233.

41 Ibid., pp. 223–227.

42 Ibid., p. 227.

43 Ibid.

In the more systematic discussion in *Physical Concept Formation* (*Physikalische Begriffsbildung*),⁴⁴ Carnap introduced further details and also considered the pervasive issue of unity in the distinction between the natural and the cultural sciences; the latter distinguished by the aim of understanding without general laws. On that issue, he added that, as a matter of theory formation, the unity of physics depended on the theory of electromagnetism, echoing the widespread support of the electromagnetic world picture. Not only had optics and magnetism become parts of the theory of electricity, he noted; in the new atomic model of the physical world (including the quantum postulate), he added, all physical and chemical appearances, with the exception of gravitation, had been either reduced or declared in principle reducible to electromagnetism.⁴⁵ Without it, Carnap insisted in a grander tone, the most important result in the development of physics in the last hundred years, the development of electromagnetic theory in the form of electron theory, would not have been achieved, or with it, a new unified theory of physics.⁴⁶ With this result, he concluded, the theory of electricity had had on physics 'revolutionary impact'.⁴⁷ If considerations of revolution in physics are supposed to enable philosophical change, here is one example, but it is neither about relativity nor about quantum mechanics.

Now, where in the *Aufbau*⁴⁸ is next the theory of electrons and electromagnetic fields? They featured more discreetly in the set of choices of a physical basis. Carnap listed only a selection from the examples of axiom systems for natural laws that he had introduced in 'On the Task of Physics'. The narrower set of available projects still illustrated and supported the conventional nature of the required choice.

In addition, appeal to physical theory in the epistemological or experiential system allows for the objects of perception constructed out of experiences in the autopsychological basis, to be used in the construction of physical objects. Carnap referred to the explication of the physical-qualitative correlation he had offered in the earlier essays I have presented.⁴⁹

The new manifesto had already pointed to the linguistic nature and unity of science. In 'Physics as a Universal Science' ('Physikalische Sprache als Universalsprache der Wissenschaft')⁵⁰ Carnap heeded Neurath's call for

44 Carnap 1926/2019.

45 Cf. *ibid.*, p. 405.

46 *Ibid.*, 407.

47 *Ibid.*, 409.

48 Carnap 1928.

49 *Ibid.*, p. 182, art. 136.

50 Carnap 1931/1995.

physicalism in the proper scientific account of linguistic empiricism. Yet Carnap's choice of a physical language that would capture the role of experience, or protocol language, was much closer to physics than was Neurath's. The context was relatively new, but his proposal in part wasn't. It included the terms for sensory qualities that were either characterized or characterizable in terms of the numerical determinations of physics, of 'a definite value or range of values of a coefficient of physical state' attached to 'a specific set of coordinates (three space, one-time co-ordinates)'.⁵¹ Any acceptable alternatives would have to be reducible accordingly. Among the qualities he considered, he again paid more attention to the visual and the case of color. Since Maxwell's identification of light with electromagnetic waves, the correlation, as he had discussed it in his pre-*Aufbau* essays, required measures of wave oscillations. He was now explicit that the application of science required the mathematical formulation of general laws of nature. Here he explicitly pointed to Maxwell's second equation, linking the spatial distribution of the electric field in the infinitesimal neighborhood of a point and the rate of change of the magnetic field at the same point.⁵²

More importantly, he argued, with Neurath, that the mathematical determination allowed by the equation had the virtue of being both intersubjective and inter-sensory, independent of color perception and visual perception altogether. In fact, the technological arrangement that would make the cross-modality possible involved the use of electricity, so that, by a further application of Maxwell's theory (or a modern development), the information about the set of frequencies associated with a certain color could have its ordering or structural property expressed in the motion of a palpable pointer or the audible frequency of acoustic waves.⁵³

He also pointed out that the formal sameness of content of qualitative and physical representations or propositions was independent of the images and conceptions associated with them.⁵⁴ Now, notice that the rejection of associated images or conceptions and the emphasis on the common numerical determination constitute precisely, as I have already mentioned, the sort of epistemic decoupling that Neurath had identified in Hertz's restrictive conception of Maxwell's theory and that Duhem had noted and approved.

Finally, I want to conclude this brief examination of the enduring and significant role of electromagnetic theory in Carnap's philosophical evolution

51 Ibid., pp. 52–53.

52 See *ibid.*, p. 56.

53 Ibid., p. 60.

54 Ibid., p. 91.

with a reference to *The Logical Syntax of Language*.⁵⁵ Carnap was now walking in lockstep with Neurath's syntacticism as well as physicalism. In the new theory of scientific knowledge, Carnap characterized the logical syntax of science in terms of sets of transformation rules and concluded with an examination of physical language.⁵⁶ Testing physical P-sentences and introducing primitive physical P-terms through the derivation of protocol terms was, following Duhem, a holistic affair, not a single-file chain of logical derivations. To argue the point, he drew new attention to the case of Maxwell's equations.⁵⁷

The argument also illustrated the kind of foundational investigation that should characterize what he called non-metaphysical philosophy, the logic analysis of science. The task he now declared syntactical was the analysis of scientific statements, of so-called language-forms, expressed by formal statements about other statements, that is, in the formal mode. He called these syntactical statements; the others he called descriptive. However, he also warned against assuming that the distinction between the logical or syntactical analysis of science and the specific sciences rests on the distinction between syntactical and descriptive statements.⁵⁸ He offered a detailed example from the empirical sciences, namely, the analysis of Einstein's discussion of Maxwell's equations for moving bodies and the propagation of light. Beginning with Einstein's opening statement, he offered a paraphrase that allowed for the identification of descriptive and syntactical statements. Maxwell's equations, however, appear as primitive and not linguistic rules and, as such, would be retained as a matter of convenience. This view is consistent with his earlier conventionalism, although it weakens the commitment to the formalistic interpretation of the equations adopted by Hertz and Neurath – and Neurath's attention to analogies.⁵⁹

The evolving significance of Maxwell's equations and electromagnetic theory reappears here now in the syntactic analysis, one also of the recent significant new place in Einstein's argument for his special theory of relativity. Attention to relativity, in fact, requires attention to electromagnetic theory. In the earlier discussions, among examples of actual projects of axiom systems, he had recognized the place of Maxwell's equations also in every unified theory of physics based on the space-time structure of general relativity.

55 Carnap 1934/1937.

56 See Carnap 1934/1937, pp. 316–318.

57 *Ibid.*, p. 319.

58 *Ibid.*, p. 331.

59 I am grateful to Iulian Toader for suggesting the qualification.

To conclude, each round of examples illustrated and supported changes in his philosophical evolution. And in Carnap's case, Maxwell's equations track an evolution different from Neurath's, and also their interaction, surrounded by affinities and marked contrasts. In each case, electromagnetism tracked their respective disciplinary choices, their epistemological priorities, their methodological standards and, in particular, their conceptions of unity.

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Reductionism, Structuralism, and Carnap's *Aufbau*

Thomas Uebel

Abstract

This chapter discusses whether the charge of reductive failure that has been levelled against Carnap's *Aufbau* can be deflected by various deployments of the post-Quinean anti-foundationalist interpretation this work has received in recent decades. A negative conclusion appears to be indicated.

1 Introduction

Rudolf Carnap's first major work, *Der logische Aufbau der Welt* (*The Logical Structure of the World*, 1928, hereafter *Aufbau*),¹ is famous for seeking to provide reconstructions of all empirical concepts on a purely phenomenal, “methodologically solipsist” basis. Recent decades have seen a successful reinterpretation of what this project was undertaken for and the question of the success or failure of the reconstruction has receded into the background.² My discussion here addresses the question whether recent re-interpretations of Carnap's *Aufbau* make its methodological solipsism any less problematic than it was on the old empiricist perspective. The first reference point here is W.V.O. Quine's well-known objection that no eliminative definitions of concepts for objects of the perceptual world had been provided, the second is Alan Richardson's observation that mathematical physics was simply assumed in the construction of the intersubjective world. Both findings seem to point to the failure to provide a proper phenomenalist reduction and call Carnap's methodological solipsism into question. I investigate whether these deficits are rendered harmless by the structuralist interpretation of the *Aufbau* or whether methodological solipsism remains problematic, and I argue for a negative conclusion.

1 References to the *Aufbau* are by paragraph; all references solely by paragraph number are to this work.

2 This is not to deny that alternative attempts at broadly *Aufbau*-style reconstructions have been undertaken that seek to avoid standard criticisms. My concern here lies with Carnap's original. For some pertinent comparative remarks see section 6 below.

Given its methodological solipsism, *Aufbau* cannot deliver what the structuralist reconstruction of objectivity requires.

2 The Charge of Reductionism: from Foundationalist to Structuralist Reductionism

Once upon a time, *Aufbau*-related matters were thought to be simple. The *Aufbau* was to provide a foundationalist reduction of scientific discourse in its entirety; Carnap thought he had succeeded (one niggler apart), and nearly everybody believed him until Quine argued otherwise and Carnap conceded the point in the second edition ten years later.³ But the foundationalist story no longer holds up and the structuralist interpretation of his reconstructive efforts has to be taken account of.⁴ Already early in the *Aufbau* such a different story is told and Carnap's apparent endorsement of Quine's reading in later years can be explained away.⁵ Yet one question appears to have remained unanswered still: how far can the replacement of the foundationalist story help in defending the *Aufbau* against the criticism of its reductionist failure?

Apart from demonstrating the unity of empirical science by exhibiting that all of its domains are comprehensible in one language, the aim of the *Aufbau* is demonstrating that "even though the subjective origin of all knowledge lies in the contents of experiences and their connections, it is still possible [...] to advance to an intersubjective, objective world, which can be conceptually comprehended and which is identical for all observers" (§2). A successful simulation

3 For the *locus classicus* of both the criticism and the traditional reading, see Quine (1951) and (1969). For Carnap's concession see his (1961). I neglect Goodman's criticism (1951) which does not engage methodological solipsism and the phenomenalist reduction as such (but only the constitution of phenomenal objects) and refer readers to the discussion in Richardson (1998, Ch. 2); for repairs to Goodman-type problems see also Mormann (1994) and (2009) and Leitgeb (2011, §§4 and 6–7).

4 For authoritative statements of the new structuralist reading see Friedman (1987) and (1992) and Richardson (1998). It is endorsed with qualifications also in Pincock (2005) and Uebel (2007, Ch. 2).

5 In the mid '50s Carnap referred to the *Aufbau* in foundationalist terms (1963, p. 50). Friedman (1992/1999, Pt. IV) suggests that these puzzling passages be read as reporting how the *Aufbau* was (initially) understood in the Vienna Circle, especially by members with a Machian background. Another interpretation has it that Carnap himself did come to see the *Aufbau* that way ca. 1929/30, influenced by the psychologistic interpretation of the *Tractatus* then shared in the Circle and seemingly encouraged by its author. But this discrepancy need not be decided now: what matters is the agreement that foundationalism cannot be held to have been the mover of the *Aufbau* itself.

of human knowledge under carefully controlled thought-experimental conditions is to show that what matters for objectivity is not the relation knowledge claims bear to what they are about (though that matters for their truth), but that their content is expressible in purely structural terms, internally and in relation to other propositions, without any reference to intersubjectively inaccessible manifestations of subjectivity like intuition or meanings seemingly reaching out beyond experience itself. So far, so radically structuralist.

Yet the *Aufbau* is not shy also to announce a far-reaching reductive project. Its main business, "constitution", is defined as the reduction of a concept to more basic concepts: "Because of the transitivity of reducibility, all objects of the constitution system are thus indirectly constituted from objects of the first level" (§2) Therein, of course, lies the motivation for Quine's reading: why would one want to pursue reductionism if not for foundationalist purposes? The answer is easy, however, once the *Aufbau's* radical structuralism is recognized. Reduction to one type of basic element ("elementary experiences", the "objects at first level") and one basic relation ("remembered similarity") would allow the demonstration that "scientific statements speak only of forms without stating what the elements and the relations of these forms are" (§12), for it would eliminate all terminology as shorthand in favour of the basic relation between basic elements: on analysis, all scientific statements translate into myriad iterations of the basic relation and logical permutations thereof and so are wholly structuralized. All objects of science can be given a "definite description through pure structure statements" (§15) so that "each scientific statement can in principle be so transformed that it is nothing but a structure statement" (§16).

By why should one want such radical structuralism-cum-reductionism? Answer: "this transformation is not only possible, it is imperative. For science wants to speak about what is objective, and whatever does not belong to the structure but to the material [...] is, in the final analysis, subjective" (§16). (Note the radical nature of Carnap's structuralism: no non-structural elements must remain if objectivity is to be ensured.) So reductionism is in the program even without foundationalism. It is required, for Carnap planned to build his account of objectivity on it (to mark this important difference, let's call it "structuralist reductionism" as opposed to the more common empiricist "foundationalist reductionism" which was traditionally ascribed to the *Aufbau*).

Yet can Carnap achieve what this structuralist program promises? There is not only the niggler that he noted himself but did not manage to dispose of satisfactorily, the foundedness issue to do with whether the *Aufbau* did, after all, extrude all reference to content even at the very basis of the system, the basic

relation (§§ 153–156). This issue is set aside here.⁶ There is also the question whether at the upper level of the sketch of the system of concepts that was actually executed in the *Aufbau* (not in any variants), Carnap was able to effect the promised reduction of physical object statements into statements about sense experience. Quine’s famous “is at” objection is relevant here. When it comes to constituting the perceptual space-time world, in particular “the assignment of colours to world points and the subsequent constructions”, the *Aufbau* no longer provides definitions in terms of previously defined lower-order concepts, but only indicates “desiderata” to be followed in the construction (§126). As Quine put it, “[t]he connective ‘is at’ remains an added undefined connective; the canons counsel us in its use but not its elimination” (1951, 40). In the “Preface to the Second Edition” Carnap conceded that “without clearly realizing it, I already went beyond the limits of explicit definitions in the construction of the physical world” (1961, VIII). But without explicit definitions, it may be suspected, no pure structure statements are possible.

Put thus blandly, the suspicion is false. The *Aufbau* does not only employ explicit definitions, but also so-called definitions in use and Carnap stated that “the ascension to a new constructional level takes place always through a definition in use” (§40). The latter do not provide a term-for-term replacement but replace sentences (propositional functions) containing the term to be defined with sentences that do not contain it, but which remain extensionally equivalent. As Ka Ho Lam has recently clarified, Quine’s “is at” objection concerns both types of definition. Carnap himself employed “explicit definition” ambiguously and should have conceded rather that no “explicit definition in the wide sense’ (as contra ‘implicit definition’)” is provided at §126 (this includes both explicit and in-use definitions) (2018, 11). But this does not get Carnap off the hook of Quine’s objection, for it remains crucial that no eliminative translation for “is at” is offered. All that the canons supplied allow for are “multiple legitimate but conflicting translations” of phenomenal data into the perceptual world: they all balance the various factors to be considered to some extent but need not even preserve extensional equivalence between themselves, as Lam has also pointed out (2018, 14).

So the construction of our empirical concepts that the *Aufbau* provides for does not provide the required determinacy. This is problematic also in light of

6 What critics consider the biggest problem of the *Aufbau* tends to reflect the interpretation of they prefer. Given his structuralist reading, Friedman (1987) drew attention to the foundedness problem at §§154–156, replacing Quine’s exclusive focus in (1951, 1969) on the problem of the constitution of the perceptual world at §126. For an important new take on the foundedness problem see MacBride (2021).

the structuralist program that the *Aufbau* does pursue. It is not compensated by the fact that Carnap himself announced the reductive shortfall of his provision at the beginning of §122 and then proceeded at §126 in full realization of the underdetermination of the perceptual world by phenomenal data (enacting what Quine in later years called “one of Carnap’s deepest insights” [1984/2008, 126]). Since the problem does not depend on attributing a foundationalist program one may conclude that Quine’s objection remains relevant despite his failure to identify Carnap’s project correctly. A crucial step of its construction remained incomplete. Was it even completable in principle?

3 Reductionism Redux: Atomistic and Holistic Structuralism

Alan Richardson discovered a still different difficulty facing the *Aufbau* as a reductionist enterprise that deserves greater attention than it has received so far. In focus for Richardson is Carnap’s constitution of the intersubjective world, a different quarry from that mined by Quine. The question of failed reductionism is raised so starkly here that we must even ask whether the damage can be limited at all. First recall the structuralist agenda.

Since the stream of experience is different for each person, how can there be even one statement of science which is objective in this sense (i.e., which holds for every individual, even though he starts from his own individual stream of experience)? The solution to this problem lies in the fact that, even though the *material* of the individual streams of experience is completely different, or rather altogether incomparable, since a comparison of two sensations or two feelings of different subjects, as far as their immediately given qualities are concerned, is absurd, certain *structural properties* are analogous for all streams of experience.

§66, orig. emphasis

It was such structural analogies that Carnap set out to build his reconceptualization of objectivity on, but the construction was not a simple matter.

Richardson’s analysis of the procedure of “intersubjectivization”, by which the *intersubjective world* is constituted, focuses on what Carnap himself stressed, namely that a structural analogy does indeed hold between my autopsychological objects and the autopsychological objects I have reconstructed for another person at lower levels, but that “higher up the constitutional hierarchy”, in the constitution of physical objects, these structural analogies break down because the objects constituted by me and by me for

another person do not share all properties (my body is constituted differently by me from how it is constituted by you because it is not your body). This breakdown was compensated for by the introduction of a new kind of descriptive framework to be applied to constituted objects. Carnap decreed: "A one-to-one correspondence holds between the spatiotemporal world of physics in S [my constitution system] and that in s_M [another person's constitution system as constituted by me] in the following way: the spatiotemporal relations which hold for the physical world points in s_M also hold for the corresponding world points in S " (§146, insertions added). This operation established the so-called "intersubjective correspondence".

Intersubjective correspondence makes up for the lack of structurally fully analogous constitution formulas by postulating that the objects constituted by "us" (i.e. by me and others reconstructed by me) are embedded in an identical space-time structure and are therefore brought into congruence. In consequence, objects that are constituted (partly) disanalogously can correspond intersubjectively, for they "represent (in realistic language) 'the same' object, once as it is recognized by me and the other time as it is (so far as I know) recognized by M [the other]" (§146, insertion added). This holds, Carnap stated, for all types of objects. Richardson summarizes: "Utilizing the mathematical structure and the law-governed nature of the world of physics, mapping from one person's world of physics onto another's can be done in a univocal way. The class formed from such objects is the intersubjective object that is available to all agents and that grounds the possibility of intersubjective judgment" (1998, 86).

Note then what Carnap required and indeed called on for his solution to how the objectivity of science is to be accounted for by means of the constitution of the intersubjective world: mathematical physics. Richardson again: "Carnap clearly indicates that it is the extra, superadded formal structure of mathematically expressed physical concepts that is crucial for the intersubjectivity of knowledge" (1998, 89). Note particularly that it was mathematical physics that Carnap here imported, not just mathematics which, given some form of logicism, he counted as presupposed, like logic, by his reconstructive enterprise. Mathematical physics, however, was not constituted from a person's experience but was invoked *deus ex machina*, taken off the shelf ready-made. Should the verdict be that Carnap's *Aufbau* doubles up on problems, now also failing the demands of structuralist reduction on account of his wholesale importation of mathematical physics into his process of intersubjectivization?

It is no excuse that already in the earlier step of the constitution of *the world of physics* from the perceptual world, Carnap introduced wholesale the system of mathematical physics (§136) and it was this move that his intersubjectivization

depended upon: its own legitimacy is in question. To see all that is involved, let's return to Carnap's *still earlier* constitution of the "space-time world" (§125) and of the perceptual world with perceptual things (§§126–135) from it. Note that this perceptual world was constituted by importing space-time as a purely logical object consisting of four coordinates with numerical values from abstract geometry and turning this abstract object into a concrete one by ascribing to its individual space-time points sensible qualities. Here the importation of the Euclidean metric was perfectly legitimate since abstract geometry, like arithmetic, was presupposed by Carnap's constitution theory (it was populating that metric by ascribing colours to world points so as to constitute perceptual things at §126 with the help of mere desiderata that was problematic). However, when Carnap then constituted the world of physics from this perceptual world, he imported the system of mathematical physics wholesale and so, like in the constitution of the intersubjective world, went beyond what he did when he introduced space-time. The world of physics is constituted when "physical state magnitudes" – quantities, not qualities! – "are assigned to the points of the four-dimensional number space" that is space-time (§136). These physical state-magnitudes are required for the formulation of mathematically expressible laws (which in turn allow a determinate and complete description of the world, unlike the observed qualitative regularities). The reason for the wholesale importation of physical theory then is that the precise physical state-magnitudes required for the formulation and testing of determinate laws are not uniquely determined by the assignment of sensible qualities to the space-time points. The physical state-magnitudes are neither constituted by nor reducible to sensible qualities: between the qualities and the quantities there obtains a one-many relation (§136), so uniqueness of reconstruction cannot be achieved by constitutional definition. We note then, at §126 and §136, two distinct constructional steps that invite critical charges of incomplete or unfinished reduction. The problem noted by Quine is not the only one.

Consider now again the indeterminacy left by the non-reductive constitution of the perceptual world at §126 which allowed for different and extensionally non-equivalent ways of satisfying general principles concerning potentially conflicting desiderata. This indeterminacy was gradually reduced by integrating the data of other sense modalities and revising and completing assignments of sense data to world points so as to arrive at the constitution of perceptual things (§134). It was overcome still further at the level of the constitution of the physical world with its mathematically expressed laws, though even here a choice between "different systems of physics" with incompatible physical magnitudes was ultimately required though the world points of the

perceptual world and the world of physics correspond one-to-one (§136). So the constitutions of the three different non-subjective worlds – the perceptual world, the world of physics and the intersubjective world – cannot be viewed in isolation from each other, moreover, the degree of determinacy that finally obtains is due largely to considerations of overall coherence in construction, albeit at the level of perceptual things or higher.

Yet Carnap's appeals to physical theory at §§136 and 146 violate the program of structuralist reductionism. Now on what grounds can we take Carnap's commitment to such a program for granted? As we've seen, Carnap was aware of the reductive shortcomings of the constructions he provided. Should we perhaps think of the process of constitution in §§136 and 146 as involving something like conventional postulation – as Carnap later darkly suggested had been his procedure in the *Aufbau* without “clearly realizing it”? The procedure he employed was, he wrote, “related to the method of introducing concepts through postulates” (1961/1967, viii) which he employed in (1956). However, even though Carnap's retrospective remark referred to his procedure at §126, not to §§136 and 146, it misapplies across the board: if “the method of introducing concepts through postulates” means the introduction of theoretical concepts either by correspondence rules or implicit definition, then Carnap misidentified the procedure used at §§126, 136 and 146 – or at least left utterly unclear how the method applies.⁷

Yet in any case, what are the prospects for a radical rethink? Rather than think of Carnap's moves as violating his own program, how about expanding horizons and thinking of them as bringing into play a second, different mode of constitution? (To be sure, this would need far more explanation than readers of the *Aufbau* have been given). So far our interpretation of the *Aufbau* moved from foundationalism to reductive structuralism, affording a reinterpretation of the reductionism so prominent in play. Should we perhaps move towards dropping any reference to reductionism as a primary characteristic feature, focus solely on structuralism and allow Carnap both reductive and holistic versions of it to pursue his reconstructive goal?

7 Did Carnap think of the lists of desiderata as approximating implicit definitions informally? Both Richardson (2016, p. 7) and Lam (2018, p. 21) find it difficult to make sense of Carnap's retrospective remark. Ricketts notes that it “does not fit the text of the *Aufbau*” (2010, p. 324), but rather relates to the longer-term development of Carnap's views of theoretical terms. Leitgeb suggests that the issue at §126 could be solved by Ramseyfication and points out (2011, p. 296) that already in the *Aufbau* Carnap toyed with the idea – albeit, it must be noted, only with regard to the foundedness problem at §155.

One clear motivation to do so is that the mathematical structure of physics plays an indispensable part in Carnap's account of objectivity: "[T]he necessity of constructing the world of physics rests on the circumstance that only this world, but not the perceptual world ... can be made intersubjective in an unequivocal, consistent manner" (§136). With regard to his importation of the mathematical structures of physics we cannot simply speak of an unwitting violation of the demand of structuralist reductionism, given that Carnap intentionally pursued his structuralist agenda in an explicitly different fashion. At a minimum we should speak of a "substantial tension" between two strategies for substantiating structuralism – as indeed Richardson does, withholding further judgment on the charge of reductive failure (1998, 90).

4 An *Aufbau* for All Seasons? Post-Reductionism Considered

Clearly then, structuralist reductionism is not the only game in Carnap's *Aufbau*: there is also the nonreductive constitution of the world of perceptual things and the holism of mathematical structures. Both structuralist reductionism and the holism of mathematical-physical structures play essential roles in accounting for the objectivity of science. This invites an alternative interpretation of Carnap's undertaking as seeking to overcome the opposition of reduction and systemic holism. Let's consider whether one such interpretation can resolve the *Aufbau* problems which exclusively reductionist structuralism still left us with.

Suppose that the *Aufbau* project was as described above, to provide a logico-linguistic exemplification of the unity of science thesis and to provide an account of objectivity – albeit, and that's the difference, in a *two-part process* of reconstruction. The objectivity of scientific knowledge is owed to the structural nature of its medium, but "structure" does its constitutive work differently at different levels of the constitution system: once by reduction, once by systemic holism.⁸

The first part, the constitution of objects at the lower levels proceeds wholly according to the official plan by explicit definitions and definitions in use. At this still subjective level of pure experience, it was a reductive analysis that was employed to cut through phenomenological vagueness and instability to fix the determinacy of content types to establish constancy across temporal periods.

8 It may be noted that, put in such broad two-step terms, the rescue effort envisaged here parallels Leitgeb's "new" *Aufbau* (2011) which also switches to a non-reductive strategy for the constitution of perceptual world and higher, but the similarity stops at this broad outline.

This was the structure revealed by the “quasi-analysis” of the strictly speaking unanalysable, elementary experiences of the specious present (assuming access to the complete inventory of an individual’s experiences, their content was analysed into iterations and logical permutations of the basic relation of remembered part similarity between any two such experiences). As soon as we reach any of the external worlds, however, the strategy changes and becomes one of systemic holism: here the objects of experience are constituted in their seeming independence of the subject by matching assignments of previously constructed objects onto abstract structures in light of certain desiderata. At the first stage of this second part, we get the experiential perceptual world in place (with help from “logic” in the provision geometrical structures), and then, at the next stage (helped by the provision of arithmetical structures), we build the world of physics and the intersubjective world on top of it. We build the perceptual world by postulating that our assignments of sensory qualities to world points shall maximize the coherence of experience, and the world of physics by overcoming the quantitative indeterminacy of qualitative assignments to space-time points via the postulation that the assignments shall match our best science and then we proceed to the process of intersubjectivization.

So rather than fail the *Aufbau* for its frustrated reductionist ambition, this proposal considers its reconstructive project, to be structuralist in approach but having two components: an atomistic and a holistic one. The former provides a reductive constitutional analysis of the experiential given to regiment pure subjectivity and the latter integrates the objects so constituted ultimately in mathematical structures whose adoption is guided by holistic considerations. Reductively regimented and linked up by postulation to certain nodes in an abstract structure, the subjective thus becomes a cornerstone of objectivity.⁹

A radical reinterpretation of the *Aufbau* project that promises to iron out the tensions, which we saw afflicting it, emerges here. It pre-emptly the antireductionist criticism by forswearing reductionist ambitions precisely where they are problematic. On it no foundationalist agenda could possibly make sense: not only is the *Aufbau*’s remaining reductionism differently motivated, but foundationalism’s whole *raison d’être* is contradicted by the holistic postulation manoeuvres needed to constitute the external worlds. Moreover, the holistic considerations involved in ascribing colours to space-time points in

9 Carnap here followed Ernst Cassirer’s lead who conceived of mathematical physics as purely structural already in *Substance and Function* (1910), which is referred to repeatedly in the *Aufbau* (unlike Russell’s *Analysis of Matter* [1927] which was added to its bibliography but not yet “taken account of” in the text [1928/1967, 347]).

the process of constituting the perceptual world, can be considered informal complements of the importation of mathematical physics in the constitution of the world of physics in so far as they ensure consistency with laws of physics, i.e. the speed of light (§127). Quine's "is at"-objection would no longer hold, for reductionism would no longer be the *sine qua non* of the *Aufbau* but only a local strategy, namely for regimenting pure subjectivity. Instead a conventionalist motive is at work in a structuralism that actively opposes any foundationalism.

Yet before we can celebrate the new proposal, we must consider its plausibility as an interpretation of the *Aufbau*. Is it the case that with a two-track structuralist reconstruction in place the *Aufbau* better withstands standard criticisms and also can weather new ones? The first thing to note here is that while advocates of the proposal would presumably start by noting again that, according to it, Carnap did not attempt what critics like Quine hold to be impossible, namely reduce external world discourse, ordinary or scientific, to talk of private sense data or unanalysed whole phenomenal experiences, they would have to give this line of defence an unusual, perhaps unexpected twist. So far it may have sounded as if the new defence against the charge that no reductions of physical object to phenomenal discourse were achieved conceded that such a reduction is not after all possible and must be replaced with conventionalist stratagems akin to those in physical science. If that were the case, however, the methodological solipsism that informed the way the constitution system of the *Aufbau* was developed would be compromised. For this reason, the defence of the two-track structuralist reading of the *Aufbau* cannot take its talk of "perceptual world" and "world of physics" at face value – on pain of failing to offer a reading of what makes Carnap's *Aufbau* distinctive. To have neither effected nor attempted a phenomenalist reduction would mean the abandonment of methodological solipsism professed there – unless a radical correction of our ordinary understanding of words "perceptual world" and "world of physics" in the *Aufbau* were offered. Indeed, it is precisely this latter path that advocates of the two-track structuralist reading must take, since there is, apart from the alleged failures, no indication in the *Aufbau* that Carnap was not fully committed to the application of methodological solipsism as reflecting not only the epistemic order of objects but also as encompassing the entire domain of empirical discourse.¹⁰

10 Lest my talk of "commitment" here and in similar contexts later be misunderstood, let me state that it refers to the reconstructive methodology professed in the *Aufbau* and is not to be understood as suggesting that Carnap believed the autopsychological language to be the correct one ontologically. Carnap chose the autopsychological basis for his construction system precisely because he took it to reflect the rational epistemic order of human

Accordingly, such advocates would point out that Carnap never pretended to provide more than a simulation of cognition of the physical and intersubjective worlds and that because of this even his appeal to mathematical physics was ultimately harmless.¹¹ Carnap declared that it is not only “the constitutional supplementation of the physical world” and “the constitution of the heteropsychological domain” that “consist *merely in the reorganization of the given* (cf. §140)” and added: “The same holds for the constitution of the intersubjective world” (§148, orig. emphasis).¹² Here Carnap implied that helping himself to the framework of mathematical physics in the *Aufbau*’s constitution of the world of physics and the intersubjective world made no difference to the methodologically solipsistic nature of the reconstruction provided. If this is thought puzzling, then it must be remembered that when Carnap constituted “the world of physics” he constituted it “as a pure world of numbers” (§136).¹³ In other words, the world of physics, *as constituted in the Aufbau*, is not the physical world proper. It would be wrong to say that Carnap treated mathematical physics itself as a logical object, for its numerical values were after all empirically determined. But he clearly used it not as a theory of what it is a theory of, namely the physical world “out there”, but merely as a fully determinate relational *framework* within which he sought to embed what ultimately remained autopsychological objects. What Carnap did was to borrow the concrete relational structure of mathematical physics to organise, as schemata at higher levels of the constitutional system, objects meant to be constituted from his own experiences. Since the “intersubjective coordination” to which the constitution of the intersubjective world is owed likewise builds on the relational order of this “world of physics”, the same must be said of that

cognition (§§54 and 64) and even considered the autopsychological domain “the natural starting point in the epistemic order of objects” (§66).

- 11 It may be wondered how it is possible for me both to speak of the *Aufbau* providing a thought-experimental simulation of human cognition *and* to take Carnap as characterising “the” epistemic order of cognition. The answer is that Carnap distinguished “the actual order of cognition” (§54) from the rational logical structure that underlies knowledge claims actually made and that the *Aufbau* reconstructed. The simulation the *Aufbau* offered was precisely that of reconstructing knowledge claims in a methodologically solipsist manner.
- 12 This was no stray comment. Concerning the domain of the heteropsychological as a whole, he noted that “the entire *experience sequence of the other person* consists of nothing but a *rearrangement of my own experiences and their constituents*” (§140, orig. emphasis; cf. §§145, 147 and 160).
- 13 Ricketts reminds us with reference to *Physikalische Begriffsbildung* (1926, p. 51–52) that Carnap thought of the physical world so reconstructed as “a class of 14-tuples” consisting of the space-time coordinates and ten further types of physical magnitude (2010, p. 318).

constitution of the intersubjective world. In short, both the world of physics and that of people as constituted in the *Aufbau* are but worlds-in-the-image.

In consequence the proposed reading of the *Aufbau* faces a new challenge. The telling objection is not that his *Aufbau* refers to unreduced physical objects – it does not, for the reasons just rehearsed – but that on this two-track structuralist reading the *Aufbau* makes substantive use of an empirical theory that presupposes the existence of objects which, as noted, it cannot reconstruct on its own terms. So even though the nomological framework of mathematical physics into which the autopsychological objects are projected was only used for purposes of simulating cognition of the physical and intersubjective worlds, it remains the case that such simulations proceed with illegitimate means. To see this, we must stress that the reliance on physical theory here is of an entirely different order from Carnap's procedure elsewhere in the *Aufbau* of letting mature science guide one's choice between constitutive formulae for autopsychological objects (§122): here the reliance of the framework of mathematical physics is itself (partly) constitutive of the domain being reconstructed. What makes this reliance troublesome is that the methodologically solipsist project of rational reconstruction must reject the categorical assumptions made by the physical theory which it relies on because it is unable to constitute their equivalents by its own resources. The very idea of object transcendence that the methodologically solipsist rational reconstruction must discount is essential to the physical theories the descriptive and explanatory capacity of which the reconstruction claims to simulate.

Might it be countered here that this objection is void because the methodologically solipsist subject need not know the truth or empirical adequacy of the physical theory relied upon, that all that is required is that it be true or empirically adequate? I don't think so. There is no need to bring subjects and their knowledge into the discussion at all – “The given does not have a subject” (§65) – and without it talk of merely externalist conditions having to be justified seems redundant. Moreover, the ambition was to reconstruct all scientific concepts on the exclusive basis of autopsychological, phenomenal objects and relations obtaining between them. So even if the reconstruction were to be psychologised, an appeal to an externalist condition requiring conceptual resources not reconstructable in terms of phenomenal objects and relations obtaining between them would be illegitimate in light of the reconstruction's own methodological demands.

It is difficult then to escape the conclusion that appeal to mathematical physics is incompatible with the aim of upholding methodological solipsism. The envisaged procedure amounts to the abandonment of the *Aufbau's* claim that a methodologically solipsist base is sufficient for the reconstruction of

human cognition. This undermines the envisaged new reading of the *Aufbau* we are considering and its effort to avoid the need for reductions by stressing the merely simulatory nature of the reconstruction aimed for. Its reconstruction of the physical and the intersubjective worlds does not contradict methodological solipsism directly as one with a realist reading of “world of physics” and “intersubjective world” would, but its reliance on an unreduced mathematical physics is incompatible with it all the same. If this is correct, then far from showing that the failure Quine alleged in the construction of the perceptual world can be set aside, Carnap’s construction of the intersubjective world only adds to his difficulties.

5 Hope against Reason? The Reductionist Ambition Excused

A defender of Carnap’s *Aufbau* project may at this juncture drop the demand for a radical anti-reductionist reinterpretation. This defender will take Carnap at his word and heed his admission “that our kind of construction of physical points and of the physical space is by no means a fully satisfactory solution” (§124). In defence of the *Aufbau* project as a structuralist reductive one – which, we just saw, is required for the methodologically solipsist case to carry – it will now be argued that Carnap was merely “making do,” and that his menu of principles for assigning colours to world points was merely a placeholder, that it was not meant to replace forever the provision of definitions allowing the elimination of terms for our sentences about physical entities in favour of terms for our sentences about methodologically constructed objects. What briefly looked like a bold overcoming of reductionism is but a temporary fix, a holding operation until the real constitutional definition comes along. This *Aufbau* is, as it were, self-consciously incomplete.

The validity of this defence appears to me to be as questionable as that of the two-step structuralist reinterpretation. Of course, there is no denying that Carnap saw himself as engaged in a holding operation in §126. Yet this fact cannot do more than save his reputation as earnest investigator, it cannot save his project. To consider §§126, 136, and 146 holding operations, one must hold out hope for a successful or at least more satisfactory outcome than what the *Aufbau* achieved. What could have Carnap been reasonably hoping for as a solution to the reductive failures so far and why was he mistaken?

That Carnap did entertain hope for a reductive definition of physical concepts (either of the perceptual world or the world of physics) seems clear.

In the following outline we shall give only the construction of the lower levels in [the symbolic language of logistics]. The reason for this does not lie in the fact that the objects of higher type offer particular difficulties of expression for this language, but in the fact that the problem of constructing the higher objects has itself not been solved with precision and that these constructions therefore can be given only in bold outline. As soon as the content of the construction of any object is precisely known, there are no difficulties in the way of a logistical formulation.

§95, insertion added

This is naturally read as indicating that Carnap “remained hopeful” that the formulation of constitutional definitions of the objects of higher type “can eventually be accomplished” (Lam 2018, 17). Yet on closer inspection Carnap here claims only that he is confident that the language of symbolic logic can deal with any type of content one might wish to formulate – once we know what we want to say. But the problem is precisely that we have no idea how such a constitutional definition of physical objects on a methodologically solipsist basis would go. So it is difficult to see reasons for Carnap’s undeniable hope in this remark. Elsewhere the suggestion is that the difficulties were due to the demand that his project be fully consistent with the results of science. “These shortcomings were not so much due to difficulties which arise from some of the unsolved logical problems; rather, they arose from the difficulties and as yet unresolved problems in the individual empirical sciences” (§156). But this also does not offer an adequate response to the question at issue in §126. What empirical theory might provide us with a phenomenalist reduction formula for physical objects?

At the time Carnap believed that while he had as yet been unable to provide precise definitions of physical objects in phenomenal terms, he had good reason to believe that their in-principle definability in these terms was established on logical grounds. To be sure, Carnap himself was less than fully explicit about this in the *Aufbau* and even the critical discussion has only caught up with the facts of the case relatively recently. Help is at hand from Thomas Ricketts’ investigation of Carnap’s “confidence” that “the concepts which figure in our knowledge of the perceptual and physical worlds are explicitly definable by the application of logic to sensory concepts” (2010, p. 312). Carnap’s underlying idea appears to have been that of treating physical concepts as theoretical in relation to phenomenal ones and conceiving of the former as definable

in terms of the latter, at least in principle.¹⁴ Since Carnap did not share his reasons for being so confident, Ricketts explored the issue in the context of Carnap's concurrent work on the logic of axiom systems (work that remained mostly unpublished in his lifetime). What emerges is a complex piece of metalogical reasoning on Carnap's part suggesting that phenomenalist reductions should be possible in principle.¹⁵ But Ricketts also shows Carnap's confidence to be deeply mistaken, namely "based on a faulty understanding of higher-order quantifiers", in particular, the conflation of "assertions of set-existence with assertions of set-definability" (2010, p. 312–313). Very roughly, the argument is this:

Until Gödel showed otherwise, Carnap believed that, not only in first- but also in second-order logic, the categoricity of an axiom system (having only models that are isomorphic) entailed the decidability of all of its theorems (see Awodey and Carus 2001). Since a consistent set of axioms determines a second-order concept (namely the property of satisfying the conditions laid down by the axioms) and since decidability entails definability, Carnap concluded that definitions of this second-order concept in the first-order terms employed in the axioms became available in principle. It was on these supposedly purely logical grounds that Carnap's confidence was based that the perceptual world and the world of physics can in principle be defined in phenomenal terms (given that the empirical and mathematical conditions imposed on the constitution of the perceptual and physical worlds are consistent).¹⁶ Yet this definability in principle comes to nought once it is realized that categoricity does not entail decidability for second-order concepts. Discovery

14 With this Carnap may be seen to have taken a leaf out of Frege and Russell's book who defined a natural number n as the collection of sets with n numbers.

15 To be sure, Carnap at the time had not yet recognized it as a "metalogical" argument as he had not yet recognized the language/metalanguage distinction. The, at the time unpublished, work is Carnap (2000).

16 As Ricketts puts it: Carnap "assumes the consistency of the mathematical and empirical conditions that extant knowledge imposes on the constitution of the physical world, and so assumes the existence of a class of 14-tuples satisfying these conditions. This existence claim is expressible by a higher-order existential generalization inside the constitution system. Moreover, I maintain that Carnap believes that any true existential generalization has true instances expressible in the constitution system. A definition of a class of 14-tuples satisfying all the conditions on the physical world can be extracted from any such instance. Of course, we may not on the basis of the existential generalization actually formulate a definition that constitutes the physical world; but the *definability* of the class is all that Carnap claims in the *Aufbau*. The constitution of the perceptual world, the assignment of colours to locations, can be treated similarly" (2010, pp. 322–323, orig. emphasis).

of this logical error dashes hope for a successful execution of the reductive side of the *Aufbau* program at some future point. Its failure to offer a phenomenalist reduction appears to look ever more principled.

Ricketts' analysis explains both why at the time Carnap held out hope for the definability of physical concepts by phenomenal ones and why that hope is forlorn after all. It further builds the case against taking the *Aufbau* as actually developed as a reasonable holding operation. The inadequacy of the reduction effected was no news to Carnap, we saw, but now we know better than to share his hope for a better solution. While Ricketts' analysis does not prove in full generality that a phenomenalist reduction is logically impossible, it grounds inductive confidence that none will be found. For we may glean from it also that Carnap himself had no idea just how such a reduction would go in concrete terms: he tied his reductionist flag to very general abstract reasoning that turned out to be deeply flawed. Note also that Carnap never brought the reductive program closer to completion, nor has anybody else.

6 Conclusion

The success of Carnap's *Aufbau* project demands a methodologically solipsist, phenomenalist reduction whether it is given a foundationalist or a structuralist interpretation. Yet such a reduction was not forthcoming, neither in the case of the constitution of the perceptual world nor in that of the case of the intersubjective world: ordinary physical object concepts are left unreduced by the former and the latter relies on mathematical physics. Carnap was aware of the insufficiency of his provision (at least in the former case), but held out hope that the reduction could be completed, wrongly believing himself to be in possession of an in-principle metalogical proof of its possibility. Since his methodological solipsism blocks all non-reductionist strategies and since the *Aufbau* also cannot be regarded as a successful holding operation, its reductive failure remains unredeemed.

Note again that it is Carnap's *Aufbau* I am concerned with – and the very version of a constitution system of concepts elaborated there (not the versions he could have but did not elaborate). That recent attempts to revive something like Carnap's project broaden his base considerably in order to retain a chance of success would seem to prove my point. Hannes Leitgeb's *Aufbau* in (2011), which comes closest to Carnap's own while claiming success by contemporary standards, employs experiential tropes (property instances) in place of whole experiences as elements of the system, employs two basic non-formal relations between them in place of the one basic relation of remembered similarity

(plus set-theoretic membership), and replaces co-extensionality with empirical equivalence as adequacy condition on the reconstruction. Importantly, Leitgeb's *Aufbau* abandons Carnap's demand that the reconstruction be purely structuralist, that there be no adversion to the meaning of any non-logical or non-mathematical term for the reconstruction to work (ibid., 296). Even more importantly, Leitgeb's *Aufbau* abandons Carnap's methodological solipsism by remaining neutral about whether its basic elements, the experiential tropes, are to be understood phenomenally or physicalistically (ibid., 283). Note that this neutrality can only be upheld in the constitution of the perceptual world. When it comes to the constitution of the intersubjective world where, as we saw, reliance on mathematical physics is essential, Leitgeb's *Aufbau* only escapes the illegitimate reliance on the ontology of physics – the fate suffered by Carnap's *Aufbau* – by embracing the physicalist reading of its basic elements, a move that is barred to Carnap, of course. Leitgeb discusses Goodman's problems and Quine's problem with the *Aufbau*, but not Richardson's, and so, to be sure, does not discuss the steps leading to the constitution of the intersubjective world, but it is difficult to see how he could avoid this difficulty in another way.¹⁷

Carnap's *Aufbau* has many virtues, but its strategy of methodologically solipsist simulation of human cognition does not appear to be one of them. The *Aufbau's* value lies in having asked what allows for objective knowledge, given a subjective starting point in subjective experience, and in the logical ingenuity displayed in developing a structuralist answer – but not in so far as that answer built on his assumption about the epistemic order of the objects of human cognition. Still, some failures in philosophy are far more instructive than most successes: that of the *Aufbau's* methodological solipsism would appear to be one of the most spectacular and deep ones.

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¹⁷ Another popular contender, Chalmers' scrutability framework (2012) abandons from the start any semblance of retaining the methodological solipsist restrictions Carnap put in place, so its purported similarity to Carnap's *Aufbau* is very superficial.

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PART 2

Physicalism, Protocol-Sentences, and Semantics



Sources of Linguistic Physicalism: Carnap's and Neurath's Views on Autopsychological Sentences, Private Language, and the Nature of Experience

Gergely Ambrus

Abstract

In the chapter I analyse some differences between Carnap's and Neurath's version of physicalism in the early 1930s and their respective grounds for accepting it. One of the major reasons for advocating linguistic physicalism, Neurath suggested, is the intersubjectivity of physicalistic language, a requisite for any scientific language. Carnap, of course, agreed that the language of science must be intersubjective; between 1930 and 1932 a major point of controversy between them had been whether phenomenalistic protocol sentences could be made intersubjective by translating them into physicalistic sentences (reporting the bodily states and behavior of the protocollers corresponding to their protocol judgements).

The evolution of Carnap's and Neurath's physicalist views is a complex issue that has been investigated by Vienna Circle scholarship in much detail. Within this many-faceted story, I want to focus on a particular argument of Neurath's, which, I believe in agreement with Thomas Uebel, ought to be taken as an argument against the possibility of private languages, purporting to show that phenomenalistic languages are not only inapt for being the universal language of science as they are not intersubjective, but also that they are impossible, hence literally senseless. My contention is that Carnap did not accept Neurath's argument and adhered throughout his career to the view that phenomenalistic private languages are possible – even in the 1960s (with this claim, however, I do not intend to challenge the received view that Neurath had a major role in Carnap's physicalist turn).

I shall analyse Neurath's argument in some detail, contrasting it with the verificationist reading of Wittgenstein's private language argument laid out in *Philosophical Investigations*. Furthermore, I shall also present some background assumptions about the nature and knowledge of experience, to which Carnap and Neurath must have been committed to, at least implicitly, as their views regarding private language partly rested on them. Regarding this latter claim, I also hope to show that my contention that such empirical-scientific assumptions also constrained Carnap's views on linguistic,

i.e. philosophical issues can be reconciled with his avowed metaphilosophical view on the separation of linguistic-philosophical and empirical-scientific matters.

1 Introduction

The doctrine of physicalism in the Vienna Circle, according to Feigl's retrospective formulation, comprised of two theses:

- i. Physicalistic language¹ is the universal language of science.
- ii. All scientific laws and facts can be reduced in principle to the fundamental laws and facts of physics.²

And we may add:

- iii. No ontological thesis about the nature of the world had been advocated, according to which "Everything is physical" or "There is nothing over and above the physical", or "Everything supervenes on the physical",³ as held by later-day metaphysical physicalists.⁴

The language thesis (i) was generally accepted in the 1930s by the physicalists of the Vienna Circle, while the nomological reducibility thesis (ii) was disputed. Carnap, for example, regarded it as a possible but as yet unjustified hypothesis; Neurath, however, was against such a reductionist conception at least from 1932 (see his 1932a),⁵ and promoted his own distinct conception

1 I use "physicalistic language" to refer to languages the terms of which denote ordinary physical objects and properties, i.e. observable entities and properties, and "phenomenalistic language" which refer to conscious experiences. Hence, physicalistic language is not the language of physics; its terms do not refer to entities or properties postulated by physical theories, as e.g. "electron" or "charge". "Physical" and "psychological" refer to objects or states which are commonsensically regarded as physical (bodies and planets, being heavy or solid), and as mental (persons, beliefs, desires, perceptions), respectively. Hence, we may use the following formulations: according to linguistic physicalism, reports about both physical and psychological phenomena are to be couched in physicalistic language, according to linguistic phenomenalism in phenomenalistic language.

2 Cf. Feigl 1963, pp. 227–228.

3 For a summary about the contemporary formulations of metaphysical physicalism, see Stoljar 2017.

4 Thus, in line with the general anti-metaphysical attitude of the logical empiricists, Carnap's and Neurath's physicalism does not mean the reduction of "everything", including mental phenomena, to physics.

5 "The development of physicalistic sociology does not mean the transfer of laws of physics to living things and their groups, as some have thought possible. Comprehensive sociological laws can be found, as well as laws for definite narrower social areas, without the need to go back to the microstructure, and thereby to build up these sociological laws from the physical ones" (Neurath 1931b/1983, p. 75). Cf. also Cartwright/Cat/Fleck/Uebel, 1996, Part 3: Unity on the earthly plane, pp. 167–252, esp. pp. 182–188.

of the unity of science that evolved into his encyclopedia project soon. I will concentrate on the language thesis, and within it, on the issue of the physicalistic accounts of autopsychological sentences. In particular, I will investigate the parallel developments of Carnap's and Neurath's views between 1929 and 1932, highlighting the similarities and differences between their physicalisms in general. I will also argue that Neurath proposed a particular argument in favour of physicalism, which may be interpreted as an argument against the possibility of phenomenistic private languages; and further that Carnap did not accept this argument of Neurath and stuck to the view that solipsistic phenomenistic languages are intelligible (though they are not part of the universal language of science).

2 Part 1: Carnap

2.1 Carnap's Views on Autopsychological Sentences

1930 and early 1931: Proto-Universalsprache and Proto-Psychologie. In the beginning of his physicalist period, between 1930 and 1932, Carnap changed his account of autopsychological sentences several times. In 1930 he was already endorsing a universal language doctrine, according to which all scientific statements can be formulated in physicalistic language, including all psychological statements. He gave several lectures promoting this view,⁶ and in 1930 he had already written the first versions of his papers later to be published as "Die physikalische Sprache als Universalsprache der Wissenschaft" and "Psychologie in physikalischer Sprache".⁷ According to these draft papers, dubbed "proto-Universalsprache" and "proto-Psychologie" by Thomas Uebel,⁸ system statements about psychological states must be formulated in physicalistic language, that is, about bodily and behavioral states of persons, while protocol sentences, describing the observations of protocollers verifying the system sentences, ought to be formulated in phenomenistic language. The reason for Carnap's adherence to phenomenistic protocol sentences was his assumption that judgements about autopsychological experiences were

6 "Die Materialistische Basis der Wissenschaft", Sozialistische Arbeitsgemeinschaft, 10 May 1930; "Einheitswissenschaft auf physischer Basis", Verein Ernst Mach, 20 May 1930; "Die Psychologie in Rahmen der Einheitswissenschaft", 28 May 1930. Psychologisches Institut Kolloquium.

7 Carnap 1931/1934 and Carnap 1932a/1959. Henceforth referred as "Universalsprache" and "Psychologie".

8 Carnap RC 110-03-22 ASP and RC 110-03-36 ASP.

epistemically prior to judgements about physical objects. Accordingly, he maintained that – private – phenomenalist languages are intelligible.

1932. *Universalsprache* and *Psychologie*. After substantially revising the drafts, Carnap published “Universalsprache” and “Psychologie” in May and December 1932. In these papers he elaborated the thesis that all *scientific* sentences should be formulated in physicalistic language in detail. According to his new proposal, not only system statements but also protocol statements should be expressed in physicalistic language.

Nevertheless, in “Universalsprache” Carnap also addressed the question of how to analyse the content of perceptual *experiences*, i.e. perceptual reports formulated in phenomenalist language. He suggested that phenomenalist sentences expressing the content of perceptions may have different logical forms: they may refer to atomistic sense data, or to Gestalts or to physical objects, i.e. to medium-size macroscopic material objects and their properties. Carnap did not choose between these options in the paper.

The reason why Carnap discussed this topic at all is somewhat puzzling, considering that his new account required protocol sentences to be physicalistic. This apparent contradiction or tension may be resolved, however, by noting that Carnap required only perceptual reports *erving as protocol statements* to be expressed in physicalistic language. For unless they are physicalistic, they could not be understood intersubjectively, hence they were inapt for scientific purposes as they could not verify system statements. But this does not entail that all phenomenalist sentences are meaningless, or that *solipsistic* phenomenalist languages *per se* are unintelligible (though, of course, phenomenalist languages cannot be part of the universal language of science).

Moreover, according to Carnap, phenomenalist sentences about experiences with a private meaning *can be translated* into *physicalistic sentences* that have a public, intersubjective meaning. For example, the private phenomenalist sentence *p* “Red (is being seen by S) now” is to be replaced with the physicalistic sentence “The body S is seeing red now” to be understood as “S(’s body) is in a red-seeing state now”.⁹ Expounding on what such a translation entails may reduce the *prima facie* implausibility of the suggestion. First, Carnap’s explanation of the claim that the protocol statement *p* “Red (is being seen by S) now”, can be translated into the physical statement *P* “S’s body is in a red-seeing state now” is that the *scientific content* of a sentence is exhausted

9 An objection to such a translation may be that the indexical “now” has also to be translated into physicalistic language, but it is not clear how? I do not address this problem here, and since this is a quotation from Carnap’s text (see *Unity of Science*, p. 87.), it would be misleading to change it.

by what is intersubjectively verifiable. Since “S has a red experience now” cannot be intersubjectively verified, the *scientific content* of the sentence “Red (is being seen by S) now” is nothing over and above “S’s body is in a red-seeing state now”. Therefore, p can be deduced from P and vice versa (the syntactical concept of “the same content” is defined as “reciprocally inferable”).¹⁰ Hence the next question, “What is the relation between having a red experience and being in a red-seeing bodily state?” is meaningless because “S is having a red experience now” has no scientific content, so the question cannot be formulated meaningfully.

It is, of course, intuitive that we *mean something different* by the statements “Red (is being seen by S) now” and “S’s body is in a red-seeing state now” (the former also including that S has a red experience beyond being in a certain bodily state). But this, according to Carnap, is mistaken. The illusion that “Red (is being seen by S) now” has a surplus meaning over “S’s body is in a red-seeing state now” stems from confusing the logical content of the sentence “Red (is being seen by S) now” (which is: S’s body is in a red-seeing state now) with the accompanying image, the content of the accompanying representation (*Vorstellungsgehalt*), (i.e. that S has a red experience).¹¹

Second, it should also be noted that since the physicalistic sentence P involves no reference to S’s experience, *prima facie*, the possibility of such a translation does not require that there be a general parallelism between S’s experiences and bodily states/behavioural dispositions. It is enough if a diachronically constant correlation between certain physical stimuli and bodily states and behavioural responses of S exists. Thus, the availability of such physicalistic translations of psychological sentences does not depend on the capacity to know other persons’ experiences – which would be necessary, if the possibility of translation required the content of psychological sentences to be identified by reference to the experiences of their owners and then paired with the physicalistic sentences, referring the bodily states/behaviour correlated with the experience with which they are to be substituted.¹²

1932. *Über Protokollsätze*. In “Über Protokollsätze” (1932b/1987),¹³ which was a reply to a criticism of Carnap’s account (above) put forth by Neurath in

10 Cf. Carnap 1931/1934, p. 87.

11 Cf. Carnap 1931/1934, pp. 90–91.

12 It is worth noting that I do not intend to suggest that Carnap was an eliminativist or sceptic about experience (in agreement with Uebel 2018). I claim only that the physicalistic translation of psychological sentences need not appeal to a general psychophysical parallelism thesis.

13 Cf. Carnap 1932b/1987, p. 457.

“Protokollsätze” (1932/1983), Carnap set out to investigate several possible formulations of *physicalistic protocol sentences* and opted for Popper’s suggestion, against his own earlier account, and also Neurath’s. But he *still did not* deny that private phenomenalistic languages are possible. In the paper he addressed only the question of what sort of *physicalistic protocol sentences* are best suited for scientific purposes. However, he did provide replies to some objections to the idea that protocol sentences, sentences about perceptual observations, should be formulated in physicalistic language. Let’s see them in turn.

The objection from different meaning. Phenomenalistic protocol sentences seem to have a different meaning than physicalistic protocol sentences, that is, they are not only reporting about bodily states, but also about the perceptual experiences of the observer. Carnap’s answer was the same as in “Universalsprache”, namely that the objection rests on confusing the factual content of protocol sentences (the physical state of the protocoller’s body when in a certain perceptual state), with the accompanying representation of that state (i.e., the phenomenal experiences accompanying the bodily state).

The objection from the individuation of psychological states. Psychological states are *private* in the sense that they are presented only to their subject. How is it then possible to individuate psychological states by physicalistic sentences (i.e. by reports about physical states) the meaning of which is *intersubjective*?

My perception of red, my hunger, my anger are presented only to me but not to my neighbours. However, in physicalism all sentences are intersubjective. Where do we locate the fact about the immutable separation of subjects from each other? This fact should not be denied; but it must be carefully formulated. “S is hungry” is synonymous with “The nervous system of S is in a hunger state”; “S sees red” is synonymous with “The nervous system of S in a red-sensing state”. “Only S is immediately aware of his hunger” means: “Only S is able to make the statement ‘S is hungry’ – directly based on his hunger, i.e., with no physical causal connection with processes outside of S’s body”.¹⁴

Let us analyse Carnap’s proposal for individuating psychological states in some detail.

First of all, it should be noted that Carnap’s aim was not to answer the question “how is it possible that physicalistic sentences that have an intersubjective meaning may refer to private experiences?” Such an objection would be

¹⁴ Carnap 1932b/1987, p. 468.

plausible against a *metaphysical* consciousness-brain state identity theory, but it need not, and did not, concern Carnap, who, of course, did not advocate any such metaphysical physicalist view. Instead, he proposed the *linguistic* physicalist thesis according to which the scientific content of autopsychological reports are bodily states and behaviour.

Thus, Carnap was after something different. He aimed to explain how psychological states can be individuated and discriminated from each other using physicalistic descriptions, by referring to bodily states and behaviour alone. It is not obvious first how to do this. In contrast, individuating psychological states using phenomenalistic terms is quite straightforward. Since, by assumption, each particular experience may belong only to one person, any experience can be individuated by reference to the person who has it. Persons, in turn, could be identified by a set of compresent experiences and by a personal history, i.e. a temporal series of experiences connected by causal and memory relations between them.¹⁵ However, if reports of psychological states cannot refer to experiences, only to bodily states and behaviour, as physicalism suggests, then this way of individuating persons is not available.

Why not, then, identify a psychological state by reference to the bodily state/behaviour it is correlated with? Unfortunately, this is also unavailable, since it presupposes that we directly identify another person's experience and pair it with the bodily state/behaviour with which it is co-instantiated. But such identification of others' experiences is not possible.

Carnap's suggestion is as follows. We identify a psychological state, for example, a particular state of hunger, by reference to the neural state which directly causes (i.e., without involving inference) the verbal utterance "Hunger now" displayed by S's body (mouth), such that the causal process leading from this neural state to the verbal act of reporting takes place exclusively within S's body. This way, a particular psychological state can be identified by reference to that particular bodily state which directly causes the utterance "Hunger now" without referring to the experience of hunger. This means that the psychological state can be identified by relying only on physicalistic sentences.

Furthermore, the particular psychological state can be distinguished from other similar psychological states (e.g. from other states of hunger instantiated by some other person at the same time). For S may utter the same sentence "Hunger now", reporting a psychological state upon perceiving S*'s, another person's bodily state and behaviour characteristic of being hungry. In such a

15 It seems justified that such a broadly Humeian approach to persons could be credited to Carnap.

case, this psychological state can also be identified by reference to the neural state (causing the characteristic bodily state and behaviour of that person) which caused S's utterance. However, the two psychological states can be distinguished from each other, since in the former case S's utterance "Hunger now" was caused by a causal process taking place entirely in S's body, while in the latter part of the causal process takes place externally, involving S*'s body, that is leading from S*'s neural state (through S*'s behaviour and S's observing it) to S's utterance. Therefore, the two hunger states can be distinguished exclusively by processes described by physicalistic sentences.

It is important to see that Carnap's proposal for individuating psychological states still does not undermine the view that phenomenalist autopsychological sentences are meaningful. For while it is true that one cannot formulate the criterion that distinguishes *between* persons in phenomenalist language, this, *prima facie* at least, does not exclude that the use of phenomenalist terms by a particular person referring to his or her *own* experiences may be constant.

2.2 *Carnap's Implicit Views about the Nature of Experience Supporting a Phenomenalist Account of Autopsychological Sentences*

After presenting Carnap's account of autopsychological sentences, I will now discuss some views about the nature of experience and experiential states, which I believe form the background of this account. In my opinion, Carnap was committed to the following theses.

- (1) Experience is logically private.¹⁶
- (2) The individual Given has a diachronically constant nature. In other words, in similar environments and under similar perceptual conditions a person's experiences at different times are phenomenally similar.
- (3) We have reliable direct/introspective capacities to diachronically re-identify our experiences.¹⁷

¹⁶ I argue for this in detail in Ambrus 2020.

¹⁷ A note on the term "nature of experience" and its characterization I ascribe to Carnap (and to Neurath in the next section). It may be objected that, in what follows, I am upsetting the common understanding and self-understanding of Neurath and Carnap by attributing to them substantial metaphysical views, i.e. "theories of experience" or "theories of consciousness" (even if only implicitly). Moreover, I even claim that certain linguistic theses, for example, Neurath's claim that there cannot be a private phenomenal language is based on these "metaphysical views". Hence I may be seen as interpreting Carnap and Neurath in a way that invites the objection that they are inconsistent in their avowed anti-metaphysical approach to philosophy, an objection similar to Armstrong's, who accused linguistic philosophers of being closet-metaphysicians (Armstrong's target was Ryle's logical behaviourism. See Armstrong 1977/2002, p. 80). What I have in mind, though, is less radical, and, I believe, it can be accommodated with Neurath's and Carnap's

First a comment: the thesis of the privacy of experience (either logical or empirical) is, in itself, neutral concerning the question whether private phenomenalistic languages are possible. While Carnap maintained both theses, Neurath accepted the former, but rejected the latter¹⁸ (to be shown later). It is theses (2) and (3) on which the possibility of private phenomenalistic language turns.

An argument for the *logical* privacy of experience, consonant with Carnap's physicalism, is that a particular experience is related only to one body (i.e., to the experienter's "own body"). More precisely: *some* bodily sensations, at least, are localized in some particular body part: for example, a certain pain is a *tooth-ache* or a *head-ache* etc. Since a body part cannot belong to more than one body (except in conjoined twins, but let's put this aside), a particular bodily sensation can belong only to one body.

A plausible objection against such an account is that it is at least logically or conceptually possible that a particular experience of a person correlates with the physical state of some other person's body, not his own, and in such cases it is unclear whether the experience can be individuated by reference to the body the stimulation of which gives rise to the experience, i.e. to the other person's body, if the other person also has an experience of the same kind. Carnap discussed such cases as well. In "Psychologie", he considered the (hypothetical)

metaphilosophical views, that is what they took to be the legitimate goals and methods of philosophy. I maintain that my characterization of their views about "the nature of experience" are not constituents of a metaphysical account, but of an empirical one. Whether we have reliable direct/introspective capacities to diachronically re-identify our experiences, seems to be, partly at least, an empirical question. Whether the phenomenal character of our experiences is diachronically constant or not, also seems *prima facie* empirical. As such, admittedly, they belong to science, not to the proper subject-matter of philosophy, according to Carnap's division. Nonetheless they may constrain in an indirect manner what can be formulated significantly (the investigation of which *is* a major objective of philosophy), hence it is legitimate to consider these issues in adjudicating what sort of languages are possible. To note: Ayer (1954), in his reply to Wittgenstein's private language argument, seems committed to the assumption, at least implicitly that we have a reliable introspective capacity to diachronically re-identify experiences. See e.g. Ambrus 2021. In contrast, the issue whether experience is logically private may be seen primarily as a conceptual (or "grammatical") issue: cf. e.g. Wittgenstein's arguments in the early thirties to the point the feeling another person's pain is logically impossible, that is, a sentence with this content is "forbidden by the Syntax" (cf. Wittgenstein 1964, pp. 89–94). But see also Schlick's contrary view (Schlick 1936, section v, pp. 358–369.) according to which the privacy of experience is only factual; it is not logically impossible to feel others' sensations. Accordingly, I intend to use the term in an innocent, empirical sense, without contradicting Carnap's and Neurath's meta-philosophical views.

18 Cf. Neurath 1931b/1983; 1932/1983.

case in which a person with telepathic capacities feels fear when another person's life is in danger (i.e., when the other person receives stimuli which gives rise to the fear sensation).

However, in his reply to the objection from telepathy,¹⁹ Carnap seems simply to assume that experience is private but does not argue for this claim. Nonetheless, there are arguments which aim to establish that it is conceptually impossible to interpret such cases of telepathy as one person being aware of some other person's experiences. Or, in other words, that it is logically impossible to feel an instance of fear which is numerically identical with some other person's fear. Wittgenstein, for example, between 1929 and 1932, proposed on several occasions that the statement "I feel your pain" is nonsensical and logically impossible. Wittgenstein's main points were that accepting such a description of the situation when my feeling of pain is correlated with another person's bodily pain state would commit one to the – according to him, absurd – assumption that there exist experiences no-one ever has; and moreover, that such a view rests on a false analogy between introspection and the perception of physical objects.^{20,21}

As for the arguments for (2) and (3), Carnap seems to take them for granted. One strong piece of evidence for this is that in the *Aufbau*, the construction system is based on the recollection of similarity, and the capacity to determine the similarity between present and past experiences is direct. One could argue that referring to Carnap's *Aufbau*-views, in the proposed context, is misguided, since he had already moved away from these views in the physicalist period I am investigating. This is, of course, true of many aspects of the *Aufbau*. Most importantly, Carnap gave up the whole idea that the system of scientific concepts should be logically constructed on a methodological solipsist phenomenalist base. But this notwithstanding, there is no indication in the papers

19 Carnap 1932a/1959, pp. 177–179.

20 Cf. Waismann, 1967, pp. 49–50; Wittgenstein 1964/1975, pp. 88–94. See more detailed in Ambrus 2020.

21 It is interesting to note that Ayer (1956) accepted the logical privacy of experience but maintained both the possibility of private and public phenomenalistic languages. However, he interpreted the logical privacy of experience and the publicity of language differently from Carnap. According to Ayer, the logical privacy of experience is a linguistic convention that is based on the empirical fact that we have no direct access to other minds, while the criterion of the publicity of a language is not that its terms refer to public objects, but that the meaning of its words, i.e. the rules of their use, can be made intelligible to others, even if the language was created by a single person, "in solitude"; cf. Ayer 1956, Chap. v (iii) The privacy of experience; (iv) What can we communicate? pp. 226–238; see also Ambrus 2021.

I discussed that Carnap also gave up the idea that we have a direct identifying and reidentifying capacity about the contents of our experiences. It is one thing to assert that the functioning of such a capacity cannot be intersubjectively tested, hence phenomenalist sentences expressing perceptions cannot be intersubjectively verified, and therefore that protocol sentences ought not to be couched in phenomenalist language. But it is another to query whether we have such a capacity, and whether it can be used to ground the meaning of the terms of a private phenomenalist language.²²

3 Part 2: Neurath

According to the received view in Vienna Circle scholarship, Neurath had a major influence on Carnap's physicalist turn. This influence has been discussed in great detail by Thomas Uebel in many of his writings,²³ and also by Juha Manninen.²⁴ I shall strongly rely on these investigations as a background to the issues I discuss in the remaining sections of the paper. In what follows, after setting the context regarding the changes of Neurath's accounts and arguments for physicalism between 1929 and 1932, I will concentrate mainly on what may be called "Neurath's private language argument". I shall analyse this argument in some detail, compare it with Carnap's versions of physicalism, and also discuss whether Neurath's argument may have influenced Carnap. My claim is not that Neurath's multifarious efforts to promote physicalism did not play an important role in Carnap's physicalist turn. Rather, I would suggest more modestly that Neurath's "private language argument" was not accepted by Carnap, and hence that particular idea of Neurath did not contribute to Carnap's opting for linguistic physicalism in the early thirties.

3.1 *Neurath's Views on Autopsychological Sentences*

Neurath's commitment to physicalism emerged much earlier than Carnap's, in the first half of the 1920s (partly originating from historical materialism).²⁵

22 Regarding this issue, I find myself in agreement with Uebel 2018, where he elaborates the claim that private phenomenalist languages were considered possible by Carnap throughout his career, not only in his phenomenalist period, or in the beginning of his physicalist period, as I am investigating here; phenomenalist languages are ineligible only as *scientific* languages. See later also.

23 E.g. in Uebel 1992, 1995, 2007.

24 Cf. Manninen 2002, 2003.

25 Cf. Manninen 2002, 2003.

However, Neurath's arguments that certainly had an impact on Carnap were put forth in the late 1920s. Neurath's reasons for suggesting physicalism in his "Uniformity of the Objects of all Sciences"²⁶ (written around 1930) and in the draft version of his paper "Soziologie im Physikalismus" (1929–30), dubbed "Proto-Soziologie" by Uebel,²⁷ drew upon his central idea and programme of unified science. Accordingly, if psychological reports were solely about behaviour and bodily states, and thus formulated in physicalistic language, then psychology could be incorporated into the framework of unified science. However, in these writings Neurath did *not* suggest that a private phenomenistic language is unintelligible. He claimed only that if psychological statements are formulated in phenomenistic language, then they are not *intersubjectively* intelligible, and therefore they cannot be part of the body of statements of unified science.

From 1931 onwards, however (at meetings of the Vienna Circle in early 1931, in his papers "Physikalismus" (1931), "Soziologie im Physikalismus" (1931) and "Protokollsätze" (1932)),²⁸ Neurath changed his mind, and argued for a more radical physicalism, featuring the claim that private phenomenistic languages are not only inapt for scientific purposes, but also meaningless. He claimed not only that protocol sentences cannot be formulated in phenomenistic language, but also that solipsistic phenomenistic languages – created and used only by one person – are *impossible*. In the following, I reconstruct Neurath's main points in favour of this view.

3.2 Neurath's "Private Language Argument"

In the following passages from "Physikalismus" (1931a) and "Soziologie im Physikalismus" (1931b) Neurath made points which may be read as an argument, albeit rather brief and condensed, against the possibility of private languages.

If someone makes predictions and wants to check them himself, he must count on changes in the system of his senses, he must use clocks and rulers; in short, the man who supposedly is in isolation already makes use of the intersensual and 'intersubjective' language. The forecaster of yesterday and the controller of today are, so to speak, two persons.²⁹

26 "Einheitlichkeit der Gegenstände aller Wissenschaften", Neurath undated/1930/1981.

27 Neurath undated/1930.

28 Neurath 1931a/1983, 1931b/1983, 1932/1983.

29 Neurath 1931a/1983, p. 55.

The exclusion of ‘phenomenal language’ in its present form, which does not seem to be even suitable for ‘predictions’, that is, for what is essential for science, will probably necessitate a number of alterations in the *constitutional (constitutive) system*. Together with this the ‘methodological solipsism’ (Carnap, Driesch) will probably also disappear.³⁰

In laying out my interpretation of Neurath’s points, it is instructive to consult Uebel’s views on these passages. He writes:

This was indeed a private language argument. Against Carnap’s methodologically solipsist protocol language, Neurath argued that a language must be usable by one individual over time, and he derived from this [...] the condition that there be “constancy of use”.

[...]

Neurath did not argue that phenomenal language were logically impossible, Carnap had obviously shown how to construct one. Neurath nevertheless contended that [...] the belief that one’s language use is constant, must be justifiable. But it is not, if phenomenalist languages are being spoken. With intersubjective language, by contrast, it is reasonable to claim that had one’s use become inconsistent as failed attempts at communication would have become ever more frequent and so the inconstancy would have shown up. Since they did not, one’s language use did remain constant.

[...]

Neurath’s new argument (i.e., the private language argument – G.A.), presupposed the view that only physicalistic language provides for public references whose accessibility makes for intersubjective language. (...) Neurath noted about phenomenal language that “it does not seem to be even suitable for ‘predictions’ that is, for what is essential for science”. [...] his meaning was clearly that predictions formulated in the phenomenal language could not be tested by others. What was new [...] that he added an argument which focussed on *what was required for each individual agent to comprehend and systematize their own experience* (It did not focus only on what was required for communication.)³¹

30 Neurath 1931b/1983, p. 65.

31 Uebel 2007, pp. 228–229 (emphasis added – G.A.).

Accordingly, Neurath held that *it may have been the case*, that the use of the terms of a private phenomenalist language by a subject was, in fact, constant; but he asserted that the ‘constancy of use’ of the terms of such a language *could not be verified* either

- (a) intersubjectively, by others,
- or
- (b) subjectively, by the subject him or herself.

As for (a), *the justification of intersubjective unverifiability*: although the use of the terms of a private phenomenalist language by a subject may be diachronically constant, this cannot be verified by others, because of the *logical privacy of experience*. In order to make this point more clearly, it is illuminating to consult Wittgenstein’s discussion of the “beetle in the box” in *Philosophical Investigations*, sec. 293. According to Wittgenstein, it is conceivable that different persons have different sort of beetles in their respective boxes, or that a beetle in a box is constantly changing over time, or even that there is no beetle in someone’s box at all. If the use of the word “beetle” is constant across the different language users, then the word “beetle” cannot refer to a private object.³² Neurath holds, similarly, that because of the logical privacy of experience, psychological reports of a person cannot be about his or her experience, for then the constancy of his or her use of phenomenal terms could not be checked by others. Therefore, a person’s predictions (which are of fundamental importance for science) could not be checked by others either.

As for (b), *the justification of subjective unverifiability*: according to Uebel

Neurath held that even *a solitary thinker* required a system of symbolic representation for the ordering of his experience over time which was of necessity *intersensual and intersubjective*. A phenomenal language does not “come into question” for it does not allow for the mechanisms whereby the constancy of an individual’s language use can be controlled *by the individual himself* allowing predictions to be checked.

And

Once on a solipsistic base, there is no preventing solipsism-of-the-moment.³³

³² See Wittgenstein 1953, sec. 293.

³³ Uebel 2007, p. 228.

The latter claim is intended to ground the former, i.e. that a solitary thinker cannot verify the constancy of use of her phenomenalistic terms, since she is confined to the solipsism-of-the-moment. But what exactly justifies these radical claims?

In my view, we may distinguish two strands (not completely independent of each other) in Neurath's reasoning. According to the first, which may be called *the argument from verification*, the constancy of use of phenomenal terms cannot be verified subjectively, because there is no means or method for determining whether memory representations are correct, *if* the subject can only rely on what is introspectively accessible to her; i.e. if the only test of the subject's memories being correct is that they seem to be so to the subject. According to the second, what we may call *the argument from symmetry*, a person's predictions about the perceptions of her "future self" (i.e. a diachronically distinct person, who is conveyed as identical with her),³⁴ have the same epistemic status as a person's predictions about the experience of another synchronically existing distinct person. The first point is aptly expressed by Uebel's formulation that solipsism leads to solipsism-of-the-moment, and the second by Neurath's formulation that the forecaster of yesterday and the controller of today are, so to speak, two persons.

3.2.1 The Argument from Verification in Detail

According to Neurath, the controller cannot know whether her experience is of the same sort the forecaster predicted, because her senses may change. It is possible that an experience seems to the controller to be an instance of the sort the forecaster predicted, but in fact, it is not; and also, it may seem different to the controller, while it is, in fact, the same. If the only way for the controller to test whether her current experience is of the sort the forecaster predicted that she (the controller) would have, is to rely on that it *seems* to the controller to be so (in other words, if the controller can base her judgement only on that it seems to her that her current experience is the same sort she remembers as appearing in the forecaster prediction), then the controller *does not know* whether her experience is of the same sort the forecaster predicted.

The source of the problem is *not* that memory may not be reliable. It is possible that the memory of the controller is reliable, thus, when the controller

34 I interpret the controller and the forecaster, in line with Neurath's intended use, as two persons existing at different times who are psychologically and bodily connected and continuous, (i.e. that the constituents of their minds and their bodies overlap to some appropriate degree), that is, who would be regarded commonsensically as the same person at different times.

judges that she has the kind of experience the forecaster predicted, she is right (that is, it may be a fact that her use of the phenomenalist term is constant). The problem is rather that if she can rely exclusively on her introspection, then there is no way *of justifying* whether her memory is reliable. The controller cannot justify this, since she cannot distinguish cases in which she remembers correctly from cases in which it only *seems* she remembers correctly.

In order to the controller to know that she is observing what the forecaster predicted, she must be able to identify the content of her observation *objectively/intersubjectively*. She cannot do this by relying solely on the phenomenal content of her perceptual experience introspectively accessed, but only by identifying the external physical objects of her perception, which can be executed only by objective/intersubjective means, by “clocks and rulers”, that is, by the space-time localization of the object of perception. These instruments and the space-time localization, however, already involve reference to public, intersubjective objects, so the content of perceptual experience is already identified by intersubjective notions and language. So, Neurath’s fundamental point is that without relying on external means, the constancy of the meaning of terms cannot be guaranteed, which is tantamount to the claim that even a private, subjective phenomenal language used by a “solitary” thinker, serving only for personal use, is impossible.

Such an interpretation of Neurath’s objection against the possibility of a private phenomenalist language, in my view, anticipates Wittgenstein’s anti-private language argument put forth between sections 243–315 of *Philosophical Investigations*.

258. Let us imagine the following case. I want to keep a diary about the recurrence of a certain sensation. To this end I associate it with the sign “S” and write this sign in a calendar for every day on which I have the sensation. – I will remark first of all that a definition of the sign cannot be formulated. – But still I can give myself a kind of ostensive definition. – How? Can I point to the sensation? Not in the ordinary sense. But I speak, or write the sign down, and at the same time I concentrate my attention on the sensation – and so, as it were, point to it inwardly. – But what is this ceremony for? For that is all it seems to be! A definition surely serves to establish the meaning of a sign. – Well, that is done precisely by the concentrating of my attention; for in this way I impress on myself the connexion between the sign and the sensation. – But “I impress it on myself” can only mean: this process brings it about that I remember the connexion *right* in the future. *But in the present case I have no criterion of*

correctness. One would like to say: whatever is going to seem right to me is right. And that only means that here we can't talk about 'right'.

265. Let us imagine a table (something like a dictionary) that exists only in our imagination. A dictionary can be used to justify the translation of a word X by a word Y. But are we also to call it a justification if such a table is to be looked up only in the imagination? – “Well, yes; then it is a subjective justification.” – But justification consists in appealing to something independent. – “But surely I can appeal from one memory to another. For example, I don't know if I have remembered the time of departure of a train right and to check it I call to mind how a page of the time-table looked. Isn't it the same here?” – No; for this process has got to produce a memory which is actually *correct*. If the mental image of the timetable could not itself be *tested* for correctness, how could it confirm the correctness of the first memory? (As if someone were to buy several copies of the morning paper to assure himself that what it said was true.) *Looking up a table in the imagination is no more looking up a table than the image of the result of an imagined experiment is the result of an experiment.*³⁵

We may briefly reconstruct Wittgenstein's argument as follows.

- (1) The condition for a term having meaning is that there must be a verifiable difference between applying the term correctly and incorrectly.
- (2) If there is no verifiable difference between a present experience *being* of the same sort as some earlier experience S, and it is only *seeming* as if the present experience were of the same sort as S, then the sensation term “S”, putatively referring to S, has no meaning.
- (3) If one can rely only on introspection, then one cannot determine whether a present experience *is* of the same sort as some earlier experience S, or whether *it only seems* so.

Hence

- (4) There is no verifiable difference between using the sensation word “S” correctly or incorrectly.

Hence

- (5) “S” has no meaning.

Neurath's point seems similar. Neurath claimed that if a person wants to determine whether her present experience is of the same sort as some earlier one, and she can rely only on introspecting her experiences, then she has “no

35 Emphasis added – G.A.

controlling mechanisms” to execute this task. This is why a language referring to public physical objects is needed to formulate predictions. Regarding the terms of such a physicalistic language, it can be determined whether they are used with a diachronically constant meaning. If my present use of a physical term turns out to be inconsistent with my earlier uses – for example, when my earlier use of the term ‘table’ was consistent with the use of other language users, but now our uses differ, then it is intersubjectively verifiable that my use of the word is not constant over time.

Now, the moral of Wittgenstein’s argumentation in *Philosophical Investigations* 243 and 258 is similar: to determine whether the use of a word is constant through time (i.e. whether we take it to refer to the same object) is possible only if the word refers to some public, intersubjectively accessible object. Conjuring up memory images of train timetables, in itself, cannot justify my belief about the departures and arrivals of trains. Such memory images may be justificatory only if the content of the timetables is grounded in the objective facts regarding the departures and arrivals of trains. Without such grounding, memories of the timetables cannot establish that I have remembered the departure and arrival times of the trains correctly. Similarly, trying to justify the belief that my current experience is of the same sort as a particular past experience of mine by trying to remember or to imagine other instances of the putatively same experience is useless. Remembering or imagining instances of experiences may be justificatory only if there are some external criteria determining whether in these cases of remembering or imagining, the subject is in the same internal, i.e., experiential, state or not.

3.2.2 The Argument from Symmetry in Detail

What counts in favour of Neurath’s suggestion that the controller is distinct from the forecaster in the same way as two synchronously existing persons are distinct from each other, as concerns their access to the other’s mind?

In my view, the point Neurath wants to make is that the controller has no direct access to the sensations or perceptual experiences of the forecaster (i.e. her “past self”), *just in the same way* as a particular person has no access to the experiences of other synchronously existing persons. A person may have assumptions about another distinct person’s experiences based on the observational situation the other person is in, and she may have imaginative representations of the other person’s experiences. But these, of course, do not provide direct access to the other person’s mind, to her experiences. Similarly, the controller is not in the position to know, based only on her introspectively accessible experiences and memories, whether the experience the forecaster predicted is the same kind of experience the controller is now having. For

the memory representation the controller has of some past experience of the forecaster (her “past self”) provides no direct access to the forecaster’s past experience, just in the same way as a person’s imaginative representations of a distinct and contemporaneous other person’s experiences do not provide direct access to the other person’s experiences either.³⁶

3.3 *Neurath’s Implicit Views on the Nature of Experience Supporting the Physicalistic Account of Autopsychological Sentences*

In my opinion, Neurath’s implicit account of the nature of experience and conscious states³⁷ backing his private language argument may be characterized as follows.

- (1) Experience is private.³⁸
- (2) The individual Given,³⁹ i.e. the phenomenal content of experience, may not have a diachronically constant nature (neither in its monadic nor in

36 Someone may object that we have better access to our past selves than to other minds. The obvious reply from Neurath could have been that this is a difference of degree not kind; access in both cases is indirect and unreliable, hence any such difference is therefore irrelevant. One may press the issue further, however, by arguing that there are decisive – phenomenological – differences between imagining or even “quasi-remembering” (in the sense of Parfit 1971) another person’s experience and remembering, recalling one’s own earlier experiences (see e.g. Schechtman 1994, 1996, 2011 and Casey 1987). For, the argument goes, the content of memories are – in most, or perhaps all cases – personal, which is tantamount to saying that the contents of memory states are auto-noetic: they include that the subject of the remembered experience is identical with the rememberer. But since there is no textual evidence that Neurath was concerned with such considerations, it is not to be expected that his account may provide answers to these possible objections.

37 Concerning the term “nature of experience” or “nature of consciousness”, see my remarks in footnote 17.

38 It may be asked whether experience is logically or empirically private, according to Neurath. In other words: is the proposition that one has no direct access to other minds, and in Neurath’s view, neither to one’s own “past self”, based on empirical or logical impossibility? Carnap, Hempel and Schlick discussed the problem of direct access to other minds in terms of logical possibility or impossibility, and I would venture this was the dominant approach among the logical empiricists. Whether Neurath also subscribed to this view, needs further elucidation. It is also possible that for him the issue was of no importance, or that he held the distinction could not be made precise in principle. In any case, the truth of claims (2) and (3) seems independent of this issue, and Neurath’s argument against private language could be formulated also if we assume that experience is only empirically but not logically private (or else, private *simpliciter*, if this distinction is not to be applied in Neurath’s case).

39 Neurath would have rejected the terminology of ‘Given’ – I am using it only to make the contrast more vivid between Neurath and Carnap, by formulating the views I ascribe to them by using the same terminology.

its relational phenomenal properties). In other words, in similar environments and under similar perceptual conditions a person's experiences at different times may be (substantially) different.

- (3) We do not have a reliable direct, introspective capacity to diachronically re-identify our experiences.

In my understanding, both Neurath's argument from verification and his argument from symmetry rely on (2) and (3). (3) is more fundamental; if we do not have a reliable capacity to re-identify diachronic experience, then the claim that the phenomenal content of experience does not have a diachronically constant nature cannot be true and since it is unverifiable, it has no truth-value.⁴⁰

Clearly, if we had such a capacity (and we could verify whether the phenomenal content of our experiences is diachronically constant or not), then both of Neurath's arguments could be rejected. On the one hand, it would be false to claim that one cannot subjectively verify that a particular present experience is of the same sort as some past experience. If I had a reliable capacity to re-identify diachronic experience then the fact that it *phenomenologically seems* to me that the present experience is like the earlier experience would indeed *justify* my judgement (its *seeming* to me *in the judgemental sense*) that the present experience is of the same sort as the earlier experience. Such a verification is not conclusive, of course. Judgements based on such a capacity to compare and determine the identity of qualities of experiences at different times are not infallible, but they may be reliable.

Furthermore, if we had a reliable capacity to re-identify diachronic experience, then the symmetry thesis would not stand either. For then, in the diachronic case, there existed a reliable epistemic mechanism that would ground the controller's judgement about the completion of the forecaster's prediction; and the controller would be justified in her judgement about the phenomenal similarity of her present experience to the past experience of the forecaster. At the same time, in the synchronous case, a person's imaginative representation would not provide such a reliable epistemic mechanism for obtaining knowledge about another person's experience. Hence, such judgements about other persons' experience would not have the same epistemic status as the controller's judgements about the completion of the forecaster's predictions.⁴¹

40 This phrasing does not mirror Neurath's views in that it seems to assume that Neurath embraced a traditional version of the Verification Principle, what he arguably did not. Nonetheless, I hope, the content of his view may be correctly captured by (3).

41 One may object that this interpretation implies that Neurath was not an empiricist – which would, indeed, be a strange claim to make – because accepting (2) and (3) would

4 Conclusion

In 1932, in “Universalsprache” (Carnap 1931/1934), in “Psychologie” (Carnap 1932a/1959) and in “Über Protokollsätze” (Carnap 1932b/1987), Carnap suggested that protocol sentences should be formulated in physicalistic language. But he did *not* claim in these papers that phenomenalist languages are unintelligible. Consequently, Carnap did not change his mind, and suggested that protocol sentences should be formulated in physicalistic language *under the influence of Neurath’s private language argument*. To this extent my view may be in conflict with the generally accepted view that Neurath was the major source of Carnap’s physicalist turn. I, however, do not dispute Neurath’s having an important role in turning Carnap to physicalism, only that Neurath’s private language argument was not among its motivating factors, for, as I see it, Carnap did not accept it.

It seems though, that Carnap did not explicitly reject Neurath’s private language argument, but rather ignored it, deeming it not so much as an argument but a bundle of unelaborated ideas, *aperçus*. Nonetheless, we may investigate what Carnap could have answered to Neurath. All the more so, since it seems obvious that Carnap must not have accepted Neurath’s private language argument even later, as he maintained that private phenomenalist languages are intelligible (as late as in 1961, in his introduction to the English edition of the *Aufbau*, and in 1963 in his reply to Goodman).⁴² ⁴³ So, could Carnap rebut Neurath’s private language argument based on his views?

imply that there is no possibility any longer to consistently and intersubjectively refer to the perceptible world. But I do not see why this would follow (2) allows that our experience, the phenomenal character of our perceptual states, may, in fact, have a diachronically constant nature (i.e. being the same or similar in like observational circumstances). But since we cannot verify this solely internally, such a claim has no truth value, hence cannot be true. In order for a person to be able to diachronically re-identify the content of her perceptions, she needs some external guarantee; hence perceptual contents must be interpreted as referring to public physical objects and properties. But to claim that the content of perceptual reports must be about observable physical objects and properties, not about the properties of experience is not to deny that only statements having some sort of empirical i.e. observational verification base are meaningful. Neither does it imply denying that all factual knowledge must be based on perceptual experience one way or another. So, it seems to me, my characterization of Neurath’s views by (2) and (3) is neutral on the issue of empiricism.

⁴² Cf. Carnap 1928/1967 p. vii, and Carnap 1963, p. 945.

⁴³ Consider the following formulation of Carnap: “When I read the old formulations today, I find many a passage which I would now phrase differently or leave out altogether; but I still agree with the philosophical orientation which stands behind this book. This holds especially for the problems that are posed, and for the essential features of the method which was employed. The main problem concerns the possibility of the rational

First, both Carnap and Neurath accepted the *privacy of experience*. Based on the privacy of experience, however, it can only be proved that *others* cannot verify whether a person's use of his phenomenalistic terms is constant through time. *Others* may not be able to verify whether a person's predictions are correct or not. But the subject herself may be able to verify it. Thus, based on the logical privacy of experience it may only be proved that a phenomenalistic *protocol* language is not possible, but it *cannot* be proven that a private phenomenalistic language is impossible *per se*.

However, in my view, Carnap assumed in his early physicalist period, between 1930 and 1932, that we have a reliable introspective capacity to diachronically re-identify our experiences. (And probably also later; cf. footnote 43). If this is the case, then both the claim that one cannot subjectively verify the constancy of use of her phenomenal terms, and the claim that synchronically and diachronically distinct persons are in a symmetrical position to know each other's experiences, can be rejected, as argued above. Moreover, there is a bonus point: if we assume that we have a capacity to re-identify experience diachronically, then it seems that the verificationist interpretation of

reconstruction of the concepts of all fields of knowledge on the basis of concept that refer to the immediately given" (Carnap 1961/1967 p. v). And "It is an essential characteristic of phenomenal language that it is an absolutely private language which can only be used for soliloquy, but not for common communication between two persons. In contrast, the reistic and the physical languages are intersubjective" (Carnap 1963, p. 86g). These statements clearly show that Carnap upheld the view also in the '60s that phenomenalistic private languages are possible. It is, of course, puzzling first, how to reconcile this with his claim that the *universal* language of science ought to be the physicalistic language, he advocated since the "Unity of Science". However, as I suggested, this apparent contradiction can be explained away by suggesting that Carnap only proposed physicalistic language *to be used for scientific purposes*. This suggestion does not rule out that solipsistic phenomenalistic language is meaningful. Furthermore, according to Uebel 2018, Carnap, by 1935 at the latest, reaffirmed that phenomenal autopsychological sentences are verifiable directly via introspection or indirectly by behavioural indicators, hence they again, ought to be considered as scientifically meaningful again (cf. Carnap 1936/37). So my view is in line with Uebel's detailed explanation concerning the different tasks what Carnap assigned to the solipsistic phenomenalistic language and the intersubjective physicalistic language. Cf. "It is evident that Carnap's abandonment of methodological solipsism remained intact [also in the late period when writing the introduction to the second edition of the *Aufbau* – G.A.)] [...] After all, Carnap's abandonment only meant a discontinuation of the employment of phenomenalist languages for practical use or their rational reconstruction; *they remained available as the objects of logical investigation*" (Uebel 2018, p 377; emphasis added – G.A.).

Wittgenstein's private language argument, which I presented as anticipated by Neurath, can also be rebutted.⁴⁴

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44 Of course, it is to be further investigated whether Carnap or Neurath is closer to truth on this point, further reasons may be discussed for and against the assumption that we have a reliable introspective capacity to re-identify our experiences, but I shall not pursue further this issue here.

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Conceptions of Protocol Sentences in Neurath and Carnap and the Bipartite Metatheory Conception

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Abstract

Discussions of protocol sentences, for obvious reasons, frequently focus on the period of the protocol sentence debates, whilst discussions of Neurath and Carnap's later work typically concern not protocol statements, but rather semantics, explication and the unity of science. But important elements of the compatibility of Carnap and Neurath's respective mature projects can be gleaned from an examination of their conceptions of protocol statements as presented in their later works.¹ With this in mind, the purposes of this chapter are threefold. First, to elaborate the mature conceptions of protocol statements held by Carnap and Neurath. Second, to explicate and defend Neurath's conceptions of protocol statements in the face of potential disagreements and misunderstandings. Third, through the introduction of a terminological distinction, to demonstrate the compatibility and complementarity of Carnap and Neurath's conceptions of protocol statements, and how this further demonstrates the complementarity of their respective meta-scientific projects. In so doing, this chapter provides further support for Uebel's bipartite metatheory thesis.

1 Protocol Statements and the Bipartite Metatheory Conception

What Uebel has called the bipartite metatheory conception attributes to Neurath and Carnap 'a broadly shared program for a bipartite metatheory serving as a joint replacement for philosophy'.² This scientific metatheory is a second-order inquiry into science itself, composed of two methodologically distinct parts; the empirical study of science, including the history, psychology and sociology of science, and the formal study of the logical structures of axiomatized scientific theories and terminology. These two meta-theoretical

¹ 'Sentence' and 'Statement' are used interchangeably in this chapter.

² Uebel 2007b, p. 435.

projects will be referred to as the pragmatics of science and the logic of science respectively.³ The majority of Neurath's work fell into the pragmatics of science. Carnap's work typically falls under the rubric of the logic of science. These two parts of the metatheory are not in competition but are complementary (perhaps even necessary for one another) and allow for collaboration. Neurath's emphasis on pragmatics and Carnap's emphasis on logic is therefore to be seen as a division of labour based on their individual interests and strengths, rather than a methodological disagreement. The bipartite metatheory conception has been articulated, and defended in more detail elsewhere.⁴ The focus of this essay is solely on the implications of Neurath and Carnap's conceptions of protocol statements for the bipartite metatheory conception.

Protocol sentences were understood by the members of the Vienna Circle as the basic evidence statements of science, those used in testing and confirmation. Though frequently interpreted as incorrigible statements of immediate experience (as utilised by traditional foundationalist empiricism) by both the Vienna Circle's contemporaries and modern commentators, there was no consensus within the Vienna Circle as to how protocol sentences ought to be understood. Rather, the protocol sentence debates of the early '30s involved extensive discussion of the form, status and purpose of protocol sentences within science. And whilst this may appear on the surface as a narrow technical debate, really under discussion were competing conceptions of scientific knowledge and the process of justification. Often (although not always) implicit in the debates over the correct understanding of protocol sentences was a deeper disagreement about the nature of scientific knowledge, and the appropriate methodology for philosophy. Discussions of these competing conceptions of protocol statements have typically (and with good reason) focused on the crucial period of the debates, between 1932 and 1934. When specifically considering the debate between Neurath and Carnap over protocol sentences, one's attention is immediately drawn to the interaction of 1932, particularly Neurath's *Protocol Sentences* and Carnap's reply *On Protocol Sentences*. Prima facie, this seems like the crescendo of the Neurath-Carnap debate; after 1932, it re-orientates to a clash between Neurath and Schlick. And from 1935 onwards, the disagreements and discussions between Carnap and Neurath centre on Carnap's embrace of Tarskian semantics. The issue of protocol sentences may therefore appear to have lost its importance in Neurath and Carnap's thinking.

3 The name 'pragmatics of science' is taken from Frank (cf. Frank 1957, p. 360), not Neurath, but is less cumbersome than Neurath's term, the 'behaviouristics of scholars' (Neurath 1936, p. 137). 'Logic of science' is Carnap's own term for his project (Carnap 1934).

4 See Uebel 2010; 2011; 2012; 2015; 2022.

Although their emphases change, Neurath's to his *Encyclopedia* project and Carnap's to semantics, protocol sentences continue to play a key theoretical role for both. And given the significance of protocol statements for each of their respective projects, a fundamental disagreement over what protocol sentences are would pose a significant challenge to their compatibility, and therefore to Uebel's bipartite metatheory interpretation. Evidently Carnap and Neurath were not always in complete agreement over the form and content of protocol statements; Schlick apart, there wouldn't have been a protocol sentence debate if they agreed. But what is of particular importance for current purposes, and insufficiently discussed in the secondary literature, is the status of their fully-developed notions of protocol statements. It is the compatibility of these mature conceptions that this article is concerned with.

Whilst Neurath's proposals are not set in stone, his basic conception of protocol statements is stable from the early '30s onwards. For Neurath, protocol statements are physicalist statements describing the perceptual states of an observer and information about the spatio-temporal context of observation. Protocol statements have no epistemic privilege; they are corrigible and revisable. They do have a purely methodological privilege however, due to their decisive role in the testing of scientific theories. It is only the very specific (and initially unintuitive) structure of protocol sentences that undergoes any significant revision in Neurath's conception. Initially, in the early '30s, he adopts a triple-layered structure:

Otto's protocol at 3:17 o'clock: [Otto's speech-thinking at 3: 16 o'clock was: (at 3:15 o'clock there was a table in the room perceived by Otto)].⁵

But he later adopts a quadruple-layered structure; 'Karl's protocol: Karl formulates: Karl is seeing: In the room there is a round table.'⁶ The advantage of this change, which will be made clearer below, is that it separates out the act of observation from the object under observation. It is this quadruple-embedded conception that I will refer to as Neurath's mature conception. By contrast, Carnap's notion of a protocol sentence undergoes frequent and significant alterations, especially during the debate of the early '30s. Over the course of the debate, Carnap revises the form, role and epistemic status of protocol statements. I will therefore start with a brief overview of Carnap's developing

⁵ Neurath 1932, p. 93.

⁶ Neurath 1935, quoted in and translated by Uebel 2007b, p. 386.

conception of protocol statements during the first half of the '30s, before arriving at his mature conception in 1936 in *Testability and Meaning*.

2 Carnap's Changing Conception

For current purposes, three main periods in the development of Carnap's conception of protocol statements can be identified.⁷

1. Early-Carnap: pre-1930
2. Middle-Carnap: 1930-35
3. Mature-Carnap: 1936 onwards

The early Carnap includes the *Aufbau* era, and predates the protocol sentence debates. The middle is the period of intense fluctuation of his conception, with almost every different work published in this period involving a slight change in either his ideas or the deployment of terminology. This includes the crucial period of the protocol sentence debates. The mature period begins with *Testability and Meaning* in 1936, where Carnap develops a conception which is essentially retained from then on, as other issues took precedence over protocol sentences.⁸

Prior to the protocol sentence debates, in the *Aufbau* Carnap utilised sentences in the auto-psychological language describing the immediate given (at bottom remembrances of similarity relations), although he recognises the possibility of constructing a similar system on a physicalist basis.⁹ In 1930, Carnap still allows both methodological materialism and solipsism, treating physicalist and auto-psychological languages as equally basic, although he continues to refer to statements as 'reducible to the given'.¹⁰ The auto-psychological protocol statements were understood as incorrigible. However, translation into the physicalist language was necessary for scientific purposes, because only a physicalist language 'makes inter-subjective knowledge possible'.¹¹ Carnap

7 This historical overview is a slight simplification. Uebel identifies five different positions maintained by Carnap between the *Aufbau* and *Testability and Meaning* (cf. Uebel 2007a, p. 442). But the slightly simplified account given here is sufficient for current purposes. For a more detailed history of Carnap's developing conceptions of protocol statements and methodological solipsism, see Uebel 2007a; 2018 pp. 372–375.

8 It could be argued that the ideas in *Testability and Meaning* are a development of Carnap's talk in 1935, later published as *Truth and Confirmation*, and that the mature period could therefore be dated from 1935. But only in *Testability and Meaning* is his conception of protocols stated explicitly.

9 Carnap 1928, §59.

10 Carnap 1930, p. 145.

11 Carnap 1930, p. 144.

therefore concludes that both constitution systems are equally 'correct and indispensable', the physicalist because it allows for the inter-subjective testability required by science, and the solipsistic for epistemological purposes because it allows justification via reduction to the given.¹² There is however still an asymmetry of translation, with primacy given to the auto-psychological. Whilst physicalist statements are translatable into the auto-psychological language, the reverse is not entirely possible; only the inter-subjectively verifiable component of an auto-psychological protocol is translatable, but a component of the meaning is private to the speaker.¹³

By early 1932 in *Unity of Science*, Carnap concedes the inter-translatability of the physical and auto-psychological languages, discarding the translational primacy of the auto-psychological. The protocol language was no longer understood as a private phenomenological language, but as a sub-language of the physical universal language.¹⁴ But statements still required translation into the auto-psychological language for epistemological purposes, primarily justification. And Carnap still maintains the conception of "the given", describing protocol statements as a 'direct record of a scientist's ... experience'.¹⁵ Auto-psychological protocol statements, despite losing their privacy, retain their epistemic privilege; 'a protocol sentence, being an epistemological point of departure, cannot be rejected'.¹⁶ Carnap still allows for primitive protocol statements, assuming a 'sharp (theoretical) distinction between the raw material of scientific investigation and its organization'.¹⁷ The auto-psychological protocol language therefore is still somewhat distinct from the physical language, despite Carnap's claims that it is simply a sub-language.

The major change in Carnap's conception of protocol statements, in which these prior commitments are abandoned, came about in late 1932 in *On Protocol statements*. Here Carnap finally withdraws the epistemological primacy and privilege of the auto-psychological; physicalistic statements can now serve epistemological purposes. With this epistemological privilege revoked, protocol statements also lost their certainty and incorrigibility. Now the form of protocol statements is completely unrestricted; 'it is a matter of decision which sentences one wants to use at various times ... as protocol sentences'.¹⁸

12 Carnap 1930, p. 144.

13 This is made clear in unpublished manuscripts from 1930. See Uebel 2007a, pp. 191–200.

14 Carnap 1932a, p. 88.

15 Carnap 1932a, p. 42.

16 Carnap 1932b, p. 191.

17 Carnap 1932a, p. 43.

18 Carnap 1932c, p. 465.

This renunciation of epistemological privilege is the most important change that Carnap's conception undergoes during the protocol sentence debates. The consequence is a total conventionalism about protocol statements; 'Every concrete sentence of the physicalistic system language can serve under certain circumstances as a protocol sentence'.¹⁹ Here Carnap's logical tolerance first manifests;²⁰ there is no longer a fact of the matter about what constitutes a protocol statement, only proposals to be evaluated according to their practical utility. And the proposal Carnap advocates is one according to which any statement can be considered a protocol statement.

The late 1932 conception is partially an overreaction to Neurath's anti-absolutist, anti-foundationalist arguments for the fallibility and revisability of protocol statements, captured by what Haller called the Neurath principle.²¹ But Carnap was also influenced by Popper who, he claimed, took this anti-absolutism a step further than Neurath had.²² In so doing, Carnap also takes conventionalism a step too far. Carnap sees in Popper's rejection of a 'last sentence' the most anti-absolutist form of protocol statement, even more so than Neurath's.²³ But we should remember that Neurath only has "last sentences" in a very mitigated sense; his protocol statements have only a special methodological role in testing, but no distinguished or unique epistemic status. This provides them significance via a particular role in theory testing, one which they are deprived of if we allow any sentence to qualify as a protocol statement. As Neurath shows, one can reject the epistemological privilege Carnap had previously attributed to protocol statements without falling into the total conventionalism of *On Protocol statements*. It is still a matter of decision as to what qualifies as a protocol, but it is not an arbitrary decision; it is a decision guided by the practical requirements of scientific practice. Whether for these reasons or not, Carnap arrived at a similar conclusion; this completely conventional conception of protocol statements appears only in *On Protocol statements*.²⁴ But whilst Carnap dials back the conventionalism, his rejection of epistemological privilege is definitive.

19 Carnap 1932c, p. 465.

20 See Awodey and Carus 2007, pp. 183–192.

21 See Haller 1982, p. 121.

22 Carnap 1932c, p. 469.

23 Carnap 1932c, p. 469.

24 That this completely conventional conception of protocol statements is not Carnap's final mature conception is not always made clear in secondary literature. See for example Creath 1990, p. 412; Coffa 1991, p. 371; Richardson 1997, p. 211; 2000, p. S158.

Carnap's mature position arrives in 1936 in *Testability and Meaning*. Here, Carnap settles for physicalist statements in a 'thing language' about medium sized objects as the most practical form for protocol statements.²⁵ Protocol statements are still neither certain nor incorrigible, but status as a protocol statement is no longer purely a matter of decision. Carnap proposes the requirement that protocol statements contain only inter-subjectively observable predicates, by which Carnap means that for a predicate P, a person 'is able under suitable circumstances to come to a decision with the help of few observations' that P or \neg P is confirmed.²⁶ What this conception amounts to is physicalist protocol statements formulated in ordinary language containing only observable predicates. With this mature conception of protocol statements in hand, Carnap no longer focuses on debating competing conceptions, but instead attempts to build on his conception by providing deductive relations between theoretical and observational terminology.²⁷

3 Mature Positions: Apparent Disagreement

This simplified sketch of the protocol sentence debates, at least the Carnap-Neurath axis, sees Carnap conceding a certain amount to Neurath; abandoning foundationalism, methodological solipsism and phenomenalist auto-psychological protocol statements as practically impossible, and consequently adopting a physicalist protocol language. The fundamental disagreement about the need for a phenomenal protocol language seems to be overcome, and a broad agreement reached; protocol statements are physicalistic reports, open to revision and accepted by decision. But crucially, Carnap never adopts the specifics of Neurath's account. Two key elements of Neurath's proposal are never embraced by Carnap. Firstly, the multiply-embedded bracket structure, and secondly, the requirement of contextualising information contained within the protocol. These requirements are intimately intertwined for Neurath; the purpose of the structure he advocates is to exemplify the contextualisation, as will be demonstrated below. But Carnap rejects both; the multiply-embedded structure was rejected as impractical, and the restrictions Carnap placed on contextual information never required containment within the protocol itself.

25 Carnap 1936/37, p. 466.

26 Carnap 1936/37, p. 455.

27 For the best examples, see Carnap 1936/37; 1956.

This point of contention resurfaces in the later correspondence between Carnap and Neurath, a decade after their axis of the protocol sentence debate seemed to have concluded:

As far as your formulations are concerned (which unfortunately are not in agreement with your opinions) I have told you since many years that I cannot accept them and hence I agree with the criticism of these formulations by R[ussell], Schlick, and many others. In distinction to R., I know your actual conception from conversations; and I am in agreement with it ... I agree with R. in his criticism of your triple-involved form of protocol sentences.²⁸

To which Neurath replies:

You always tell me, you agree with Russell's and Schlick's remarks on my protocol statements, but my statements intended are different. Please, tell me first what you think how I should express my statements properly and then please tell me, why even then they are not in harmony with your opinion. I think it is important to come to some clearness.²⁹

This exchange suggests a lingering uncertainty about protocol statements that is sadly left unresolved, the possibility of further discussion cut short by the fractiousness of their later correspondence and Neurath's unexpected death. Despite claiming that he is in agreement with Neurath, Carnap goes on to reject his proposal for the form of protocol statements. The question then lingers: did they agree or not? Making sense of this interaction is essential for understanding the compatibility of their mature conceptions of protocol sentences. In the rest of this article, I will explore the nature of this apparent disagreement, and its significance for the bipartite metatheory conception. There are two potential disagreements between Carnap and Neurath, which broadly correspond to the two elements of Neurath's proposal that Carnap doesn't adopt. The first is a disagreement about structure and logical form. The second is the issue of what information needs to be contained within the protocol statement itself. The issue of logical form will be discussed first.

28 Carnap to Neurath, March 15, 1943, (ASP RC 115-07-62) in Tuboly and Cat (eds.) 2019, p. 577.

29 Neurath to Carnap, September 25, 1943, (ASP RC 102-55-03) in Tuboly and Cat (eds.) 2019, p. 598.

4 Carnap's Formal Objection

To give some context to the quotes above, they appear in the written correspondence between Neurath and Carnap, during a discussion of Bertrand Russell's recently published *An Inquiry Into Meaning and Truth*, which Neurath was deeply critical of and Carnap found somewhat misguided but largely uninteresting. The passage Carnap refers to is this one:

Thus according to Neurath the data of empirical science are all of the following form: "A certain person (who happens to be myself, but this, we are told, is irrelevant) is aware at a certain time that a little while ago he believed a phrase which asserted that a little while before that he had seen a table." That is to say, all empirical knowledge is based upon recollections of words used on former occasions. Why recollections should be preferred to perceptions, and why no recollections should be admitted except of thought-words, is not explained.³⁰

Russell's criticism specifically targets Neurath's multiply-embedded bracket structure. If Carnap agrees with Russell, as he claims in his letter, then his problem must also be with the structural and formal-logical aspect of Neurath's proposal. As Carnap admits, he agrees with what he understands as Neurath's conception of protocol statements (physicalistic, revisable, unprivileged). But this conception, Carnap claims, is in conflict with or is undermined by Neurath's proposed bracketed structure. This is not the first time Carnap questioned the logical structure of Neurath's protocol statements. Carnap had expressed similar doubts during the protocol sentence debates, where he criticised Neurath for the impracticality of a protocol sentence with 'three nested components'.³¹ He also claims that Neurath's proposal 'has the defect, from the point of view of syntax, that a sentence which refers to another contains the other as a clause'.³² Exactly why this troubles Carnap is unclear and unexplored, but again it is rooted in the logical and structural elements of Neurath's proposal. It has to be conceded that the logic of Neurath's protocol statements is not made sufficiently clear by Neurath himself. It can also be conceded that Neurath was not always sufficiently explicit about the motivations for the protocol structure he adopts. Carnap's confusion at the time of their correspondence is understandable. But Neurath would later attempt to make explicit his

³⁰ Russell 1941, p. 146.

³¹ Carnap 1932c, p. 465.

³² Carnap 1932c, p. 465.

reasons for choosing the multiply-embedded structure. And a detailed look at Neurath's explanation demonstrates that, quite contrary to Carnap's claims, Neurath's proposed structure is in harmony with his understanding of protocol sentences.

In a paper written after this correspondence, Neurath argues that protocol statements must provide the information 'When, where, and how?', the same information we would require of any other scientific statement to decide whether or not we should accept it as a scientific datum.³³ To accurately categorize different types of observation reports, and make decisions on their acceptability, we require this contextualising information to be supplied along with the observation report itself. The multiply-embedded structure is designed to exhibit exactly this information to facilitate synoptic understanding of all the factors relevant to the acceptance of observation reports and, their inter-relations.³⁴ This understanding cannot be achieved through lists of conditions treated in isolation from one another. Rather, synoptic understanding requires the presentation of protocol statements as constituted by concatenations, simultaneously comprehensible wholes. It is the simultaneous presentation of the parts and the whole that allows synoptic understanding. Protocol structure is intended to render these factors quite literally open to view. Here, a possible (albeit not direct) comparison can be made to Neurath's picture language, ISOTYPE. Whilst ISOTYPE was primarily concerned with the communication of quantitative data, some of the principles are clearly applicable in this context to his protocol statements. Both are forms of visual presentation that allow for the derivation of complex information from a relatively simple image. Consider Nemeth's description of how to engage with ISOTYPE:

Neurath's pictures should prompt those looking at them to go back and forth between at least two constellations (normally more than two) of elements, figuring out for themselves what the comparison is all about.³⁵

What the comparison with ISOTYPE shows is that instantaneous, *prima facie* understanding was not the expectation. The information to be conveyed is complex, and as such requires a degree of complexity in presentation to fully

33 Neurath 1946, p. 233.

34 The choice of "synoptic" is deliberate, as it echoes Neurath's usage of the same terms elsewhere. Neurath describes the purpose of his *Encyclopedia* as allowing science to be 'presented to us synoptically in its totality' (cf. Neurath 1938, p. 141). I think the same motivation underlies the proposed structure for protocol statements.

35 Nemeth 2019, p. 130.

express it. It is only active and deliberate engagement with and consideration of the protocol structure by the epistemic agent that reveals and communicates the information conveyed therein.

What is essential to understand is that Neurath provides a protocol sentence schema, and the structure is designed with a specific schematic purpose in mind. For Neurath, protocol statements are not about truth-conditions, but acceptance conditions. So his schema prioritises the presentation of the factors relevant to the acceptance procedure in a way that maximises both ease of understanding and practical utility. Maximum clarity about when these conditions are met and the ease with which these statements can be put to use in practice are of much greater significance for his purposes than providing his proposals in proper logical form. Consequently, rather than a protocol sentence as a statement of standard predicate calculus, Neurath gives a syntopic schema that visualises the factors and process of protocol acceptance with maximum perspicuity. As opposed to grammatical usage, punctuation is deployed for purposes of visual communication. The price of this maximisation of epistemological perspicuity is a loss of typical logical form. But Neurath's emphasis on the pragmatics of science over the logic of science justifies this prioritisation.

It is therefore a mistake to read Neurath's proposal as a typical grammatical sentence. But this is exactly what Russell (and by extension Carnap) does. Russell's worry is the result of an understandable, but mistaken, reading of Neurath's proposal for the structure of a protocol statement. Let's return to the example given above:

'Otto's speech-thinking was at 3:16 o'clock: (at 3:15 o'clock there was a table in the room perceived by Otto)' and further: 'At 3:15 o'clock there was a table in the room perceived by Otto.'³⁶

Russell interprets Neurath's proposal as a conventional sentence reporting an experience, albeit one containing a lot of brackets. Consequently, he expands the brackets in the typical grammatical way. But when we recognise that Neurath's protocol statements are not the regular sentences Russell treats them as, it becomes clear that the brackets cannot and should not be expanded this way. The brackets are not being used to separate linguistic clauses, but to visually separate distinguishable factors in the appraisal of observation reports.

³⁶ Neurath 1932, p. 93.

This objection by Russell, and by extension Carnap, shows a failure to appreciate Neurath's purposes.

The most important demonstration of the role Neurath intends his schema to play is provided in his 1941 paper *Universal Jargon and Terminology*. Part of the paper is a direct response to Russell's book, published the same year.³⁷ It therefore seems very plausible that Neurath provided this example specifically to dispel the confusion shown by Russell, and by extension Carnap. Even if this was not the case, his examples can play that role for us. Neurath begins by separating 'the four 'parts' of our protocol statement: A (protocol), B (word-thinking), C (zebra), D (person perceiving)'.³⁸ Neurath then gives three example charts for the categorisation of protocol statements:³⁹

A, B, C, D, accepted	A, B, C, D, accepted	A, B, C, D, accepted
B, C, D, accepted	B, C, D, rejected	B, C, D, accepted
C, D, accepted	C, D, accepted	C, D, rejected
D, accepted	D, rejected	D, accepted
'factual statement'	'type of lying'	'hallucinatory statement'

What these charts show is that the multiply-embedded structure of Neurath's protocol statement is intended to simultaneously exhibit four sub-statements, each of which is essential for the reception of observation reports. These four sub-statements of Neurath's schema are:⁴⁰

- (i) protocol (thought [stimulation state {observable fact}])
- (ii) thought [stimulation state {observable fact}]
- (iii) stimulation state {observable fact}
- (iv) observable fact

Following Uebel's terminology, the sub-statement contained in the innermost bracket, the statement expressing an observable fact, is referred to as the

37 Neurath 1941, p. 226.

38 Neurath 1941, p. 220.

39 Neurath 1941, p. 220.

40 The order of stimulation state and observable fact (Neurath's D and C respectively) has been switched in my schema. This is based on Neurath's own usage in a letter to Kaufman from 1935, quoted in Uebel 2007b, p. 386. The advantage of this order over that used by Neurath in 1941 is that it places the object sentence (iv) at the centre of the protocol. The importance of the centrality of the object sentence will be made clear below. For more detailed discussion, see Uebel 2007b.

protocol statement's object sentence.⁴¹ The object sentence contains the factual content that an observation report delivers.

Each of sub-statements (i) – (iv) corresponds to one of four conditions for protocol acceptance, as previously explicated by Uebel.⁴² For an object sentence X, these conditions are:⁴³

- * (i) the institutional condition: somebody made the explicit claim that somebody thought that somebody was stimulated as if she perceived X
- * (ii) the intentional condition: somebody conceptualised that somebody was stimulated as if she perceived X
- * (iii) the sensory condition: somebody was stimulated as if she perceived X
- * (iv) the negative coherence condition: there is no evidence available that contradicts X

Each condition captures a necessary feature of acceptable scientific testimony. Condition *(i) guarantees that the report was entered into the scientific record; reliable experience reports must come from the observers themselves, specifically entering their experiences into the record. Condition *(ii) guarantees the preservation of the intent and content of the report through the retention of the original linguistic and conceptual framework in which the report was couched. This prohibits attempts to translate or re-interpret past reports, in so doing corrupting them.⁴⁴ Condition *(iii) guarantees speaker sincerity, ruling out lies. Condition *(iv) is a negative coherence condition establishing speaker competency, ruling out hallucinations and errors by guaranteeing an absence of known defeaters. It shouldn't be understood as a factive condition, as this would be redundant; why bother with the four conditions if we already know the object sentence is true? Rather, it embodies the possibility that reports can be rejected on the basis of background knowledge and theory.

For Neurath the assessment and categorization of protocol sentences is not simply a matter of the acceptability of each bracketed clause, but the

41 Uebel 2009, p. 6. In an earlier work, he instead uses the term "content statement", but I adopt the more recent terminology (cf. Uebel 2007a, p. 388).

42 Uebel 2007a, pp. 383–388.

43 Uebel 2007a, pp. 383–384.

44 This could be understood by analogy to the incommensurability between Kuhnian paradigms. Reports made from within a Newtonian paradigm are corrupted if we attempt to translate or reinterpret them within an Einsteinian paradigm. The markings of the initial paradigm cannot be removed without loss or corruption of the report itself.

acceptability of the four sub-statements and satisfaction of the four conditions. Checking which of these four conditions are met and which are not allows categorisations of protocol sentences. To qualify as an *accredited protocol*, an observation statement only needs to fulfil *(i), being entered into the scientific record. To qualify as a *valid protocol*, all four conditions must be met, that being the case in which all four sub-statements are themselves accepted.⁴⁵ Importantly, what these charts make clear is that Neurath's protocol sentence schema plays a practical role on top of its theoretical role. The structure of his schema not only exhibits the factors relevant to the process of acceptance, but in combination with charts of the sort Neurath provides, facilitates evaluation and categorisation of specific observation reports. The schema is not designed as simply a theoretical model providing a solution to an epistemological question about acceptance, but for practical application in concrete cases. The role of the schema in the process of protocol acceptance and the categorisation of protocol sentences will be elaborated below.

Importantly, the structure Neurath proposes was never intended as a final account of *the* form of protocol statements. Whilst protocol statements must be physicalistic, are always revisable, and must contain relevant contextualising information, the specific structure Neurath proposes is contingent on how successfully it performs the functions described above and below. Although I will argue for the success of Neurath's structure, it is quite possible that an alternative structure could display the same information in a more appealing or efficient way. So long as it fulfils Neurath's physicalistic and practical requirements, Neurath should welcome such a structure as an improvement upon his own proposal.

5 Motivation for Neurath's Schema

Before we return to Carnap, a brief discursion is necessary. Having conceded that Neurath was insufficiently explicit about the correct logic for his protocol statements doesn't mean that there is no such logic. In supplying this logic, we can not only alleviate lingering doubts about the logical acceptability of his proposal, but also provide a further justification for the structure Neurath adopts. As mentioned above, a valid protocol is one which meets all four conditions *(i)–*(iv). In other words, a valid protocol is one for which all four sub-statements (i)–(iv) are accepted. A possible logical form for a valid protocol

45 This use of terminology follows Uebel. See Uebel 2007a, pp. 384–385.

statement is then formalizable as: (i) \wedge (ii) \wedge (iii) \wedge (iv). Initially, this logical form does not seem too complicated. But, when a concrete example is used, the unsuitability of this formulation for Neurath's purposes becomes apparent. Take the example:

Karl's protocol: Karl formulates: Karl sees: In the room is a round table.

For this example, (i) \wedge (ii) \wedge (iii) \wedge (iv) becomes:

'Karl protocolises that Karl formulates that Karl sees that in the room is a round table' \wedge 'Karl formulates that Karl sees that in the room is a round table' \wedge 'Karl sees that in the room is a round table' \wedge 'In the room is a round table'

Obviously this sentence is incredibly unwieldy.⁴⁶ But more importantly, the additional complexity masks the inter-relations of the four sub-statements. When rendered explicit like this, we can no longer see the woods for the trees. Such a logical form would be so complex that our ability to grasp the connection between parts and whole is lost. The desired synoptic understanding is not achieved. By contrast, Neurath's schema exhibits the whole and the parts simultaneously, thereby highlighting the inter-connections, and allowing easier engagement by the observer.

But in addition to facilitating synoptic understanding, Neurath's protocol statements were also designed with a more practical application in mind:

This [protocol] sentence is so constructed that, after 'deletion of brackets', further factual sentences appear, which, however, are not protocol sentences: 'Otto's speech-thinking was at 3:16 o'clock: (at 3:15 o'clock there was a table in the room perceived by Otto)', and further: 'At 3:15 o'clock there was a table in the room perceived by Otto'.⁴⁷

But what purpose does the revealing of further factual statements within a protocol statement serve? Here we must be careful interpreting Neurath. If we take this talk of deleting brackets as referring to the expansion of brackets,

⁴⁶ The above is not intended to capture or provide a specific logic of acceptance. Rather, it is only intended to demonstrate that even the most rudimentary attempt to formalize Neurath's conditions according to any such logic would obliterate the visually communicative purposes of Neurath's structure.

⁴⁷ Neurath 1932, p. 93.

then we are led to the same confusion Russell exhibits above. Rather, having recognised Neurath's proposal as a schema and not a typical sentence, we should understand "deletion of brackets" as meaning the deletion of the entire clause contained within the brackets.⁴⁸ And the reason for such deletions is to facilitate quick and easy checking of conditions *(i)–*(iv). To see how, we can use an example. Again, take the protocol sentence:

Karl's protocol: Karl formulates: Karl sees: In the room is a round table.

With it, we can decide whether condition *(i) is met. Having done so, we can then "delete the bracket", i.e., delete the first clause. We then have the sub-statement (ii):

Karl formulates: Karl sees: In the room is a round table.

This allows us to check condition *(ii). Deleting brackets again gives sub-statement (iii):

Karl sees: In the room is a round table.

We can now check *(iii). And finally, deletion gives sub-statement (iv):

In the room is a round table.

This allows us to check *(iv). These successive deletions allow all four conditions to be quickly and easily checked, allowing for simple categorisation of protocol statements, via charts like those given above. In real-world situations, deletion could be achieved by covering the "deleted" clauses with your hand, or striking them through with a pen removal. This process could be streamlined and visualised even further. Taking Neurath's charts from above, we could supplement "Accepted" with a tick and "Rejected" with a cross. During the process of successive deletions, one could simply leave a tick or cross for each successive check. The result could then be immediately compared with the categorisation charts. This demonstrates how Neurath intended these protocol statements to be made use of practically and visually, in a way that sentences in proper logical form cannot. Most importantly for our purposes though, in

⁴⁸ It seems likely that this confusion is a result of English being Neurath's second language. In German, "brackets" can be used not simply to refer to the symbols themselves, but the symbols *and* the clause contained within, as with "parenthesis" in English.

contrast to Carnap's accusations, Neurath's protocol statements, when understood in their proper context, are eminently practical *by design*. Neurath's structure, far from being a mistake, is tied to his conception of the methodological role of protocol statements.

6 The Methodology and Logic of Science

Although the motivations for Neurath's proposal are now clearer, this does not demonstrate that Carnap and Neurath's conceptions cohere. In light of everything shown so far, one may conclude that Carnap simply ignored or rejected the entire process of protocol acceptance as described by Neurath. After all, he seems to have overlooked Neurath's motivations for the protocol structure, and if he rejected the process of protocol reception then he would have no need for the structure designed to facilitate it. But this would be wrong. In their correspondence, Neurath gives the following account of acceptance, utilising the conditions detailed above:

And now I ask, how we may speak of accepting something as a "lie", you know I answer, when I accept X says this is brown, when I accept X says internally this is black and not brown, we may accept tentatively X is a liar. If we accept X is saying I see [a] brown table and X is internally saying I see [a] brown table, and we do not accept the statement here is a brown table (as a combined statement, as it were) then we call the X-statement a dream statement or an illusion statement.⁴⁹

This is a (slightly clumsy and protracted) description of the process outlined in the previous section. In his reply, Carnap refers to this passage and says:

I am in complete agreement with your description of the scientific procedure. I should classify this as belonging to the methodology of science.⁵⁰

This is a confirmation that Carnap accepts Neurath's account of the reception and categorisation of observation reports. This should not come as a surprise however, as Carnap repeatedly emphasised the importance of the pragmatics

49 Neurath to Carnap, September 25, 1943 (ASP RC 102-55-03), in Tuboly/Cat 2019, p. 596. Importantly, nothing he says here is new.

50 Carnap to Neurath, February 4, 1944 (ASP RC 102-55-04), in Tuboly/Cat 2019, p. 609.

of science, despite his own focus on the formal study of the logic of science.⁵¹ And yet, despite this essential agreement, Carnap still never adopts Neurath's schema. Why not? Uebel has previously argued that Carnap and Neurath's disagreement over protocol sentences shines a light on the differences between their two projects (or the two halves of their joint project).⁵² He is right, but simple difference of emphasis is not the whole story. Carnap and Neurath differ on more than emphasis; they deploy the term "protocol statement" differently. But this terminological divergence should not be understood as indicating a deeper theoretical rupture. If anything, their varying use of terminology actually masks the essential agreement between their conceptions. But to see this, we first need a more detailed explanation of the processing that protocol statements undergo to integrate them into the body of scientific knowledge.

7 The Processing of Observation Reports

According to Neurath, how does an observation report become a scientific datum? If we establish that a protocol statement is valid, as described above, processing into evidence is relatively simple: extract the object sentence embedded in the protocol, and add it to our encyclopedia as a piece of evidence. By establishing that a protocol is a reality statement (Neurath's term for a valid protocol), we licence the extraction of the object sentence at the protocol statement's core as a piece of scientific data. But there is a subtlety to the process that can be easily overlooked. For Neurath, so long as a statement is accredited, it can be added into our encyclopedia. It is not incorporation into the encyclopedia that distinguishes valid and invalid protocol statements, but whether the object sentence is *also* added to the encyclopedia:

If we incorporate part of the above mentioned protocol, the statement "in the room was a table perceived by Charles" along with the whole protocol into the body of science, then we can speak of a 'reality formulation', whereas we would speak of a 'dream or hallucination formulation', if we accept the whole protocol but not the part "in the room was a table perceived by Charles".⁵³

51 See for example Carnap 1934c, p. 8; 1935, p. 332; 1938, p. 393.

52 Uebel 2007a, p. 396.

53 Neurath 1934, p. 107.

To clarify this, we need to differentiate two different areas (metaphorically speaking) of our body of scientific knowledge, what Neurath calls our encyclopedia. I therefore propose a distinction between two different but related bodies of information; the protocol bank and the data bank.⁵⁴ The protocol bank is a comprehensive list of all accredited protocol statements. It acts as an enormous archive, a compendium of scientific observation reports, from which scientists and scholars can draw as required. Inclusion of a protocol in the protocol bank requires only accreditation, meeting condition *(i), but not validity. The protocol bank therefore includes not only reality statements, but also hallucination statements, dream statements and even lies. It may seem counterintuitive, but even lies and hallucination statements are potentially useful pieces of evidence. This is particularly true for social sciences like anthropology, history and sociology. For instance, anthropologists or psychologists studying religious experiences would find relevant hallucination statements of extreme interest. Similarly, lies are potentially important evidence for historians and biographers. And all non-valid statements may serve as the basis for abductive reasoning. Neurath is therefore right to accommodate such potential uses.

The data bank contains the body of statements that make up the confirmed data of our best current scientific theories. However, the statements that make up the data bank are *not* complex, multiply-embedded protocol statements, but the object sentences that form a protocol's core. The data added to the data bank are the object sentences of valid protocol statements. Again, take Neurath's example:

Karl's protocol: Karl formulates: Karl is seeing: In the room there is a round table.

For the sake of argument we will assume it is accredited. As an accredited protocol, the whole statement can be added to the protocol bank. For the sake of argument, let us also accept that this protocol is valid. In this case, as well as adding the whole sentence to the protocol bank, we are licenced to extract

54 The proposed terminology is my own. However, I think the distinction is not only consistent with Neurath's account, but potentially implicit in his work. For example: 'A distinction will certainly be made between the protocol statements (that turn up as physical formations) made by an astronomer, or a chronicler, and the statements that have a precisely defined place within a physical system, though obviously there are some overlapping transitions' (Neurath 1931, pp. 65–66). The former is the protocol bank, the latter the data bank.

the object sentence “in the room there is a round table” and add it to the data bank.⁵⁵ The data bank is therefore smaller and more exclusive than the protocol bank. Any scientific datum in the data bank must have at least one corresponding protocol in the protocol bank. However, not all protocol statements in the protocol bank will have corresponding data in the data bank, only the reality statements. Whilst any protocol can be added to our encyclopedia as a report to be used as a potential piece of evidence in future, only valid protocol statements are absorbed into our encyclopedia *alongside* the object sentence embedded at its core. A valid protocol provides both the report *qua* report, *and* the data delivered by the report. It is this additional incorporation that distinguishes the role of a valid protocol. All reports provide potential evidence, even lies and mistakes. But acceptable observation reports bring with them additional evidence of a factual sort, a piece of scientific data, by virtue of their validity.

This interpretation of Neurath’s protocol statements may not be immediately obvious, but it is demonstrated explicitly in the correspondence between Neurath and Felix Kaufmann from which the above quote is taken. There, Neurath says:

Karl’s protocol: Karl formulates: Karl sees: In the room is a round table.
and

Karl’s protocol: Karl formulates: Karl touches: In the room is a round table.
are both statements featuring the part:

In the room [is] a round table.⁵⁶

This passage makes clear that the object sentence embedded within a protocol is a detachable component of it. In Neurath’s example, we have two different protocol sentences with the same object sentence. Both supply the same piece of scientific data (about the table) but the reports supplying the data are different, one being prompted by a visual stimulus and the other by a tactile stimulus.⁵⁷ Clearly the object sentence itself can be meaningfully separated from the

55 Given Neurath’s insistence on the revisability of protocols, it should also be noted that membership in the data bank is also open to revision.

56 Quoted in and translated by Uebel 2007b, p. 386.

57 As Uebel notes, this provides Neurath with the additional benefit that ‘it allows for the convergence of reports in different sense modalities to be clearly displayed’ (Uebel 2007b, p. 387). This differentiation allows for the mutually reinforcing use of protocols indexed to different senses supplying the same object sentence.

protocol statement that initially delivers it, and can be understood and used in isolation from it.

Crucially, the resulting datum is a simple physicalist statement about medium-sized objects, expressing a spatio-temporal state of affairs. The complexly structured multiply-embedded schema is a means for delivering this simple physicalist datum. Contrary to Russell's misinterpretation then, Neurath's account does not render the data of empirical science implausibly complex. It is the protocol schema, and the process of acceptance it embodies, that exhibits such complexity. This complexity is not Neurath's addition, but simply a reflection of the multiplicity of factors relevant to deciding on whether or not reports should be accepted. But the complexity of the delivery mechanism does not undermine the simplicity of the datum it supplies. It is this important clarification that I think Carnap overlooks, and which I think explains his disagreement. Carnap, like Russell, understood Neurath's protocol statements to add unnecessary complexity to what should be simple physicalistic statements. We can now see that he was mistaken.

8 Difference of Terminology

With an understanding of Neurath's account of acceptance in place, we can finally highlight a subtle but significant difference in Carnap and Neurath's usage of "protocol": Carnap's protocol statements, physicalist statements about observable mid-size objects are the object sentences of Neurath's protocol statements. Carnap gives the example protocol statement 'a black round table'.⁵⁸ This is strikingly similar to the object sentence of Neurath's example 'Karl's protocol: Karl formulates: Karl sees: In the room is a round table'.⁵⁹ Is this just a coincidence? Or is this different usage indicative of a conceptual disagreement? No on both accounts. But to see why, it is helpful to frame the issue differently. According to the bipartite metatheory interpretation, scientific metatheory is composed of two parts; the pragmatics of science as practiced by Neurath and the logic of science as practiced by Carnap. So which part of the metatheory do protocol statements fall under? According to Carnap's usage, they belong to the logic of science. But according to Neurath, they belong to the pragmatics of science. Again we have the appearance of theoretical disagreement, but really this difference is only terminological.

58 Carnap 1936/37, p. 13.

59 Quoted in and translated by Uebel 2007b, p. 386.

To see this, we must remember that the reception of protocol statements is not an event, but a process. Initially, an observation report is checked against Neurath's conditions *(i)–*(iv). If these conditions are met, the protocol is valid, and the extraction of the object sentence is licenced. This entire process falls under the pragmatics of science, as it concerns acceptance. The data bank, the output of the process of acceptance, is the starting point for Carnap's logic of science. There is then a clear continuity between the pragmatics and logic of science here. But crucially, they address different aspects of science. Logic of science concerns the logic of observation reports within the language of science; establishing definitions, patterns of deduction and so on. Pragmatics is concerned with the acceptance conditions for observation reports in scientific practice. The latter concerns science as an activity, where the former concerns science as a body of theory. The starting point for the logic of science is the output of the pragmatics of science. Neurath's object sentences are the only element of the protocol statement that makes it into the language of science, in what I have called the data bank, because only they are relevant to scientific theory. Issues of observer reliability and competency are crucial to understanding the methodology and practice of science, but not so for understanding scientific theories themselves. Carnap starts from the presupposition that the statements of the language of science are acceptable and accepted. His concern is how these data are utilised once they have been accepted. Both Neurath and Carnap use "protocol sentence" to refer to observation reports within their sub-field of the meta-theory. But both are referring to one half of a process. What Carnap and Neurath ultimately disagree about is at what stage in the process we apply the term "protocol sentence". But there is no substantive theoretical disagreement here.

As to the question of what a protocol statement "really" is, neither Carnap or Neurath would have entertained such a debate. Both recognised the protocol debate as one of competing proposals. Now we know their proposals are not theoretically incompatible, but may require a change in terminology from one party. And I think for the purposes of terminological clarity, Neurath's proposal ought to be adopted. Firstly, Neurath's proposal has brought about the Neurathian terminology utilised throughout the discussion above. But perhaps more importantly, Neurath's usage of "protocol sentence" adheres more closely to the original intention; those basic evidence statements of science. As we saw, significant processing is required to arrive at Carnap's protocol sentences. The need now is for a term to replace Carnap's use of protocol. A tentative proposal of a name for evidence statements as utilised within the logic of science is "data sentences".

9 Carnap's Context Objection

We can now finally return to Carnap's rejection of Neurath's requirement that protocol statements contain the contextual information to answer the question 'When, where, and how?'⁶⁰ Carnap specifically rejects Neurath's requirement for 'designations of actions of perception'.⁶¹ He agrees with Neurath that 'a certain connection between the basic sentences and our perceptions is required', but 'it is sufficient that the biological designations of perceptive activity occur in the formulation of the methodological requirement concerning the basic sentences ... and that they need not occur in the basic sentences themselves'.⁶² The methodological requirement Carnap refers to here is the stipulation that protocol sentences must be inter-subjectively observable. As far as Carnap is concerned, the requirement of observability is sufficient. So long as the protocol statements are observable, the inclusion of contextualising information is superfluous. And Carnap is not wrong here. For *his* purposes, such information is superfluous. But as we have seen, for Neurath's purposes it is far from it.

Carnap's requirement of observability is a reformulation of his commitment to empiricism (in spirit, his most recent version of verificationism). As such, the requirement of a "certain connection" is in place to exclude the possibility of metaphysical statements entering into the logic of science. But as should now be clear, Neurath's demand for contextual information is not simply to guarantee such a connection. Neurath's requirement is not simply a verificationist one. The contextual information itself is of great significance for Neurath, since it is this information that allows decisions on acceptance. Such information however is not necessary for Carnap. As we already saw, his logic of science starts from the assumption that the statements with which he is working are valid. That is what his observability criteria does. Carnap therefore misunderstands the significance of Neurath's demands for contextual information.

There is one further possible complication here. Neurath's criteria and Carnap's requirement of observability are not the same. Carnap argues against Neurath's protocol statements for having the limitation of being 'intersubjectively confirmable but only *subjectively observable*'.⁶³ As Uebel notes, Neurath's conditions *(ii) and *(iii), about speech-thinking and perception, whilst inter-subjectively confirmable, are not observable by others.⁶⁴ Nor are descriptions

60 Neurath 1946, p. 233.

61 Carnap 1936/37, p. 13.

62 Carnap 1936/37, p. 13.

63 Carnap 1936/37, p. 11.

64 Uebel 2007a, p. 395.

of psychological states as object sentences, statements like “I feel angry”. But Carnap preferred an observable thing-language, intersubjectively confirmable and observable. Carnap therefore rejects Neurath’s inclusion of psychological predicates in physicalist statements, which rules out reports of one’s own psychological states (anger, joy, confusion etc). Framed this way, we seem to have a potential challenge to the bipartite metatheory conception. Carnap insists that observation statements of science need to be inter-subjectively observable and confirmable. Neurath requires that the terms of the protocol language need to be inter-subjectively confirmable, but only subjectively observable. But since Carnap’s logic of science starts with the object sentences supplied by the pragmatics of science, some of the object sentences Neurath supplies would simply be rejected by Carnap for failing to meet his criteria.

But before such conclusions are reached, it needs to be emphasised that the requirements on observability placed on observation statements is, as Carnap recognises, a matter of decision about the language best suited to our purposes. The disagreement here is not one over what observability is, but what requirements will be most useful to adopt. Carnap, in line with his principle of tolerance, explicitly frames his choice of observability conditions as a decision.⁶⁵ What practical reasons are there? That some predicates are only subjectively observable ‘is a serious disadvantage and constitutes reason against their choice.’⁶⁶ Exactly why this is so disadvantageous is not spelled out in detail by Carnap. Carnap gives no further reasons for rejection. We can accept Carnap’s worries here, but his reasons are far from decisive. The question is then whether Neurath has better reasons than this for embracing inter-subjectively confirmable but only subjective observable predicates.

For Neurath, protocol statements like “Karl’s protocol: Karl formulates: Karl is feeling: Karl is scared” must be potentially valid as protocol statements because of their obvious significance for social sciences like sociology, history and anthropology. ‘Historians of human social life are highly interested in descriptive terms, such as deal with the feeling-tone of persons, their devotion their fear and hopes.’⁶⁷ Our physicalist language needs to allow for reports of ‘the state of a person who hears Beethoven or looks at certain forms of architecture.’⁶⁸ If anything, he argues, we need a more extensive and fine-grained terminology for describing feeling-tones. On this point, Neurath seems unarguably right. The disciplines of psychology, anthropology and sociology would

65 Carnap 1936/37, p. 9–13.

66 Carnap 1936/37, p. 12.

67 Neurath 1944, p. 14.

68 Neurath 1944, p. 15.

all be significantly poorer if subjected to Carnap's limitations. And importantly, Carnap concedes as much. The language described in *Testability and Meaning* is designed for limited purposes, and he notes that 'we would have to take them as primitive predicates in a language of the whole of science ... because in such a language we require them in any case'.⁶⁹ The whole of science includes the social sciences. Ultimately then, Carnap can be read as voicing caution rather than outright disagreement. What Carnap ultimately argues is that, if a language doesn't require psychological predicates, then it is more practical to do without them, since this allows for inter-subjective observability of all predicates. We can accept his point, whilst also recognising that the realities of practicing the social sciences means these criteria are not generalisable to science as a whole.

10 Conclusion

A fuller understanding of the eminently practical (if not always clearly expressed) motivations behind Neurath's protocol sentence schema ought to dissolve any concerns about the practicality of Neurath's proposal. With regards to Uebel's bipartite metatheory thesis, recognising the details of Neurath and Carnap's mature conceptions demonstrates no theoretical disagreements that would undermine its plausibility. In fact, it demonstrates the essential conceptual and theoretical agreement masked by a subtle but significant difference in use of terminology. I also hope to have shown how the distinction between protocol bank and data bank can help us to conceptualise the continuity between the two parts of the meta-theory which can be masked by the division of labour and differences of emphasis that are an inevitable element of a bifurcated project, and the process of protocol acceptance within this.

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69 Carnap, 1936/37, p. 12.

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Caught in the Middle? Empiricism, Epistemology, and Metaphilosophy in the Development of Carnap's Views on Protocol Sentences

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Abstract

Of all participants in the debate on protocol sentences within the Vienna Circle, Carnap was the only one whose views changed substantially. The first part of this chapter reconstructs the development of Carnap's views. This development, of course, was fueled by the contributions of Neurath, Popper and Schlick. It was only in 1935/36 that Carnap reached a position that satisfied him. This mature conception of protocol sentences and its embedding in Carnap's metaphilosophical position with its dissolution of epistemology is dealt with in part two. Combining naturalistic and conventionalist elements, Carnap's proposed solution is pioneering, foreshadowing future trends. Nevertheless, as I shall argue, it is not completely successful in getting rid of traditional epistemological issues.

1 Introduction

The Vienna Circle's protocol sentence debate is a complex matter encompassing the issues of truth, physicalism, and intersubjective communicability. In this paper I shall confine myself to the core issue of the debate, namely the problem of confirmation. To begin with, it can be stated that protocol sentences are meant to provide direct contact with experience: they are sentences which justify all other synthetic sentences. Therefore, the theory of protocol sentences is intended to clarify the basic contention of the empiricist stance, i.e., the claim that synthetic sentences have to be justified by reference to experience.

Of all the proponents of the debate, surely Carnap is the one whose standpoint is most difficult to grasp. At least on the surface, it is obvious which stance the other players take. According to Neurath, protocol sentences deal – in a somewhat strange interlaced structure – with physical states including states of the observer himself. Popper takes them as singular existential sentences on

observable macroscopic physical states. Both of them take a fallibilist stance, viewing the acceptance of a protocol sentence as (provisional) endpoint of confirmation as resulting from a decision, as a matter of convention. Schlick, on the contrary, calls for “absolute”, infallible endpoints of confirmation, a role that is played by his “Konstatierungen”. They deal with present, private experiences exposing the rock-bottom ground beneath the protocol sentences.

By no means these positions are *per se* free from obscurities and problems. Unfolding these views amounts to no less than far-reaching qualifications of the original idea of the role of protocol sentences, i.e., their justificatory function by reference to experience.¹ Nonetheless, Carnap differs in so far that even such an abbreviating and distorting characterization of his position is simply not available. In Carnap’s case, one is inclined to characterize his view as conventionalist. This is not false, conventionalist ideas in one or another form play a major role throughout his whole career. However, this will not suffice to distinguish his view from those of Popper and Neurath, for example.

In the first place, the difficulty of easily grasping Carnap’s position is due to the simple fact that there is no single Carnapian theory of protocol sentences. Contrary to his colleagues or opponents, his views on this issue are “work in progress”. Within a relatively short time, from *Der logische Aufbau der Welt* on,² his views had developed rather quickly. On the one hand, as I will try to show, this development was driven by the problem of adapting the *Aufbau* to empiricism. On the other hand, naturally, this development was fueled by the ongoing debate and, most notably, by a kind of pressure from Neurath. Carnap’s development only came to an end in 1936 with the publication of “Testability and Meaning”. This is quite late, considering that the whole debate is usually presented as taking place in the first half of the '30s. While Carnap’s extensive paper was immediately recognized as being a substantial and path-breaking contribution towards a liberalized empiricism, it received very little attention in the context of the protocol sentence debate.

To secure fruitful discussion, I shall narrow down the subject, i.e., I will start by focusing on the problem of demarcating protocol sentences. Speaking of a justificatory role of protocol sentences presupposes a distinction between protocol sentences and ordinary synthetic sentences.³ But how do we achieve the unambiguous characterization of the former?

1 E.g., Neurath bans any talk of reality as metaphysical; Popper’s falsificationism does not acknowledge any positive value of confirmation.

2 Carnap 1928/2003; hereafter simply *Aufbau*.

3 A possible exception is Hempel who stated that “there is no essential difference left between protocol statements and other statements” (1935/2000, p. 19). It is only a possible exception since it is not entirely clear if Hempel should be read as a proponent of a conventionalist

Throughout the debate, there has been a number of different attempts to draw the line by different proponents. The distinguishing feature was viewed to be:

- epistemological: protocol sentences are infallible, incorrigible, etc.
- semantical/syntactical: the distinctive feature is to be found either in the content of protocol sentences (e.g., they deal with present experiences) or in their special syntactical form.
- naturalistic: protocol sentences are to be distinguished not by considerations a priori, but by the results of science.
- conventional: it is simply a matter of decision which sentences are used as protocol sentences.

Not being on the same level, there are several possibilities to combine, and in fact, the characterizations of Neurath, Popper and Schlick stated above consist in combinations. It is not only possible to employ some of these features together, but they seem to be related. The epistemological thesis of infallibility, for example, can only be entertained together with the semantical thesis that the content of protocol sentences does not exceed what is momentarily present to the mind.

2 From the *Aufbau* to “Testability and Meaning”

2.1 *The Aufbau and Its Base*

Reflecting on the epistemological order, Carnap chose an autopsychological base for the constructional system erected in the *Aufbau*. The domain of the autopsychological is epistemological prior, while the other domains (physical, heteropsychological, cultural) have to be constructed on that base.⁴ Given no more than this, one is likely to expect sentences on present experiences of a more or less traditional form (“There is now a red spot within my visual field”, or “I am now experiencing a sense-datum of such and such a kind”) as basic sentences. But nothing could be further from the truth. The only basic relation Carnap chose is characterized as purely formal at the beginning (as being asymmetrical; the basic elements are constructed as the field of this relation).⁵ In the beginning it makes no sense to speak of a self (as opposed to others) or of a psychological realm (as opposed to a physical realm). These

characterization (see below). On Hempel’s proposal within the context of the debate, see Uebel 2007, sec. 9.3.1.

4 *Aufbau*, § 58.

5 *Aufbau*, § 75, 78.

characterizations become possible only after the construction is carried out to a sufficient degree. Therefore, it is only because of the unfolding of the program and not because of the initial choice of a basic relation that we are entitled to speak of constructional systems with a certain kind of base:

In our system form, the basic elements are to be called experiences of the self *after* the construction has been carried out; hence, we say: in our constructional system, “my experiences” are the basic elements.⁶

By no means can this statement be viewed as a peculiar feature of the basic concepts; it is the *Aufbau's* central thesis that a concept is a concept only insofar as it is constructed, and therefore a definite place is assigned to it within the system. The proper base in the *Aufbau* is a conventionally chosen base for logical construction. Only after the construction has been carried out sufficiently is one entitled to identify this base as an autopsychological one. In this sense, to speak of an autopsychological base might be misleading. Surely, the choice of the basic relation is motivated by the intention of constructing the autopsychological realm before the other realms. Nonetheless, there is a categorical difference between the logical base for construction and the base in an epistemological sense,⁷ the latter being non-basic in the constructional sense:

[...] in fact, the constructed objects are objects of conceptual knowledge only *qua* logical forms which are generated in a certain way. Ultimately, this holds also for the basic elements of the constructional system. [...] It is only through this procedure, that is, only as constructed objects, that they become objects of cognition in the proper sense of the word, in particular, objects of psychology.⁸

While Carnap leaves no doubt that he intends the constructional system of the *Aufbau* to mirror the epistemological order, there is no attempt made in the *Aufbau* to account for epistemological priority. Traditional epistemological terms, such as “justification”, “certainty” etc., are employed nowhere; although at one point, Carnap speaks of “direct recognition” of the autopsychological.⁹ But once constructional priority is separated from epistemological priority, it is an open question how to account for this directness. One's amazement

6 *Aufbau*, § 65.

7 This difference is stated very clearly in Russell 1924/2007, p. 325f.

8 *Aufbau*, § 177.

9 *Aufbau*, § 58.

increases even further upon learning that, according to Carnap himself, this epistemological priority cannot be stated by epistemology:

How cognition can proceed from one object to another, how, in what sequence, and in which form the levels of a system of cognition can be formulated, – all this is contained in the indicated material. The theory of knowledge cannot ask any further questions.¹⁰

It would go beyond the scope of this paper to explore the *Aufbau* and its essentially neo-Kantian conception of epistemology in more detail,¹¹ nor can we further deal with the problem of the relation of the constructional and epistemological base within the *Aufbau*.¹² It is out of the question that Carnap considered the epistemological primacy of the autopsychological beyond dispute. This presupposition was not only disputed in the protocol sentence debate, but the notion of epistemological primacy itself was on the line.

2.2 *Physicalism*

On the next stage, in contrast to the *Aufbau*, the problem of a confirming base has been in focus from the beginning. This new standpoint is, of course, the physicalistic one.¹³ Specifically, the thesis of physicalism as presented by Carnap here for the first time divides into two sub-theses: first, the whole of science is capable of being expressed in physicalistic language, that is, the physicalistic language is the comprehensive system-language. This thesis denies that there are areas of science which cannot be framed in physicalistic terms, most notably psychology. Our focus here, however, is on the second sub-thesis: the protocol-language is also part of the one and only system-language. While starting with a two-language conception (protocol-language and system-language), we end up with the all-encompassing physicalistic system-language.

The protocol-language contains those sentences which are “based on direct experience”; without any addition they just record “the raw material”, referring “to the given”, describing directly “the simplest states of which knowledge can be had”.¹⁴ It is no coincidence that these characterizations used to introduce protocol sentences are rather vague:

¹⁰ *Aufbau*, § 178.

¹¹ Cf. Friedman 1992/1999, and Richardson 1998, especially Chap. 8.

¹² For a more detailed account on the tension between basic tenets of the *Aufbau* and verificationism cf. Friedl 2021.

¹³ For the shift from the *Aufbau*'s structural approach to physicalism, see Sauer 1989.

¹⁴ Carnap 1931/1934, pp. 42–45.

In the present state of research it is not possible to characterize this language with greater precision, i.e. to specify its vocabulary, syntactical forms and rules.¹⁵

More specifically, it is an open question as to whether protocol sentences speak of simple sensations, complexes of sensations (“Gestalten”, or entire sensory fields, or even the total experience of a moment), or material objects.¹⁶ While underdetermined with regard to both semantic content and syntactical form, Carnap offers an epistemological characterization. Protocol sentences “cannot be rejected”;¹⁷ while all other synthetic sentences have to be justified by reference to protocol sentences, the latter themselves “needing no justification”.¹⁸

The first thing to note here is a somewhat careless use of epistemological characterizations. Incorrigibility (impossibility of being rejected) is not the same as not requiring justification. This sloppy use of different epistemological concepts is also to be found in Schlick, who mostly speaks of infallibility, but also uses in one sentence the three diverging characterizations finality, indubitability, and incorrigibility.¹⁹ However, the main difficulty at this stage seems to consist in maintaining an epistemological privilege (regardless of which one is preferred in the end) and at the same time adhere to the thesis of translatability. To put it in a more formal way, the three propositions

- Protocol sentences are epistemologically privileged
- Protocol sentences are translatable into physicalistic system sentences
- Physicalistic system-sentences are epistemologically all on a par (fallible, need to be justified etc.)

do not go together.²⁰

2.3 “On Protocol Sentences”

Carnap’s conception of two languages – an epistemologically privileged protocol-language and a physicalistic system-language – united only afterwards, soon came under attack by Neurath, exactly along the lines of the argument just mentioned. According to Neurath, protocol sentences have to be conceived as physicalistic from the beginning. Therefore, an epistemological

15 Carnap 1931/1934, p. 45.

16 Carnap 1931/1934, p. 46f.

17 Carnap 1932/1959, p. 19.

18 Carnap 1931/34, p. 45.

19 Schlick 1935b/1979, p. 412; for discussion see Friedl 2013, Chap. v, Sec. 3. The first exposition of the theory of ‘Konstatierungen’ (‘affirmations’) is Schlick 1934/1979.

20 Cf. Coffa 1990, p. 358.

privilege is untenable. Neurath's objections culminated in the counterexample of a person who simultaneously utters two protocol sentences contradicting each other (writing one with the right hand, the other with the left hand).²¹ Carnap's rejoinder proceeded in two steps:

Firstly, he directly rejected Neurath's argument. Surely, one cannot hold on to contradicting sentences. But Neurath is misguided to conclude from this general fact that in the case under discussion at least one protocol sentence has to be abandoned. The whole discussion takes place in the system-language. Dropping at least one protocol sentence is but one possibility, while another is to regard at least one of the conflicting sentences to result from a faulty translation. It is not without a certain irony that Carnap, in defense of his conception, employs a strategy Neurath is well-known for: in case of contradiction, amendments are required; but the place where the amendment is to take place is not fixed yet.

Secondly, and more far-reaching, Carnap reassesses the issue in holding that the problem of determining protocol sentences is no theoretical question. There are several possibilities of fixing the semantical and syntactical structure of protocol sentences. Strictly speaking, Carnap now sees the following possible ways for framing protocol sentences:

- Protocol sentences outside the system-language and subsequent translation (Carnap's view at the previous stage).
- Protocol sentences within the system-language (therefore physicalistic), distinguished by a special syntactical form (Neurath's view).
- Protocol sentences within the system-language (therefore physicalistic), distinguished by the role they play. One and the same sentence can be used as a protocol sentence (when taken as provisional endpoint of justification) or as a system-sentence standing in need of justification (Popper's view).

Note that there is neither an epistemological, nor a semantical or syntactical characterization shared by all possibilities. An epistemological privilege is obtained only for the first one, which differs in respect to semantic content from its rivals. And only the second one is demarcated by a special syntactical form.²² Which one to choose corresponds to different possible ways the

21 Neurath 1932/1983, p. 95.

22 Put more precisely, Carnap as well as Popper demand protocol sentences to be concrete sentences resp. singular existential sentences. Albeit this is a syntactical characterization, protocol sentences are, of course, not singled out by this requirement.

scientific language is built. It is a matter of usefulness and convenience, and depends on the aims. In other words, in the end it is a matter of convention like all “questions” concerning the choice of a certain language. By converting the issue of the demarcation of protocol sentences into a matter of choosing the one or the other form of language, Carnap’s position at this stage is obviously a quite radical conventionalist one.

Carnap himself leaned strongly towards the third, the Popperian variant to which he was introduced by Popper himself in personal discussions during their holidays in the Tyrolian Alps immediately before writing his paper. According to this version, protocol sentences are to be characterized conventionally in two respects. First, as already stated, by preferring this version to its two rivals, and second, by the fact that within this version, the characterization of a sentence as a protocol sentence is always relative to a certain concern. In another context (in the case of doubt, or the need for closer examination, etc.), the same sentence can be taken to stand in need of justification by other protocol sentences.²³ A protocol sentence is distinguished by the fact that we decide to stop further justification without saying that we have reached an absolute end. We are satisfied to pursue the task of justification up to this point – at least for the moment. This decision can be revised by changing the former protocol sentence into a questioned hypothesis.

For the time being, Neurath seemed to be satisfied. After all, the legitimacy of his own position was acknowledged. In addition, Neurath adhered to the view that no sentence is immune from being dropped, and that acceptance of each single sentence (whether protocol sentence or not) is always a matter of decision. Following the intense discussion in their letters preceding the joint publication of Neurath’s ‘Protokollsätze’ and Carnap’s ‘Über Protokollsätze’ at the end of 1932, the issue was no longer a central topic in the correspondence for a while.

However, ceasefire was secured only for a short time. In the course of 1934, particularly after reading Popper’s book (published in the fall of 1934, officially dated 1935), Neurath insisted on the following point: The language that Carnap, following Popper, preferred then, was flawless from a logical point of view; nonetheless, there was a decisive defect, namely that choosing such a

23 According to Popper, conventionalism is the thesis that laws of nature (universal propositions) are fixed by convention, whereas he himself treats basic sentences (singular propositions) that way (1935/2002, p. 91f.). But one wonders how this differentiation is supposed to work: any universal proposition can be defended, simply by choosing the appropriate basic sentences; therefore, conventionalism in regard to basic sentences entails conventionalism in regard to laws; cf. Stegmüller 1969, pp. 360–62.

language means betrayal of empiricism. To take arbitrary sentences to function as protocol sentences simply ignores the basic empiricist tenet. Neurath himself, on the contrary, held on to the idea that protocol sentences have to be characterized as observation-sentences; whoever does not go the same way can no longer be called an empiricist. To be sure, Neurath's attacks, in his letters to Carnap, were primarily aimed at Popper, and only secondarily at Carnap, forcing the latter to clarify his own position. Indeed, quite at the beginning of the reopening of the debate, Carnap acknowledged certain misgivings:

It would have been better if, in accordance with my wish, you had not brought up the matter at that time, when everything is still unclear. Now we have the damage: all people find the opportunity to criticize our, especially your formulations, and partly rightly so. We should rather publish only when things are sufficiently clear to ourselves.²⁴

Forced by Neurath to state his discomfort more precisely, Carnap's next letter reads:

The remark about premature publication refers to both our essays; but not to those on the general problems of physicalism, but to those on protocol sentences. This problem does not seem sufficiently clear to me (even today). I meant that you should not yet have fully addressed this problem before we ourselves are at least halfway clear about it.²⁵

One year later, in the summer of 1935, Carnap admitted that he had never published a satisfying account:

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- 24 "Es wäre besser gewesen, Du hättest, meinem Wunsch entsprechend, damals nicht schon die Sache aufgerollt, wo doch alles noch unklar ist. Jetzt haben wir den Schaden: alle Leute finden Gelegenheit, unsere, besonders Deine Formulierungen zu kritisieren, und zum Teil mit Recht. Wir sollten lieber erst dann veröffentlichen, wenn uns selbst die Dinge hinreichend klar sind" (Carnap to Neurath, May 18, 1934, ASP-RC-029-10-63).
- 25 "Die Bemerkung von vorzeitiger Veröffentlichung bezieht sich auf unser beider Aufsätze; aber nicht auf die über die allg. Probleme des Physikalismus, sondern auf die über Protokollsätze. Dieses Problem scheint mir (sogar heute noch) nicht hinreichend klar. Dieses Problem, meinte ich, hättest Du noch nicht in vollem Umfange aufrollen sollen, bevor wir selbst uns wenigstens halbwegs klar darin sind" (Carnap to Neurath, June 8, 1934, ASP-RC 029-10-61).

My view on protocol sentences is not yet clearly formulated anywhere. The important points are, however, partly discussed in an English essay I am currently writing.²⁶

These concessions are not easy to understand. One wonders how physicalism can be stated while omitting the question of how to include protocol sentences (as Carnap himself had stated right from the beginning as one major task). Putting this aside, it is obvious that Carnap's "On Protocol Sentences"²⁷ was by no means his final word on the matter. To be sure, this is an important paper, e.g., the soon to be called "principle of tolerance" is at work here for the first time. Regarding the issue of protocol sentences, however, it is nothing more than a snapshot of a certain stage in Carnap's development. This remark seems to be indicative, because, up to now, this paper has often been regarded as Carnap's main contribution to the debate.

I shall now turn to the second part of this paper and attend to Carnap's final position outlined in the paper mentioned in the quote just given, namely "Testability and Meaning".

3 Paris 1935 and "Testability and Meaning"

Carnap's mature position emerged in his talks delivered at the first conference for Unity of Science in Paris in September 1935, which appeared in print the year after.²⁸ These papers mark a significant milestone in his development, e.g. by the adoption of the semantic conception of truth (from now on strictly delimiting truth from confirmation), or the introduction of non-eliminative reduction instead of definability, a decisive step towards a liberalization of empiricism. At the same time he was working on the book-length essay "Testability and Meaning", elaborating in detail some of these new ideas.²⁹ Taken together, in these writings, not only did Carnap develop a new account of protocol sentences, but also delivered a re-consideration of the whole discipline formerly called "epistemology". It is to bear in mind that one cannot

26 "Meine Ansicht über Prot.-Sätze ist allerdings noch nirgends deutlich formuliert. Die wichtigen Punkte werden aber zum Teil in einem engl. Aufsatz besprochen, den ich gerade schreibe" (Carnap to Neurath, June 22, 1935, ASP-RC 029-09-44).

27 Carnap 1932/1987.

28 Carnap 1936a, 1936b, 1936c/1949.

29 Carnap wrote the first version in spring 1935, the final version in spring 1936 (Carnap to Olga Neurath, August 27, 1936, ASP-RC 102-52-33).

grasp Carnap's mature position on protocol sentences without considering the way it is embedded in his comprehensive conception.

Epistemology, up to now, is "an unclear mixture of psychological and logical components".³⁰ The main target of this critical remark is Schlick (although not named) who stated that the issue of protocol sentences is a psychological problem.³¹ But Schlick was not the only one who fell prey to this confusion – Carnap confessed that this unclear mixture is also to be found in his own earlier work.³²

Most notably, the adoption of an empiricist standpoint is conventional. It is misguided to view empiricism as an assertion. Much better, empiricism is formulated as the proposal to adopt a certain form of language. This restricted form of scientific language requires that descriptive predicates and synthetic sentences are admitted only if there is a certain connection to an experiential base.³³ Once this decision is made, one is obliged to state such a base. This task cannot be achieved by pure convention, since empiricism demands for testing by means of experience and not by means of arbitrarily chosen sentences. In other words, as long as protocol sentences are not characterized, empiricism itself does not occupy a distinguished position. In any case, protocol sentences cannot simply be chosen in a completely free way. This is an important self-correction on the part of Carnap because that was exactly the way he went about it in "On Protocol Sentences". In another respect, Carnap still adhered to his former view, namely that protocol sentences are neither distinguished by a special syntactical form, nor by their semantic content. As before, in the end, it is a matter of convenience, a matter of considerations of usefulness, whether protocol sentences are formulated in a phenomenalist or physicalist form. In other words, semantically, protocol sentences are not uniquely determined. Carnap now envisages the following possibilities: protocol sentences must contain observable predicates; these predicates can be:

- a) psychological predicates in phenomenalist language
- b) psychological predicates in physicalist language
- c) physicalist predicates of the "Thing-Language"

This is close to the three possibilities Carnap envisaged in 1932 (cf. above, sec. 2.3), but there are important differences. The possibility of completely freely-chosen protocol sentences is no longer a real option, instead there is now a

30 "[E]ine unklare Mischung aus psychologischen und logischen Bestandteilen" (Carnap 1936a, p. 36).

31 Schlick 1935a/1979, p. 404; see also Schlick to Carnap, April 16, 1935 (ASP-RC 102-70-14).

32 Carnap 1936a, p. 36.

33 Carnap 1936/37, p. 33.

subdivision within physicalistic language. Differing in the semantic and syntactical structure, there is a common feature among all options: the predicates have to fulfil the criterion of observability. That is the distinguishing feature of protocol sentences, securing the demands of empiricism, establishing the connection with experience. Protocol sentences, containing observable predicates, are therefore those sentences that stand in proximity to experience, or, as Carnap prefers to say, they are “confronted” with observation.

3.1 *The Criterion of Quick Decidability*

The crucial question now is how to characterize “observability”, i.e. how to make clear what is meant exactly by “confrontation with observation”. In admitting the indispensability of direct testing, Carnap appears to be closer to Schlick, but unlike Schlick, Carnap neither views this process as a comparison between sentence and reality³⁴ nor does he adhere to Schlick’s thesis that this procedure necessarily takes place in the private domain of one’s own experiences. Instead, to clarify the nature of “confronting” and therefore to obtain a criterion of demarcating protocol sentences, we have to turn towards science: “The description of that procedure is not a matter of logic but is itself empirically-scientific.”³⁵

This is nothing but a straightforward consequence of Carnap’s conception of philosophy: Everything that remains of philosophy is logic of science. Questions of logic of science are nothing but problems of language. Since confirmation by experience resp. confrontation with reality transcends the limits of language, it should not be supposed to be part of logic of sciences; it rather belongs to science itself and not to philosophy proper. Although Carnap had at the time already outpaced the general restriction to syntactical features of his “syntactical period” (culminating in Carnap 1934/1937), this point can be illustrated by the distinction of the material and the formal mode of speech: Whereas the factual statements belong to the material mode of speech, the proper philosophical statements are in the formal mode, concerning syntactical features of language.

Therefore, protocol sentences cannot be determined by purely philosophical considerations. It is an empirical, scientific task to determine what it is for a sentence to be tested by direct confrontation with experience. This distinctive feature should be delivered by – what I call – the *criterion of quick decidability*:

34 For Carnap’s critique of Schlick’s conception of comparison, see Carnap 1936c/1949, p. 125f.

35 Carnap 1936c/1949, p. 124.

We shall speak of “directly testable statement” when circumstances are conceivable in which we confidently consider the statement so strongly confirmed or else disconfirmed on the basis of one or very few observations that we would either accept or reject it outright.³⁶

Or, as it is put a little more precisely in “Testability and Meaning”:

A predicate “P” of a language L is called *observable* for an organism (e.g. a person) N, if, for suitable arguments, e.g. “b”, N is able under suitable circumstances to come to a decision with the help of few observations about a full sentence, say “P(b)”, i.e. to a confirmation of either “P(b)” or “ \sim P(b)” of such a high degree that he will either accept or reject “P(b)”.³⁷

The whole of epistemology is structured that way: the philosophical part, dealing with relations between sentences, including those sentences which are taken to be basic. This part, of course, is the logic of science. Here, we are solely concerned with relations between sentences; there is no place left for an epistemological subject of whatever kind. In this sense, traditional epistemology is completely abandoned, it is epistemology without a knowing subject.³⁸ On the other hand, within the logic of science there is no possibility of distinguishing protocol sentences. To put it more precisely: there is no possibility of distinguishing basic sentences in a way not wholly conventional, which – as Carnap admits – does no justice to empiricism. To take an empiricist stance, it is indispensable to opt for direct confirmation. Therefore, protocol sentences are to be distinguished within the realm of the empirical by means of scientific investigations.

It is instructive to relate this interplay between science and logic of science with the characterization of epistemology delivered in the *Aufbau*. There, too, Carnap held that the only epistemological task is to investigate the relations between propositions.³⁹ But now Carnap acknowledges the need for supplementation – at least, if one wants to do justice to the demand of empiricism – since if the proper philosophical task consists only in the investigation of the relations between different sentences, there is nothing to be said about

36 Carnap 1936c/1949, p. 124.

37 Carnap 1936/37, p. 454f.

38 See Uebel 2018, p. 372; Uebel’s account of the transformation of Carnap’s epistemology has been a valuable suggestion for this paper; a point of disagreement is treated within the concluding section.

39 Cf. *Aufbau*, § 178 (quoted above, sec. 2.1).

the basic level itself. Traditional epistemology violates the sharp distinction between logic of science (proceeding in the formal mode) on the one hand, and of science (proceeding in the material mode) on the other. To complain about the unclear mixture of the different parts by no means simply implies that one part (the scientific) is dispensable.

Thus, Carnap's final approach comprises the basic new idea of distinguishing protocol sentences in terms of scientific results, by means of the scientific inquiry into behavior. On the one hand, this move is demanded by the metaphilosophical position Carnap had developed by that time (a position he continued to hold, albeit not in the restricted form of his syntacticism). On the other hand, this seems to be a necessary supplement to the *Aufbau* project with its incapacity to distinguish epistemological priority. Seen from both sides, the handover of the issue to science seems to be the only available option, as long as one is intent on doing justice to the basic demand of empiricism. To the resulting criterion of *quick decidability* itself, Carnap clearly states that it does not yield a definition in the strict sense; "observability" is taken to be a necessarily vague concept:

There is no sharp line between observable and non-observable predicates because a person will be more or less able to decide a certain sentence quickly, i.e. he will be inclined after a certain period of observation to accept the sentence.⁴⁰

4 Assessing Carnap's Final Position: Epistemology Dismissed?

Looking back at his development, it is clear that at the beginning the issue of protocol sentences was not the focus of Carnap's work. He did not start his career as a marked proponent of empiricism; the *Aufbau* is designed as a neutral position to overcome traditional disputes. Neither the function of providing contact with reality, nor the function of terminating points of confirmation can be bestowed on the (constructional) base of the *Aufbau*. Protocol sentences, which should exactly serve that purpose, do not seem to be capable of being integrated in this system. Similarly, protocol sentences appear to be a residual problem for the next stage, namely physicalism. The main problem here is to make sense of the idea of endpoints of confirmation needing no

⁴⁰ Carnap 1936/37, p. 455. Carnap does not mention that the criterion cannot function as a genuine definition of "observable" if only because of the circularity contained in it.

confirmation, which is simply not in accordance with the required translatability of protocol sentences into physicalist language. To opt for a completely conventionalist base, as Carnap does at the next stage, is simply abandoning empiricism. After all these attempts, Carnap himself admitted that the issue of protocol sentences (and therefore the problem of confirmation by experience) was an open question.⁴¹ This issue became all the more urgent because of the metaphilosophical position Carnap had adopted at that time: according to the characterization of the proper domain of philosophy, he had to clarify the nature and place of epistemological issues.

No wonder that Carnap was very sensitive to the contributions of his fellow scholars at the time. He integrated what he took to be insights of his friends and opponents in his mature standpoint, but also disagreed in other points with each of them. Like Schlick, Carnap tried to make sense of the notion of direct confirmation, but he viewed Schlick's attempt to be a confused mixture of philosophical and psychological considerations. Like Popper and Neurath, he opposed the infallibility Schlick claimed, taking protocol sentences as semantical complex hypotheses. He joined Neurath's camp against Popper in holding on to the idea that protocol sentences are not to be distinguished purely conventionally – they are observational reports.⁴² In opposition to Neurath, he sided with Schlick in rejecting a radical coherentist view of justification by opting for a direct confirmation of protocol sentences (see below); due to that characteristic they are able to function as (provisional) regress stoppers.

Nonetheless, the final stage of Carnap's development is not only synthesizing but also highly original. One aim of the paper was to show the emergence of a solution in response to what he saw as shortcomings of his earlier positions. In the end, he reached a standpoint which combines different elements at different levels:

It is – and according to Carnap's metaphilosophical stance, it has to be – a matter of science to determinate protocol sentences; therefore, protocol sentences are characterized in a *naturalistic* way by the *criterion of quick decidability*.

A matter of *convention* is, in the first place, the decision for an empiricist language, which is characterized by admitting descriptive sentences only if

41 See above, sec. 2.3.

42 In addition, Carnap raised the objection against Neurath that protocol sentences have to deal with observations themselves and are not constructed as sentences on other sentences. Neurath took Carnap's charge as a misunderstanding; for a reading of Neurath's somewhat strange, interlaced protocol sentences according to which Carnap's charge is really a misunderstanding, see Bentley's chapter in this book.

there is a certain connection to protocol sentences. We are not concerned here with the appropriate characterization of this connection (by 1935, Carnap had replaced definability by so-called reduction sentences, but this was not his final word on the matter). But even concerning the protocol sentences themselves there is an inevitably conventional choice: the criterion of *quick decidability* does not determine the semantic/syntactic character of observable predicates. It remains a matter of decision to opt for physical predicates of perceptible things surrounding us (the thing language). In contrast to psychological predicates, this choice has the advantage of being the only intersubjective base, while the other way, an intersubjective language has to be constructed on the basis of different subjective languages.⁴³

This detachment of the characterization of protocol sentences from their syntactic/semantic features is of utmost importance and can be highlighted by a contextualization as well. This detachment not only clarifies the confusion of the two types of bases of the *Aufbau*, it also cuts the ties to the elementary propositions of Wittgenstein's *Tractatus*. There, the existence of such propositions as simple and indivisible units is a precondition of the picture theory: they are characterized semantically; as Wittgenstein puts it, their existence is assured by logic.⁴⁴ Apart from the contentious issue as to whether verificationism is at least implicitly contained in the *Tractatus* itself, the elementary propositions were understood in the Circle, above all, in an epistemological sense. From the perspective of further development, it was Feyerabend who made prominent use of Carnap's ideas. Under the name "pragmatic theory of observation" this view became a key idea in his crusade against what he took to be the prevailing sterile, ahistorical and dogmatic view.⁴⁵

4.1 *The Criterion of prima facie Credibility*

What has been achieved so far is a demarcation of protocol sentences, which fits into Carnap's mature conception of philosophy. The criterion of *quick decidability* achieves the distinction of protocol sentences in terms of their formation. Surely it is a scientific task to mark up those sentences that are held

43 Carnap 1936/37, p. 10f. Since Carnap still allows such purely subjective languages as starting points (while now admitting that such a choice is inconvenient), it is misleading to view his abandonment of "methodological solipsism" as the decisive step beyond the *Aufbau*. After all, it was only due to misunderstandings that he finally dropped that term; cf. Carnap 1936/1937, p. 423f.

44 Wittgenstein 1921/1961, 5.5562; for a summarizing account of the *Tractatus*' doctrine of elementary propositions cf. Glock 1996, pp. 102–107.

45 Most prominently in Feyerabend 1962/1981; for discussion of the relation to Carnap's theory of protocol sentences, see Oberdan 1990 and Kuby 2018.

true by subjects as an immediate result of making observations. But there is still something missing.

The problem is to map out an empiricist account of the role of protocol sentences in the process of justification. And to answer this problem, the demarcation of protocol sentences can be nothing but the first step. The second step has to consist of a defense of the claim that justification – at least provisionally – is gained by reaching “quickly decidable” statements. In other words, just to demarcate a certain class of sentences will not do; it has to be shown that this distinguished class of sentences plays that special role in the process of confirmation. This special role of protocol sentences cannot be accounted for in specifying the circumstances of their production. The characterization of protocol sentences as directly confirmed sentences does not include any privileged status. For Carnap, this privileged status cannot be found in an alleged infallibility, but a weaker kind of such a privilege seems indispensable if one wants to adhere to the idea that ordinary hypotheses are to be tested by means of protocol sentences. And Carnap himself clearly expresses his commitment to this:

Now, if confirmation is to be feasible at all, this process of referring back to other predicates must terminate at some point. The reduction must finally come to predicates for which we can come to a confirmation directly, i.e. without reference to other predicates.⁴⁶

Justification is needed to stop the process of “referring back” when reaching protocol sentences, i.e. directly confirmed sentences. As I understand it, it is for that purpose that another criterion comes into play, which concerns the role of protocol sentences as (at least provisional) terminating points of confirmation:

A statement established on the basis of the first operation [confrontation with experience, J.F.] is held as (sufficiently strongly) confirmed as long as in the second operation [confrontation with other statements, J.F.] no statements are found which were previously established by confirmation but are incompatible with the statement under consideration.⁴⁷

46 Carnap 1936/37, p. 456.

47 Carnap 1936c/1949, p. 125.

I will call this criterion the principle of *prima facie credibility*.⁴⁸ Taken together with the first one, the criterion of *quick decidability*, the outline of the nature and the role of protocol sentences within Carnap's general conception of philosophy is completed: The proper task of philosophy – better yet, the logic of science – is to construct and to investigate languages. To opt for an empiricist language amounts to relate all synthetic sentences to a subclass, which serves as point of contact with “reality”. The demarcation of this subclass is not a matter of logic of science; science itself distinguishes observation sentences by the criterion of *quick decidability*. Therefore, observation sentences are neither characterized by a supposed simplicity (“atomic propositions”), nor by a purported epistemologically privileged status like infallibility. Nevertheless, they are able to serve as – at least temporarily and provisionally – terminating points of empirical confirmation by enjoying the privilege of *prima facie credibility*. If there is no evidence to the contrary, they are terminating points of confirmation.

It cannot be overstated that in acknowledging the need for direct confirmation, Carnap still holds on to a basic tenet of empiricism in a more traditional sense. This differs sharply from strategies in which (provisional) termination of justification is sought by appealing to further instances. There are different ways such a strategy can take: one might appeal to the reliability of the process by which protocol sentences are acquired, or simply by referring to our practice as codified has proved to be successful. No matter how such accounts are worked out in detail, they all have in common that the “reliability” of protocol sentences results from other factors. Therefore, protocol sentences are not terminating points of justification; there is no terminating point at all since the reliability itself (or our hitherto successful practice) cannot simply be taken for granted. Stating these conditions, we get hypotheses which stand themselves in need of empirical justification, a process that refers to protocol-sentences etc., etc. It is out of the scope of this paper to discuss such “holistic”, “coherence-theoretical” accounts in more detail, for the present context it is sufficient to state that on any such account, it is denied offhand what Carnap is looking for, i.e. direct confirmation.

Let's turn to the criterion of *prima facie credibility* itself. At the least, it seems to be a traditional epistemological principle. In contrast to the criterion of *quick decidability*, its integration in Carnap's metaphilosophical stance has to

48 By identifying this criterion as a necessary supplement, I differ from Richardson (1998, p. 216) and Uebel (2018, p. 371) who view epistemological issues of whatever kind to be already done away by the naturalistic and conventionalist elements of Carnap's final position.

be put up for discussion. Another issue is if this criterion is a good one in its own right. I will discuss the latter issue first.

Slightly simplified, the criterion of *prima facie credibility* states that protocol sentences are confirmed as long as there are no contradicting sentences. That cannot mean that protocol sentences are confirmed by other sentences. Therefore, they have to be taken as points where there is – at the moment – neither negative nor positive evidence; protocol sentences are simply unexamined, “neutral” items with nothing for or against them. But surely this neutrality does not suffice for confirmation, otherwise any suitable, purely fictional hypothesis would count as *prima facie* justified. The latter case is typical for the demand of confirmation, and it is odd to demand for confirmation in this case and not in the case of neutral protocol sentences.

Nor is it of any help to emphasize the provisional nature of this purported confirmation. The phrase “as long as there are no contradicting sentences” stands in need of clarification. For each contingent sentence, a contradicting sentence can easily be “found”, most simply by negation. Hence, is it meant that those contradicting sentences are in fact entertained? Just holding a refuting sentence is still not sufficient, otherwise any sentence contradicting a protocol sentence would count as rebutting instance, however futile it may be, if it is hold to be true by – let us say – a mentally deranged person. To refute a protocol sentence, we demand that the contradicting sentence has positive evidence in favor of it; Carnap acknowledges this by admitting that the refuting evidence must consist of sentences “which were previously established by confirmation”. But what he does not seem to see is that his explication of empirical confirmation runs in a circle. In brief, absence of contradicting evidence will not do, positive evidence is required. This condensed skeptical argumentation⁴⁹ may seem unfair to Carnap, treating him as a proponent of fallible foundationalism, to use contemporary terminology. But this problem is a straightforward outcome if one takes the relation of confirmation as having a direction, like Carnap himself – insofar the accusation of circularity is a serious one. Furthermore, it should be noted that the situation is not defused by the fact that, for Carnap, opting for an empiricist language is a conventional choice. The issue here is raised within that empiricist framework and does not affect the framework itself.

It is out of the question that Carnap did not want to get tangled up in such traditional epistemological disputes. His new conception of philosophy is, at

49 The argumentation is essentially a summary of Rutte 2000, pp. 89–91, who raises these objections against Popper and his school.

least in part, designed to get rid of issues like that. Hence, we have to discuss whether Carnap could find a way to incorporate the criterion without taking it as an epistemological criterion *sui generis*. Obviously, it cannot be taken as belonging to the logic of science. In this field, relations to other sentences within a conventionally chosen framework are the subject of investigation. But the utmost we can reach is assurance that there are no contradicting sentences; this is completely different than granting the right to stop the process of confirmation. Therefore, it seems, the principle falls within the domain of science: it is a naturalistic criterion. Taken as such, it amounts to the empirical hypothesis that people “trust their eyes” and stop further investigation until they are confronted with contradicting evidence. It can hardly be denied that we actually end the process of referring back when reaching protocol sentences, but that was not the question. The issue has been if this practice is legitimate, or in other words, if we are justified in adhering to the practice (within the empiricist framework).

Obviously, we have reached one of the big issues in modern epistemology: the difficulty of placing the criterion of *prima facie credibility* reflects the problem of the nature of epistemological principles. Due to their normativity, they do not seem to be reducible, neither to conventional rules of language (at least, if one wants to stick to the idea of empirical confirmation), nor to science. My aim here was to state that there is more than just a little residual problem of minor importance; that getting rid of traditional epistemological disputes is not so easy. By no means, however, do I want to say that this problem constitutes a definite refutation of Carnap. Some scholars look with optimism to the prospect of incorporating Carnap within the naturalist’s camp.⁵⁰ In any case, with his mature view on protocol sentences, Carnap stands right at the beginning of this debate, albeit, in several respects, he seems to be caught in the middle.

Acknowledgments

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⁵⁰ Uebel 2018, p. 375f.; as already noted, Uebel does not pin this down on the criterion of *prima facie credibility*.

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Carnap and Neurath on Truth, Meta-language, and Semantics

Ulf Höfer

Abstract

The question of truth and semantics as an instrument for its solution is a repeatedly discussed topic within Vienna Circle and its upshot documenting fundamental disagreements among its members in genuine philosophical matters which go far beyond the often stressed left- vs. right-wing fractions of the Circle. While Carnap understands truth as a matter of logic and Tarskian inspired semantics including metalanguages as a means to handle it, Neurath principally refuses such a concept of truth and opposes semantics and hierarchies of languages. He only wants to allow acceptance as a pragmatical substitute – not as translation or synonym – for ‘true’ within a single universal jargon. The debate on truth between Carnap and Neurath lasted from the early '30s up to the mid '40s without a conclusive settlement. It is reconstructed here on the basis of their correspondence and with special respect to some unpublished papers they exchanged in the course of the Third International Congress for the Unity of Science in 1937 in Paris.

While in 1935 Hempel contrasted Schlick’s account of truth with that of Carnap and Neurath, one might receive the picture that – apart from some negligible details – the latter had both developed the authoritative theory of truth for the Vienna Circle and for modern philosophy too.¹ Nowadays the fact that Carnap and Neurath had conflicting opinions here and in other central questions of philosophy is broadly recognised as well as the fact that the Vienna Circle was not a uniform aggregation of like-minded scholars.² Thus the question of truth and semantics as an instrument for its solution form a more or less constant

1 Cf. Hempel 1935, especially pp. 56ff. Something like that holds for Kraft too, who – more or less ignoring Neurath – focuses on Carnap and contrasts *the* Vienna Circle position to Russell, Wittgenstein or Popper, cf. Kraft 1968, chs., A.11.2 and B.11.1–3.

2 To mention just a few, I refer to Rudolf Haller, Friedrich Stadler and Thomas E. Uebel, cf. i.a. Haller 1979, ch. 6; Haller 1993; Stadler 1997, e.g. ch. 7.2.5.2–5; Uebel 1992a, ch. 1.3.

topic in the discussions within the circle and its upshot for more than a decade. It is this question that can be analysed as a paradigmatic case involving different opinions within the Vienna Circle in that the proponents interacted with one another to arrive at a joint solution while – out of respect for the movement as a whole – giving their interactions an air of secrecy. The controversy about truth and semantics has been examined several times³ so another glimpse at it could be understood as carrying coals to Newcastle. But most of these analyses have exclusively examined published sources. Only recently A.W. Carus has dealt with the Carnap-Neurath-correspondence in a similar manner albeit focusing entirely on the later years. He defends Carnap against Neurath's attacks and concludes that the differences could be regarded as "largely terminological" and solvable in the end.⁴ Similarly, Thomas Uebel speaks of "very different but not incompatible ways" referring to Carnap, Neurath and Frank.⁵ Of course, such optimistic reconstructions are maintainable – a long lasting friendship, the common movement and the fact that both are philosophically much closer to each other than to Wittgenstein or even Heidegger do bolster these assertions. Nevertheless, they might still be too optimistic concerning the very fundamental discrepancies between Carnap's and Neurath's accounts of truth.

It is these discrepancies that I am trying to shed some light on. In doing so and in trying to avoid rehashing already well-established accounts, I shall concentrate on the direct exchange between them, i.e. by examining their correspondence and some unpublished papers they sent one another because in contrast to their publications – where they are more cautious and conciliatory – these documents advance their unvarnished views. I am going to omit issues, which have been examined in depth earlier, for instance, the discussions in the context of the 1935 conference in Paris. Instead I shall focus on the material in connection with the 1937 conference in Paris, which have been almost neglected up to now and which present their positions in a more elaborated and even sophisticated way. To start off, in the first part, I shall try to reconstruct their dispute about the name 'Semantics' as a superficial debate, substituting the deeper conflict regarding their fundamental philosophical positions, which will be subject of the second part.

3 See e.g. Coffa 1991, ch. 16.; at length Hofmann-Grüneberg 1988, pp. 96–104 and ch. 7, esp. pp. 149–161; Mormann 1999, p. 174f.; Oberdan 1992.

4 Carus 2019, p. 339, see also p. 340f.

5 Uebel 2006, ch. 4.

1 The Debate about ‘Semantics’ as the Name of “Syntax”

Dear Neurath!

Gödel suggested the term ‘semantics’ to me, which also the Poles have used occasionally. Behmann happens to suggest me the same. I actually like it better than ‘syntax’. Please quickly write a line on what you think about it, because in continuing to write the text, I want to use the term. As a book title then perhaps ‘General Semantics’ with some subtitle.⁶

Thus Carnap asked Neurath for his opinion at the beginning of April 1932. Neurath replied immediately and expressed his displeasure wholeheartedly:

Dear Carnap!

Neither me nor Olga can make friends with semantics. A bad illustration, reminiscent of ‘mantics’, art of clairvoyance. Strange and scholarly in a bad sense. In addition, as the educated woman knows, semantics is wrong, moreover, it should read ‘semiotics’, semeion is the sign, thus semiotics – the study of the characteristics of diseases. As theory of signs per se in Leibnitz, Lambert, but in a very narrow, unpleasant sense. Sema, the sign, knows no semantics.⁷

And he suggested ‘syntax’ or ‘logical syntax’ as the better name:

6 Carnap to Neurath, April 4th, 1932. All translations are mine, for convenience I add the German passages in the footnotes. Orig.: “Lieber Neurath! Gödel hat mir Terminus ‘Semantik’ vorgeschlagen, der auch von den Polen gelegentlich schon verwendet worden ist. Zufällig schreibt jetzt Behmann mir denselben Vorschlag. Er gefällt mir eigentlich besser als ‘Syntax’. Schreib bitte schnell eine Zeile, was Du dazu meinst, weil ich im Weiterschreiben des Textes den Terminus schon gebrauchen will. Als Buchtitel dann vielleicht ‘Allgemeine Semantik’ mit Untertitel.”

7 Neurath to Carnap, April 9th, 1932. Orig.: “Lieber Carnap! Weder ich noch Olga können uns mit Semantik befreunden. Ein übles Wortbild, an ‘Mantik’, Seherkunst erinnern. Fremd und gelehrtenhaft im ungunen Sinne. Dazu kommt, dass, wie die gelehrte Frau weiss, Semantik überdies noch falsch ist, es heisst ‘Semiotik’, Semeion heisst das Zeichen, davon Semiotik – die Lehre von den Kennzeichen der Krankheit. Als Zeichenlehre schlechthin bei Leibnitz, Lambert, aber in sehr engem, wenig erfreulichem Sinn. Sema, das Zeichen, kennt keine Semantik.” It is impossible to reproduce this aspect of Neurath’s doubts in English, the correct translation of ‘Mantik’ would be ‘divination’, but the point is the similar acoustic representation of ‘Mantik’ and ‘semantics’ in German, therefore I use ‘mantics’ as makeshift. Cf. also below, fn. 11 and 16.

We both think that SYNTAX – Logical Syntax – General Syntax or the like sounds much better and might be popularised if necessary.

Logical syntax sounds familiar, semiotics old-fashioned and grave. Syntax sounds sharp and bright.⁸

In addition to Gödel also Tarski and Behmann appear in Carnap's correspondence as proponents of the term 'semantics' to be introduced as a new element in philosophical language. And even Neurath names a proponent, but remains depreciative himself:

Bühler just came to me to get systematic information on visual statistics, from the point of view of 'visual language' i.a. 'pedagogy' etc.

We talk a lot about his linguistic research. I think we should find a bridge to communicate with him.

He uses the word 'semantic' plus 'sematology' etc. But I'm for 'syntax'.

Best regards to Ina

Yours

ON⁹

Carnap, in turn, made a few more attempts to change Neurath's mind.:

Couldn't you gradually make friends with the word "semantics"? It's linguistically okay, by the way. Its formation does not correspond to the common rules of word formation, but the spirit of language is as unpredictable as the counsel of God. Aristotle uses the adjective 'semantikós'; he says the statement is a 'phoné semantiké'; a beckoning sound. The Greeks later called their musical notation 'semantics'. The word sounds a bit strange and scholarly at first, but that can soon change. Today 'Arithmetic' would sound the same to us if we heard it for the first time. The advantage of semantics over 'syntax' is that it is clearer. If you use the

8 Ibid. Orig.: "Wir meinen beide, dass SYNTAX – Logische Syntax – Allgemeine Syntax oder dgl. viel besser klingt und zur Not popularisiert werden kann. Logische Syntax klingt vertraut, Semiotik altväterisch-gravitätisch. Syntax klingt scharf und hell."

9 Neurath to Carnap, April 20th, 1932. Orig.: "Augenblicklich kommt Bühler zu mir, um sich über Bildstatistik systematisch informieren zu lassen, unter dem Gesichtspunkt 'Bildersprache' u.a. 'Pädagogik' usw. Wir reden viel über seine Sprachforschung. Ich finde, man sollte eine Brücke finden, sich mit ihm zu verständigen. Er verwendet das Wort 'semantisch' und dazu 'Sematologie' usw. Aber ich bin für 'Syntax'. Herzliche Grüsse auch an Ina Dein ON".

word 'syntax' somewhere, you should always say 'not in the philological sense'.¹⁰

And from mid-1932 Carnap almost exclusively spoke of his semantics. Neurath, however, remained stubborn and, as we know, ultimately prevailed in this dispute: *The Logical Syntax of Language* is not called 'Semantics'. And he provided a few more reasons against choosing 'semantics':

'Semantics' does not become more palatable through this communication. Since 'mantics' is known to everyone as the art of divination, the less highly educated will think that 'semantics' is a variety of mantics. Please tell me right away what Frank thinks about it. I have already shown you that 'semantically' is in use. There is also semasiology – the theory of meaning. I deem 'logical syntax' already quite academic. I would probably not agree with your terminology, but try to bring 'logical syntax' through, which is still useful for half-educated people.

I would even find syntax quite good, there would then be a linguistic syntax, a logical syntax, etc. Bühler is writing a general linguistic syntax. The connection with that wouldn't be the worst. I fear the 'ideal language' as metaphysical anyway, the word syntax has something mild, human and common about it. Semantics is so Prussian-idealistic, demanding, it is, I think, not even entirely faithful in its meaning.

New words without reminiscences are questionable. For most people, semantics is completely alien, with no association. What kind of neologisms are there? Sociology. Understandable, albeit a mixed word. Behaviorism. Well. Individual psychology – representationally not very faithful. Psychoanalysis, good. Logistics, good. Do you know another

10 Carnap to Neurath, April 28th, 1932. Orig.: "Könntest Du Dich nicht doch mit dem Wort 'Semantik' allmählich befreunden? Es ist übrigens sprachlich doch in Ordnung. Seine Bildung entspricht zwar nicht den sonstigen Wortbildungsregeln aber der Geist der Sprache ist so unberechenbar, wie der Ratschluß Gottes. Aristoteles verwendet schon das Adjektiv 'semantikós'; er sagt, die Aussage sei eine 'phoné semantiké'; ein zeichengebender Laut. Später haben die Griechen ihre Notenschrift 'Semantik' genannt. Das Wort klingt allerdings zunächst etwas fremd und gelehrtenhaft, aber das kann sich doch bald verlieren. 'Arithmetik' würde uns heute ebenso klingen, wenn wir es zum ersten Mal hören würden. Gegenüber 'Syntax' hat Semantik den Vorzug, dass es eindeutiger ist. Wenn man das Wort 'Syntax' irgendwo gebraucht, müsste man immer dabei sagen 'nicht im philologischen Sinn.'"

strange word formation as semantics? And we want to become popular, want our song to be sung!¹¹

On May 25th, 1932, at a meeting in Prague, Neurath and Frank tried to soften Carnap and attempted to make 'syntax' palatable to him,¹² initially without any apparent success.

It is a pity that you use semantics with such preference and leave dear logical syntax aside.¹³

Subsequently, Neurath took note of the fact that he could not prevent Carnap from using 'semantics' and began to retract his concerns about the expression a little bit:

I still don't find 'semantics' pleasing. Logical syntax is more humane. But at least the reason Ina put forward about the formation of adjectives is one reason for it. Thus I can't defend myself against semantics entirely. It's just a bit 'academic; antique; schoolmasterly'. Well.¹⁴

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- 11 Neurath to Carnap, May 10th, 1932, p. 1. Orig.: "Semantik' wird durch diese Mitteilung nicht süffiger. Da die 'Mantik' jedem als Seherkunst bekannt ist, wird der nicht hochgebildete meinen, dass die 'Semantik' eine Abart der Mantik ist. Bitte schreib mir gleich, was Frank dazu meint. Semantisch habe ich Dir ja als gebräuchlich nachgewiesen. Semasiologie gibt es auch – Bedeutungslehre. Ich finde 'logische Syntax' schon reichlich gelehrt. Ich würde mich Deiner Terminologie wahrscheinlich nicht anschließen, sondern versuchen, die 'logische Syntax' durchzusetzen, die doch für Leute mit Halbbildung noch sinnvoll ist. Ich fände Syntax sogar ganz gut, es gäbe dann eine linguistische Syntax, eine logische Syntax usw. Bühler schreibt an einer allgemeinen linguistischen Syntax. Der Zusammenhang damit wäre nicht das schlimmste. Ich fürchte ohnehin die 'ideale Sprache' als Metaphysikum, das Wort Syntax hat so was mildes, menschlich-übliches an sich. Semantik ist so preussisch-idealistisch, fordernd, es ist, glaube ich, nicht einmal ganz sinngetreu. Neue Worte, ohne Anklang sind bedenklich. Für die meisten Menschen ist Semantik ganz fremd, ohne Assoziation. Was gibt es für Neubildungen? Soziologie. Verständlich, wenn auch ein Mischwort. Behaviorismus. Gut. Individualpsychologie – nicht sehr bedeutungstreu. Psychoanalyse, gut. Logistik, gut. Kennst Du noch eine so fremde Wortbildung wie Semantik? Und dabei wollen wir populär werden, wollen dass unser Lied gesungen wird!"
- 12 Cf. Carnap's diary entry from May 25th, 1932: "Frank suggests the title 'Logical Syntax of Language', Neurath: without 'der Sprache'; Subtitle 'Semantics'", orig.: "Frank schlägt als Buchtitel vor 'Logische Syntax der Sprache', Neurath: ohne 'der Sprache'; Untertitel 'Semantik'".
- 13 Neurath to Carnap, July 27th, 1932, p. 4, orig.: "Schade, daß Du mit solcher Vorliebe die Semantik verwendest und die liebe logische Syntax beiseite läßt."
- 14 Neurath to Carnap, September 23rd, 1932. Orig.: "Die 'Semantik' finde ich noch immer nicht glücklich. Logische Syntax ist menschlicher. Aber immerhin, der von Ina auch

But he still defends ‘syntax’ with everything that comes up:

Bühler explained to me in detail why ‘semantics’ is completely wrong. It means the doctrine of the seer signs, the interpretation of the seer signs. I think it should be called semeiology or the like. Hopefully you will change the term.¹⁵

Carnap was not particularly impressed with this, he replied: “Bühler: Semantics on prophetic signs? He probably confused that with mantics!”¹⁶ So even the testimony of Karl Bühler did not prove as fruitful. At least Carnap at the time renounced the expression ‘philosophy’, which Neurath fought even more vehemently albeit not really consistently.¹⁷ Neurath acknowledged this – partly with praise, partly with gritted teeth:

I am very, very glad that you finally threw out this disgusting ‘philosophy’ and didn’t label the uptight creature we begot with this dirty name of a disgusting old person who might have had her merits when she was young. ‘Semantics’ is noted wailingly.¹⁸

The debate only seems to take a turn when Neurath himself developed plans to publish a series of publications under the title “Einheitswissenschaft”, for which he would like to win over Carnap’s book:

So the Semantics in the collection. It’s a shame, it has such a bad name.¹⁹

vorgebrachte Grund von der Adjektivbildung ist ein Grund dafür. Ich kann mich gegen Semantik nicht so voll wehren. Es ist halt etwas ‘akademisch; antik; lehrerhaft’. Na ja.”

- 15 Neurath to Carnap, October 22nd, 1932, p. 3. Orig.: “Bühler hat mir ausführlich erklärt, warum ‘Semantik’ ganz falsch ist. Es bedeutet die Lehre von den Seherzeichen, die Deutung der Seherzeichen. Es müßte, glaube ich, Semeiologie oder sowie heißen. Hoffentlich änderst Du noch den Terminus.”
- 16 Carnap to Neurath, October 24th, 1932. Orig.: “Bühler: Semantik über Seherzeichen? Das hat er wohl mit Mantik verwechselt!”
- 17 Cf. Carnap, diary entry from September 27th, 1932: “Semantics V worked through; at Neurath’s suggestion ‘theory of science’ instead of ‘philosophy’”. Orig.: “Semantik V durchgearbeitet; auf Neuraths Anregung anstatt ‘Philosophie’ ‘Wissenschaftslehre’”.
- 18 Neurath to Carnap, October 1st, 1932, p. 1f. Orig.: “Ich bin sehr, sehr froh, daß Du endlich diese eklige ‘Philosophie’ hinausgeschmissen hast und nicht dem strammen Lebewesen, das wir gezeugt haben, diesen verdreckten Namen einer eklen alten Person gegeben hast, die ja, als sie jung war, ihre Meriten gehabt haben mag. ‘Semantik’ wird heulend zur Kenntnis genommen.”
- 19 Neurath to Carnap, November 3rd, 1932, p. 5. Orig.: “Also die Semantik in der Sammlung. Schade, daß sie so übel heißt.”

The final decision about the name of the book is likely to have been made sometime around the turn of the year 1932/33; the details of this cannot be found in the correspondence, in Carnap's diaries nor the autobiography. In any case, in November 1932 Neurath mentioned Carnap's "Logische Syntax der wissenschaftlichen Sprache" as the possible 4th volume in a list of the planned first titles of the series *Einheitswissenschaft*.²⁰ Eventually, the *Logische Syntax der Sprache* did not appear in this series, curated by Neurath together with Carnap, Frank and Hahn, but as Volume 8 of *Schriften zur wissenschaftlichen Weltauffassung* edited by Frank and Schlick. In exchange Neurath received *Die Aufgabe der Wissenschaftslogik* as the 3rd issue of the *Einheitswissenschaft* Series.²¹ Both titles were published in 1934. In his diaries until autumn 1932, Carnap consistently used the expressions 'semantics' or 'sem.' when recording his activities. Then, in fall, there is a break due to illness, among other things, but before his illness Carnap had meetings with Neurath and others in Vienna during the second week of November. And very likely it must have been these meetings, where Carnap may have sealed the name change.²² From February 1933 onwards, Carnap only recorded work on the 'syntax', beginning with the entry of February 3rd: "Holidays! After 2 months finally working on MS Syntax again. Revision of the typed MS started".²³

As a result, Carnap himself asked Neurath in mid-1933 whether he could arrange a lecture for him on "Questions of syntax in scientific language" in the Verein Ernst Mach.²⁴ Neurath takes notice of this with delight and irony and writes with a teasing undertone:

I'm looking forward to your logical syntax, called semantics; I dread that you burden your mouth or your pen with the disgusting words exact philosophy, scientific philosophy and the like!²⁵

20 Cf. Neurath to Carnap, November 22nd, 1932.

21 The main reason for this rearrangement was the length of *Logical Syntax* which exceeded the intended extent of the *Einheitswissenschaft* volumes by far.

22 Cf. Carnap, diary entries from December 10th–19th, 1932.

23 Cf. Carnap, diary entry, Feb. 3rd, 1933. Orig.: "Ferien! Nach 2 Monaten endlich wieder MS Syntax gearbeitet. Umarbeitung des fertig getippten MS begonnen". On the change of the name see also Tuboly 2017, pp. 65f.

24 Cf. Carnap to Neurath, June 9th, 1933.

25 Neurath to Carnap, June 18th, 1933. Orig. "Ich freue mich auf Deine logische Syntax, Semantik genannt, daß Du das ekelhafte Wort exakte Philosophie, wissenschaftliche Philosophie und so ähnlich überhaupt in den Mund und in die Feder nimmst, graust mir!"

Thereupon semantics is not mentioned at all in the correspondence during the following two years. Carnap reported to Neurath at the end of 1933 that he had completely reworked and finally finished his MS “Logische Syntax der Sprache” and had sent it to Springer on December 14th.²⁶ And Neurath is obviously satisfied with the change of the name.

This provisional end to the debate about semantics marks the only superficial compromise: Neurath enforced the abandonment of the term ‘semantics’, but regarding content, form and methods, Carnap does in his book exactly what he intended to do from the beginning. Much more than the name of the book this should have attracted Neurath’s very displeasure. Carnap introduced a theory of several languages or several layers of language, whereas Neurath did not want at this time – and *cum grano salis* at any time – to get involved in more than one universal scientific language, a single universal jargon: Rather, whatever we want to say about sentences or languages, according to Neurath, must remain expressible within this universal jargon.²⁷ Obviously, the last paragraphs of the introduction to *Logical Syntax*, where Carnap explains his terminology and almost literally reproduces what Neurath told him about Bühler, are to be understood as an answer addressed to Neurath.²⁸ There he sketches the meanings and connections of some of the new terms, e.g. ‘syntax’, ‘metalogs’, ‘semantics’, ‘sematology’ etc., but the passages above leave no doubt that what he is going to do will be something meta-logical, meta-mathematical or meta-linguistical.

2 Truth or No Truth in Scientific Language

The actual beginning of Carnap’s preoccupation with semantics was marked by his encounter with Alfred Tarski, who visited Vienna in February 1930 and with whom he met repeatedly for discussions. Carnap, who had already started thinking about a metalogic in 1929 and was currently venturing a “branched type theory”,²⁹ was strongly inspired by Tarski and his remarks on meta-mathematics. He also reported on this in the Schlick Circle, initially without any real echo:

26 See Carnap to Neurath, December 18th, 1933.

27 See e.g. 3 years later Neurath 1936b, p. 697.

28 Cf. Carnap 1934, p. 9, cf. also p. 191f.

29 See Carnap, diary entries from December 29th, 1929 and February 11th, 1930.

8 [o'clock] Circle: Me on Tarski and the meaning of metamathematics. I only wanted 5 min[utes], then I speak for half an hour because some, especially Schlick, underestimate its importance. I said that we must also make the explanatory language exact and symbolize it, but have not done so until now, because we always have a bad conscience, because of Wittgenstein.³⁰

This passage very well documents the starting point for further developments within the Vienna Circle. Here, at a first stage, Wittgenstein is dominant, with his thesis, presented in the *Tractatus*, that it is not possible to speak about language within language. For Wittgenstein language about language is inadmissible, this holds e.g. for the sentences of his own *Tractatus*, they only serve as a nonsensical crutch, which, as indicated in the famous ladder-metaphor, somehow help us to gain insight without having any meaning on their own. But we also have nothing besides language with which we could scientifically investigate language (“The limits of my language mean the limits of my world”),³¹ thus inadequacies and paradoxes are pre-programmed. This, of course, is an unsatisfactory situation and Carnap’s idea is more or less obvious, namely to develop a precisely formalised system-language as a supplement to ordinary language, to which the ordinary language (and of course this only means a cleaned subset of it) can be translated, but which, due to its formal structure, can avoid the shortcomings of the former. It should allow – from top to bottom, as it were – to analyse and solve problems, to clarify, and the like. Earlier models for this are, among others, the type-theoretical approach in Russell and Whitehead, Hilbert’s axiomatic calculus for mathematics and Tarski’s metamathematics. So, on the one hand, we have the option of Wittgenstein’s silence – which, for both Carnap and Neurath, was not an option at all – respectively, making the best of it, trying to make speakable though as much as possible, with an impending restriction to a considerable area of some remaining unspeakable. This was attempted by Schlick, Gödel and others. On the other hand, one could introduce a Tarskian hierarchy of two or more languages or levels of language,

30 Carnap, diary entry from February 27th, 1930, cf. also the entries from February 16th–26th. Orig.: “8 [Uhr] Zirkel: Ich über Tarski und Bedeutung der Metamathematik. Ich wollte nur 5 Min., spreche dann ½ Stunde, weil einige, besonders Schlick, die Bedeutung unterschätzen. Ich sagte, wir müssen auch die Erläuterungssprache exakt machen und symbolisieren, haben das bisher unterlassen, weil stets mit schlechtem Gewissen, infolge Wittgenstein.”

31 Wittgenstein, TLP, 6.54 and TLP 5.6.

that is metalanguage(s), which was later favored by Chwistek and Carnap and noted by Russell in his preface to the *Tractatus*.

Neurath was not happy with either option and blamed them for metaphysics or likewise metaphysical tendencies. It soon became clear to both of them that Neurath was also in opposition to his friend Carnap. So he wrote to Carnap in mid-1932:

My opinion is that there is a profound difference between our basic attitudes, which is hardly noticeable only because we are creating growing areas of conformity in the realm of scientific precision. But with you, at least it seems to me, there always remains a clear remnant of an intense, idealistic basic attitude that you are not even become aware of.³²

Even in a letter before that, Carnap had tried to specify where their opinions were diverging and – here still in the context of the protocol sentence debate and long before the appearance of *Logical Syntax* – brought Neurath's concerns to the point as follows:

But apart from the details, I think I have understood the main ideas more or less correctly. Is it correct that the following are the two main theses?: 1) We do not want to use 2 forms of language (protocol-language and system-language), but only *one*; indeed the protocol-sentences are different from the other sentences (?), but still belong to the same language form as the others. 2) A protocol-sentence can be confirmed or refuted by other protocol-sentences just like any other sentence; that is, starting from a certain set of protocol-sentences, we can eventually arrive at a sentence which is the negation of one of the original sentences. The starting sentences do not need to be protocol-sentences of only one subject (?).³³

32 Neurath to Carnap, July 27th, 1932, p. 1. Orig.: "Meine Meinung ist, daß zwischen unseren Grundeinstellungen eine tiefgehende Differenz besteht, die nur dadurch wenig zu bemerken ist, weil im Bereich der wissenschaftlichen Präzisierung wir wachsende Gebiete der Gemeinschaft erzeugen. Aber bei Dir, so scheint mir wenigstens, bleibt immer ein deutlicher Rest intensiver idealistischer Grundhaltung bestehen, der Dir gar nicht zum Bewußtsein kommt."

33 Carnap to Neurath, July 18th, 1932, p. 2. Orig.: "Ich glaube aber, abgesehen von den Einzelheiten, doch die Hauptgedanken ungefähr richtig verstanden zu haben. Stimmt es, dass Folgendes die beiden Hauptthesen sind?: 1) Wir wollen nicht 2 Sprachformen (Prot.Spr. und Systemspr.) machen, sondern nur éine; die Prot.Sätze sind zwar von den andern Sätzen verschieden (?), gehören aber doch derselben Sprachform an wie die übrigen. 2) Ein Prot.Satz kann ebenso durch andere Prot.Sätze bestätigt oder widerlegt

This is exactly what Neurath then wrote in his essay “Protokollsätze”, where he calls for a single universal jargon for unified science, which contains tautologies and real sentences, the real sentences in turn being divided into protocol-sentences and non-protocol-sentences, but both of which are “of the same linguistic form as the other real sentences”.³⁴ This means that the meta-language has to be the same language as the object-language – that is meta-language is reduced to object-language in the end. Truth and falsehood only occur on this level, i.e., actually, they do not occur at all: in the case of truth, sentences are distinguished by their being element of the set of accepted sentences in universal jargon, but that – here we should directly refer to Duhem – always provisional and without any guarantees. Negativity, on the other hand, is reduced to the existence of contradictions between sentences requiring a decision, namely that at least one of the sentences has to be rejected and eliminated from the set, or that the entire system as such is to be rebuilt. Rejected sentences would then, in a certain sense, be considered incorrect. Neurath illustrates this at the end of his article with the picture of a machine that can be fed with sentences, strictly speaking protocol-sentences, by any person and signals when a contradiction occurs.³⁵

Neurath and Carnap discussed the connections between ‘wrong’, ‘contradictory’ and ‘pointless’, among other things, before the publication of the “Protokollsätze”. Sounds like Heidegger’s ‘Nichten’ are not words at all and cannot be inserted into the language. Sentence-like formations including those sounds are consequently not to be considered as sentences – both of them emphasise this. On the other hand, contradictions are logically wrong for Carnap, while Neurath sees them rather as “logically senseless” and what Carnap calls “empirically wrong” actually has no place in Neurath and is temporarily called “half contradiction”.³⁶ Carnap also entertains a certain connection between sentences contrary to syntax and contradictory sentences, in particular, that both are to be excluded, but emphasizes that they naturally have different characters.³⁷ An empirically incorrect proposition as such is for Carnap

werden, wie irgend ein anderer Satz; d.h. wir können, von einer bestimmtem Menge von Prot.Sätzen ausgehend, unter Umständen schliesslich zu einem Satz gelangen, der die Negation eines der Ausgangssätze ist. Die Ausgangssätze brauchen nicht Prot.Sätze nur eines Subjekts zu sein (?).”

34 Neurath 1932/33, p. 579.

35 See *ibid.*, p. 584.

36 Cf. Neurath to Carnap, November 3rd, 1932, p. 4f.

37 See Carnap to Neurath, October 31st, 1932.

logically sound, it is only rejected with regard to the other propositions; on the other hand, a contradictory proposition is rejected by itself, for logical [...] reasons, without having to consider other propositions.³⁸

Hence, with Carnap, we have in logic meaningless, contradictory and wrong sentences as well-established components expressing negativity, whereas Neurath only knows meaningless and unaccepted sentences for which contradictions function as the indicator. We thus have to take notice of a quite early and profound divergence with regard to the central semantic concepts of the pair truth-falsehood.

The fact that Neurath did not attack the *Logical Syntax* more violently under these conditions is probably due to the further concession that Carnap, in addition to dispensing with the name 'semantics', also refrained from using the predicate 'true':

'True' and 'false' are not genuine syntactic concepts. In general, one cannot tell from the formal properties of a sentence alone whether it is true or false.³⁹

That could already trigger Neurath's anti-metaphysics alarm in that it could easily be read as a reference to something extra-linguistic. Carnap prudently does not operate with truth at any point in the *Syntax* and, thus, seems to have provided Neurath – at least for some time – with the building-plans for precisely the machine mentioned above, which manages our real sentences and sounds the alarm in the case of contradictions.

However, there is a problem in the construction of *Logical Syntax* that Neurath might have missed at least initially. If you want to introduce the terms 'true' and 'false', you can only do this syntactically in logical languages, provided that 'true' and 'false' coincide with 'analytical' and 'contradictory'. In other languages they have to be formed with substituting terms, e.g. with 'analytical in S', but this is not possible without contradictions if the language is to contain its own syntax. Correspondingly, in the two languages of *Logical*

38 Carnap to Neurath, November 4th, 1932. Orig: "[Ein empirisch falscher Satz ist] logisch einwandfrei, er wird nur verworfen mit Rücksicht auf die andern Sätze; dagegen wird ein kontradiktorischer Satz schon für sich allein betrachtet verworfen, aus logischen [...] Gründen, ohne daß man die andern Sätze berücksichtigen müßte."

39 Carnap 1934, p. 164. Orig: "[W]ahr' und 'falsch' sind keine echten syntaktischen Begriffe. Aus den Formeigenschaften eines Satzes allein ist ja im allgemeinen nicht zu ersehen, ob er wahr oder falsch ist."

Syntax, it is only possible to define ‘analytical in I’ or ‘provable in I’ with the richer language II, and for ‘analytical in II’ a richer language III is required,⁴⁰ and so on. Therefore, we have to assume metalanguages or even hierarchies of metalanguages in *Logical Syntax* if one wants to achieve certain results such as completeness, coherence or provability while maintaining consistency. Carnap sums it up succinctly: “Mathematics requires an infinite number of increasingly rich languages.”⁴¹ – The same holds for logic, without a doubt.⁴²

If this result is transferred to other areas of science, then it is obvious of course that these, should they be formalised – and such endeavours were entirely in line with the Vienna Circle – also will be in need of metalanguages. (I only refer to Joseph Henry Woodger with whom Carnap and Neurath were in contact since 1934, and who worked on an axiomatic biology, published *The Axiomatic Method in Biology* together with Tarski in 1937 and was an important contributor to the *International Encyclopedia of Unified Science*).

It is therefore not very surprising that the debate about semantics and the semantic concept of truth revived relatively soon, in the context of the Congresses in Prague (1934) and in Paris (1935).⁴³

Subsequently Neurath has apparently somehow come to terms with metalanguages, so he wrote, historically recapitulating to Carnap in 1936:

There was a difference between those who declared ‘language about language’ permissible and those who did not allow language about language (Wittgenstein) but engaged in comparing e.g. the complexity of ‘reality’ and ‘language’. I remember that I turned against this point of view very early and rejected any ‘pre-legitimate’ discussion about language and reality, so to speak. I believe that for Hahn and also for you, under the influence of type theory, the opinion was not so implausible that language about language is legitimate. All this was clear and decidedly determined before the acquaintance with the Warsaw-people.

40 See *ibid.* p. 164f., see also Carnap’s remarks on quasi-syntactic sentences, p. 179ff.

41 *Ibid.*, p. 165. Orig.: “Die Mathematik erfordert eine unendliche Reihe immer reicherer Sprachen.”

42 Among others, Adam Tuboly has already pointed to the fact, that semantical traits are to be found in *Logical Syntax*, cf. Tuboly 2017, p. 67ff.

43 Cf. in detail Woleński 2018, with a concise listing of the controversial positions, p. 208; or Mancosu 2008. And Neurath himself, e.g. Neurath 1936a, p. 706f., with mild potshots against Carnap, Tarski et al. when he diagnoses a “lurking danger [...] of lapsing into metaphysics” in connection with truth, correspondence and sentences about sentences, though strictly avoiding going into details. Carnap later on reports only some misunderstandings by Neurath, Naess and others, see Carnap 1963, p. 95ff.

The Warsaw people were, so to speak, an example of the fact that this can be done with the help of METALANGUAGE. They are protesting against 'the' language. Therefore, they have reservations about standardised science and its standardised language.⁴⁴

And a short time later a little bit more precisely:

[U]nder the influence of Gödel the opinion arose that one could speak about one part of the language with another one, as I explicated in the SCIENTIA. What the Warsaw-people developed is the series of METALANGUAGES. [...] Different things do cross each other here. One is the thesis that one can legitimately talk about language with language, just like about other things. I would consider that to be pre-Warsaw. And then the thesis that one should carefully separate the LANGUAGES. That probably also influenced your *Logical Syntax*. But I think that you have supported the legitimacy of the discussion about language similarly to the way I did before.⁴⁵

Here the thrust of Neurath's line of argument can already be seen: he obviously wants to go to a point earlier than the influence of Tarski and the group in Warsaw. He only wants to tolerate metalanguages as an isolated phenomenon, for example in questions about the theoretical foundations of mathematics.

44 Neurath to Carnap, April 30th, 1936, p. 2. Orig.: "Es war eine Differenz zwischen denen, die 'Sprache über Sprache' zulässig erklärten, und die, welche Sprache über Sprache nicht erlaubten (Wittgenstein) dabei aber z. B. Komplexität von 'Wirklichkeit' und 'Sprache' verglichen. Ich erinnere mich, daß ich sehr früh mich gegen diesen Standpunkt wandte und jede sozusagen 'vorlegitime' Diskussion über Sprache und Wirklichkeit ablehnte. Ich glaube, daß für Hahn und auch für Dich unter Einfluß der Typentheorie die Meinung nicht so fern lag, daß Sprache über Sprache legitim sei. Dies alles war doch deutlich und entschieden festgelegt vor der Bekanntschaft mit den Warschauern. Die Warschauer waren sozusagen das Exempel dafür, daß man das mithilfe der METASPRACHE machen kann. Sie protestieren ja gegen 'die' Sprache. Daher haben sie Bedenken gegen die Einheitswissenschaft und ihre Einheitsprache."

45 Neurath to Carnap, July 1st, 1936, p. 3f. Orig.: "[U]nter dem Einfluß Gödels entstand wohl die Meinung, daß man mit einem Teil der Sprache über den anderen sprechen könne, wie ichs in der SCIENTIA ausführte. Was die Warschauer entwickelten ist die Serie der METASPRACHEN. [...] Es durchkreuzen sich da verschiedene Dinge. Das eine ist die These, daß man legitim mit der Sprache über die Sprache reden könne, wie über andere Dinge. Das würde ich für vor-warschauerisch halten und dann die These, daß man sorgsam die SPRACHEN trennen müßte. Das hat dann wohl auch Deine logische Syntax beeinflusst. Aber ich meine, daß Du die Legitimität der Diskussion über die Sprache ähnlich wie ich schon vorher vertreten hast."

This endeavour is well documented with unpublished material of both, especially in the context of the Paris Conference in 1937. Carnap launched the debate suggesting a discussion

1) on semantic concepts ('true' and others). 2) about probability, degree of confirmation and many-valued logic [...] but both are probably better only in a smaller circle of about 15 people, not publicly (in order not to make public the differences in opinion (1) with you and (2) with Reichenb[ach] concerning these questions).⁴⁶

For this purpose, Carnap and Neurath formulated papers, these were exchanged and later on discussed; both of them have been preserved.

As already mentioned, Neurath took a pre-Warsaw point of view, on the one hand he legitimised speaking about language: since sentences are usually related to objects and objects can be anything, it is equally possible to utter sentences about non-sentences (things, facts) as to formulate sentences about sentences. In doing so, he provides a version of Mach's theory of elements and complexes developed in the "Antimetaphysische Vorbemerkungen" to *Die Analyse der Empfindungen*⁴⁷ that is turned into language. Instead of Mach's *elementa* – disregarding obvious differences as the ontological characterisation of *elementa*, e.g. – we have propositions or sentences, all on a par. These sentences can be contrasted with one another, compared with one another, etc., which is the real everyday business of the logic of science. On the other hand, Neurath avoids the use of a metalanguage in this way: it does not matter how high or low the degree of complexity of the objects which our sentences are about happens to be – when only sentences are considered. Neurath calls this a "trick" and it should make it possible to eliminate questions about the relationship between language and reality, simply by not comparing sentences and facts, but only sentences, namely sentences about non-sentences and sentences about sentences. Thus the things, the facts, that is, the imminent metaphysical reference to reality, disappear, according to Neurath, from consideration.⁴⁸

46 Carnap to Neurath, February 14th, 1937, p. 1. Orig.: "[Zwei Probleme sind ausführlich zu diskutieren,] beide aber wohl besser nur im engeren Kreise von 15 Leuten, nicht öffentlich (um die in diesen Fragen bestehenden Meinungsverschiedenheiten (1) mit Dir und (2) mit Reichenb[ach] nicht in die Öffentlichkeit zu bringen): 1) über die semantischen Begriffe ("wahr" u. a.); 2) über Wahrscheinlichkeit, Bestätigungsgrad u. mehrwertige Logik."

47 Cf. Mach 1922, p. 10ff.

48 See Neurath 1937a, p. 2f.

Applying this trick Neurath now proposes a paraphrase of Tarski's sentence (4) for the definition of truth:⁴⁹

If I am given some encyclopedia-sentence: Here is an expression that is formed with the following letters [I, T I, S S, N, O, W, I, N, G] [...], then this expression should be called "true statement", if and only if, I have been given a second encyclopedia-sentence: It is snowing.⁵⁰

Neurath now uses the term 'encyclopedia-sentence' to refer to what he called "akzeptierter Realsatz" (accepted real-sentence) a few years earlier. Truth is thus assigned to expressions that represent or designate sentences, and truth is present if there are at least two sentences accepted in universal jargon. One of these expresses the existence of such a designating expression in some suitable form of physical representatives, and the other is the sentence named exactly in this way. So Neurath interprets the sentence "p' is true" as equivalent to the conjunction of the encyclopedia-sentences "'p' occurs in the place *xyzt*" and "p". For Neurath, this construction has the advantage that it remains within the framework of the logic of science, i.e., it does not require a metalanguage. And at the same time it maintains the naming-relation by packing the relation between designans and designatum into the realm of the objects or the content of sentences: So truth becomes a matter of sentences only; meaning, referring, the relation language-world are matters of the content of sentences and any metaphysical correspondence of language and reality should be avoided. At this point, Neurath cannot refrain from taking a swipe at Carnap here when he notes that he considers "relations between expressions of language and designated objects not to be entirely harmless",⁵¹ with explicit reference to Carnap's documents for his research seminar in 1937/38. Neurath then suggests trying out his construction and seeing how far one could go with it. And by the way he indulges in violent attacks against Tarski and Lutman, whom he accuses of metaphysics and claims to absoluteness and repeats his often-recounted metaphysical genealogy via Kotarbiński (who atypically is not mentioned at this point), Twardowski, Brentano and Aquinas tracing back to Aristotle. At least, Neurath is ready for a concession to the extent that he emphasizes that his

49 See Tarski 1935, p. 270.

50 Neurath 1937a, p. 3f. Orig: "Wenn mir der Enzyklopädiesatz gegeben ist: Hier steht ein Ausdruck, der aus folgenden Buchstaben [E, S S, C, H, N, E, I, T] gebildet ist [...] dann soll dieser Ausdruck 'wahre Aussage' heissen, dann und nur dann, wenn mir ein zweiter Enzyklopädiesatz gegeben ist: Es schneit."

51 Ibid., p. 4.

construction is only a proposal for discussion. Its viability is a matter of practical testing, it should be evaluated in terms of feasibility and usefulness strictly within scientific logic and thus be limited to the syntactical level.⁵²

In his answer to Neurath, Carnap first sharpened Neurath's remarks to the "main question" of whether "the semantic terms can always be eliminated", namely "by translating semantic into non-semantic sentences".⁵³ At this point, one can ask whether such a "sharpening" is fair to Neurath or is simply an exaggeration, as Neurath does not at all claim general eliminability and moreover, as for example in 1932, actually does not have the term 'true', but only 'accepted sentence'. Unsurprisingly, Carnap's answer is flatly negative:

This question must be answered in the negative. There are semantic sentences for which neither a translation into syntactic language nor into object language is possible. (In fact, this is the general case, while the translatable sentences are certain special cases, although perhaps most of the semantic sentences that appear in our books and articles belong to these special types of translatable sentences.)⁵⁴

Having said that, Carnap starts a more detailed analysis of Neurath's "Trick der Wissenschaftslogik", which in his terminology is "the translation of an (not essentially) semantic sentence into syntactic language", in many cases this should be possible as well.⁵⁵ With essentially and non-essentially semantic sentences Carnap takes up the distinction between syntactic and quasi-syntactic sentences or concepts from the *Logical Syntax* in reverse order.⁵⁶ In his earlier view, terms like "true" were not purely syntactic because they could not simply be defined in the syntax. Now – precisely for this reason – they are essentially semantic, while not essentially semantic terms are those that can be defined solely on the basis of syntactic language. With this tool Carnap tries to demonstrate that Neurath's trick is not viable in the case of 'true', since the

52 See *ibid.*, p. 9f.

53 Carnap 1937, p. 3.

54 *Ibid.*, p. 3f. Orig.: "Diese Frage muss verneint werden. Es gibt semantische Sätze, für die weder eine Uebersetzung in die syntaktische Sprache noch in die Objektsprache möglich ist (Dies ist sogar der allgemeine Fall, während die übersetzbaren Sätze gewisse Spezialfälle darstellen, wenn auch vielleicht die meisten der semantischen Sätze, die in unseren Büchern und Aufsätzen vorkommen, zu diesen speziellen Arten der übersetzbaren Sätze gehören.)"

55 *Ibid.*, p. 4. Orig.: "[Neurath's] 'Trick der Wissenschaftslogik' [ist] die Uebersetzung eines (unwesentlich-) semantischen Satzes in die syntaktische Sprache".

56 See above, notes 37 and 38, Carnap 1934, pp. 164f. and 179ff.

translation of 'true' into 'encyclopedia-sentence' (or 'accepted', i.e. 'accepted encyclopedia-sentence'), as Neurath would have to do, does not work properly for Carnap, even if 'true' and 'accepted' are often regarded as synonymous by others. He justifies this with the remark that expressions such as 'encyclopedia', 'believed', '(scientifically) recognised' or 'accepted' always require the specification to a person or a group of persons and at least a chronological location. And so it is immediately clear that, according to Carnap, these are terms of the behavioristics of science, therefore empirical, and that corresponding sentences can be syntactically analysed. In the case of 'true', on the other hand, details about some person and time never are necessary and not even possible⁵⁷ – why so, however, he leaves unfounded. In any case, with Carnap, 'true' is an essentially semantic term, while 'encyclopedia-sentence' etc. are not essentially semantic. In the following, he tries to clarify his position with examples, initially with the sentence (1) "Goethe died in Weimar in 1832' is true". In terms of their common understanding, this can be translated into the sentence of the object-language (2) "Goethe died in Weimar in 1832". However, the person-related and time-related object-language sentence (3) "Goethe died in Weimar in 1832' has been a sentence in the encyclopedia of Japanese historians since 1873" is not a correct translation. From Carnap's point of view, Neurath's syntactic trick does not produce satisfactory results in the case of the translation of 'true', because the sentences obviously have different meanings – (2) and (3) are completely different sentences in object-language. Neurath could counter this by saying ad hoc that such a postulated relation to person and time of 'encyclopedia-sentence' etc. could also be understood very broadly, in the sense of 'encyclopedia-sentence from any person at any time' (which he does not in fact), whereby the result could be extensionally the same. Thereupon Carnap again could object that it might be so, but that the meanings of the two translations are nonetheless very different.

In order to strengthen his position, Carnap gives further examples, each with the same result. His counter-trick is that by translating the semantic (system-language) sentence into one of the object-languages, the expression "true" is effectively truncated. This corresponds to Neurath's basic attitude as well as to Carnap and Tarski, who all accept that to say "p' is true" is nothing else than to say "p".⁵⁸ This allows Carnap to get from object-language "p" to "p' is true" with the very same meaning in syntax-language. In presenting Neurath's views, however, Carnap holds that Neurath has to add something

57 See Carnap 1937, p. 4.

58 Explicitly stated e. g. *ibid.*, p. 1.

to the “p” in order to be able to express “p’ is true” in his sole object-language, namely something like “is accepted”: Neurath thus does not arrive at “p” but at “p’ is accepted”. This indicates that the meanings of the respective translations are different and if the meanings of the two translations are different, then, if “p” is supposed to keep its meaning, the two concepts of truth must also be different.⁵⁹ Hence, Carnap concludes that Neurath’s procedure fails at least for the term ‘true’ and consequently, among others, for the term ‘to know’, which Carnap regards to be essentially semantic too, since in its classical wording as true belief relying on good reasons, it contains the term ‘true’ as an indispensable condition in the definition.

In response to this we may fall back on three documents by Neurath in which he as well replied to Carnap as he made concessions too. First, he wholeheartedly recognised the importance and fertility of semantics for “calculus languages” and emphasised that he has no suspicion of the formal correctness of the proposed calculi,⁶⁰ but at the same time warned of a dangerous tendency to abandon empiricism. He agreed with the view that sentences of semantics cannot always be translated into syntactic sentences or into real-sentences in the manner outlined by Carnap.⁶¹ But in all three texts Neurath questions the practical importance of the concept of truth in everyday life and in science and even claims the possibility of “writing an encyclopedia of the total knowledge without having to include the term ‘true’ or a related term”.⁶² He points out that in his paraphrase of Tarski’s sentence (4) there are two encyclopedia-sentences, of which “entirely in the sense of the semantic suggestions, one encyclopedia-sentence is called the carrier of a true expression if a second encyclopedia-sentence is accepted”, and he emphasizes that the term ‘encyclopedia-sentence’ is added twice, so to speak, for both sentences and not, as Carnap claims, the expression ‘true’ is translated into the expression ‘encyclopedia-sentence’ or ‘accepted’.⁶³ Neurath therefore explicitly refuses to identify Carnap’s term ‘true’ with his term ‘accepted’, which is in line with his basic negative attitude towards ‘true’ that occupies a prominent place on his *index verborum prohibitorum*, is deemed to be dispensable and

59 See *ibid.*, p. 5.

60 See Neurath 1937c, p. 1; or later, pretty analogously, in a letter, Neurath to Carnap, September 25th, 1943, p. 13.

61 See Neurath 1937b, p. 1.

62 *Ibid.*, p. 3.

63 See *ibid.*, p. 2. Orig.: “ganz im Sinne der semantischen Vorschläge [wird] ein Enzyklopädiesatz als Träger eines wahren Ausdrucks bezeichnet, wenn ein zweiter Enzyklopädiesatz anerkannt wird”. Neurath also emphatically repeats this in Neurath 1937d, a kind of letter of complaint to Carnap, p. 1.

accordingly avoided by him in contrast to the harmless ‘accepted’. And when Neurath wrote:

I once said that ‘is true’ could be replaced by: ‘is an encyclopedia-sentence that we accept’. I still think so today.⁶⁴

then this is only to be understood in such a way that in practical usage the expression ‘true’ should be dispensed with in favour of ‘accepted’ and not, as Carnap puts it, ‘true’ and ‘accepted’ are synonymous and sentences with them are equivalent or equipollent.

3 Conclusion

Concluding, it is quite clear that the views of the two now, 1937 and later, just as in 1932, differ deeply from one another and are basically incompatible. In contrast to Wittgenstein both welcome speaking about language and stress the importance of it. But Carnap additionally wants the concept ‘true’ and wants metalanguages in order to deal with object languages. He understands truth as a matter of logic only. Whereas Neurath does not want such a concept of truth – on the one hand, because he always senses a claim of absoluteness in it, on the other hand, because he esteems this meagre logical version of truth of severely limited use in science. His alternative, acceptance, rather is a pragmatical matter within epistemics or philosophy of science and bears a distinct emphasis on social sciences. Carnap again does not invalidate such an insinuated absoluteness-claim in any way but obviously strengthens it by the decided rejection of person-relatedness and time-relatedness. Neurath in contrast holds the tentativeness and replaceability of encyclopedia-sentences accepted at some time *t* an extremely important point. And Neurath wants the standardised science to be implemented as far as possible in one single universal jargon without superordinate hierarchies of metalanguages, with the exception of special cases such as the foundation of mathematics in order to avoid paradoxes. Neither of them changed their views; Carnap kept working hard on semantics and Neurath affirmed his opinion regarding the dispensability of the concept of truth and metalanguages in some publications.⁶⁵ These motifs

64 Neurath 1937d, p. 1. Orig.: “Ich habe früher einmal gesagt, dass man ‘ist wahr’ ersetzen könne durch: ‘ist ein von uns anerkannter Enzyklopädiesatz.’ Das meine ich auch heute noch.”

65 See e.g. Neurath 1941a and Neurath 1941b.

popped up again in their discussions about Popper and especially on Russell's *Meaning and Truth*⁶⁶ and they also played an important role in the big row⁶⁷ between the two of them in 1944.

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66 The reproach of metaphysics – precisely “Aristotelian metaphysics” – Neurath brought forward against Russell can easily be understood as addressed against Carnap too. Neurath challenges Carnap in a series of letters, cf. e.g. Neurath to Carnap, July 17th, 1942, p. 3ff.; January 15th, 1943, p. 1f.; March 15th, 1943, enclosure, final paragraphs; September 25th, 1943, pp. 11–18. Whereas Carnap only answered late and somehow reluctantly and in the end annoyed, i.e., that he was not happy with defending Russell's positions against Neurath's, positions he himself would not really agree with on the whole. Cf. Carnap to Neurath, November 7th, 1942, p. 2; February 4th, 1944, pp. 1–5.

67 In the end neither of them changed their opinion significantly. In his posthumously published “After Six Years” Neurath repeated the accusation of Aristotelian absolutism directed against Carnap and Tarski and called Nagel and Strauss as witnesses (cf. Neurath 1946, p. 314). While Carnap speaks in retrospect about misunderstandings of his critics without showing any particular concessions (see Carnap 1963, p. 95f.).

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PART 3

Contexts and Relations



Truth in Science and Everyday Life: Bertrand Russell's Theories of Meaning and Truth as Controversial Issues between Carnap and Neurath

Hans-Joachim Dahms

Abstract

The relationship and collaboration between Neurath and Carnap worked very well for a decade (between 1925 and 1935): they were the leading authors of the famous Manifesto of the Vienna Circle and began to reformulate the logical empiricist gospel of the group along physicalistic lines. The first Congress of Unified Science in Paris in 1935 marked a turning point: on the one hand, the International Encyclopedia of Unified Science got underway; on the other hand, a dispute over the new discipline of semantics broke out between Carnap and Neurath and their respective followers, which would cast a shadow over their cooperation and friendship for the rest of their lives.

The dispute at the Paris Congress of Empiricists in 1937 (with Adorno and Benjamin in the audience) continued as a kind of “proxy war” over Russell's *Inquiry into Meaning and Truth* (1940). Neurath and Carnap debated the meaning of syntax and semantics not only in mathematics and the empirical sciences, but also in everyday life, in the courts, and in the press.

After tracing this dispute in some detail, I propose to take an example indeed from “everyday life”, namely the famous case of a respected journalist who was finally exposed as a producer of fake news for years (Claas Relotius). I hope that from this episode one can learn something about the important function and meaning of the concept of truth outside the technicalities of philosophical discussions.

1 Introduction

In the manifesto *Wissenschaftliche Weltauffassung. Der Wiener Kreis* we find the names of three “leading exponents of the Scientific World Conception”. These pillar saints of logical empiricism were: Albert Einstein, Bertrand

Russell and Ludwig Wittgenstein.¹ Russell was the only one of the trio who followed the path of the empiricist movement for a while after the publication of the manifesto in summer 1929; he participated in the First Congress of Scientific Philosophy 1935 in Paris and wrote a contribution² to the first issue of the *International Encyclopedia of Unified Science* (IEUS), a project that was adopted there and started publication in 1938.

Afterwards, however, he became increasingly dissatisfied with the development of the movement and also provoked harsh reactions by critical and polemical remarks. Particularly worth mentioning here is the exchange between Otto Neurath and Rudolf Carnap on Russell's *Inquiry into meaning and truth*³ (hereafter cited as *Inquiry*), which has since been published in the volume by Jordi Cat and Adam Tuboly *Neurath Reconsidered*.⁴ There Neurath offered meticulous criticism of the book, and Carnap replied at length. This exchange has been little commented on so far.⁵ I shall describe the dispute with the help of published as well as new unpublished sources. This controversy will be treated in my paper in the second chapter.

In the first chapter, I shall start with a prehistory, which includes – in the case of Carnap – his early correspondence with Russell along with the mostly positive evaluation by Neurath in an article on Russell in the austro-marxist journal *Der Kampf*. The contacts extend to the Vienna Circle's manifesto (coauthored by Neurath and Carnap), the first and third of those International Congresses of Unified Science in Paris and Russell's contribution to the first collective volume of the IEUS. In the third chapter, I will concentrate on the reactions of Neurath and Carnap to Russell's *Inquiry*. In the fourth chapter, I will conclude with some remarks of the relevance of the dispute between Carnap and Neurath on the notion of truth in science and in everyday life from today's perspective, where the concept of truth has become more and more a sort of endangered species. Carnap had already underlined the importance of the concept of truth in matters like jurisdiction and everyday life. Today's polemics about alternative facts and fake news in vastly advanced media (the internet, social media, etc.) makes the issue all the more important.

1 Stadler/Uebel (2012), pp. 76 and 108; for the composition of the manifesto, see Uebel (2012).

2 Russell (1938/1969).

3 Russell (1940).

4 Cat/Tuboly (2019).

5 See Hegselmann (1985), Mormann (1999) and Carus (2019).

2 Prehistory until 1940

2.1 Before 1929

Carnap, according to his own account, was influenced in his philosophical development mainly by two scholars, namely by the Jena mathematician and logician Gottlob Frege and by Bertrand Russell.⁶ From Frege with whom he had studied, he learned “carefulness and clarity in the analysis of concepts and linguistic expressions”⁷ as well as the insight that the lessons gained in dealing with mathematical logic – from concept formation to modes of reasoning – can and must be applied outside logic and mathematics. As for Carnap’s philosophical methodology, on the other hand, Russell’s example has been decisive. It started when Carnap got hold of Russell’s *Our Knowledge of the External World* and immediately understood it as a guideline that had already implicitly underpinned his own philosophical work. It even seemed to him that Russell’s appeal to resolutely sweep aside traditional approaches to philosophy and instead to pursue “the new method, successful already in such time-honored problems as number, infinity, continuity, space and time” and to apply it also to other areas of science, was virtually given to him personally as a mission. Besides, his interest in Russell’s mathematical logic and especially his relation theory continued. When in 1923 Russell sent him a long, handwritten letter with the most important definitions of the *Principia Mathematica*,⁸ a book Carnap could not afford to buy because of the mega-inflation in Germany, Russell certainly became his leading figure for a longer time. From then on, Carnap studied Russell’s new publications attentively, discussed some of them in the Viennese circle and occasionally made them the subject of his seminars at the University of Vienna.⁹

Neurath must have been less enthusiastic about Russell already in the 1920’s; in an essay dedicated to Russell in the austro-marxist theoretical journal *Der Kampf* entitled “Bertrand Russell, der Sozialist”,¹⁰ he initially praised Russell for his “anti-capitalist way of thinking”¹¹ and “perceptive clarity”, and

6 See Carnap (1963), pp. 11–14.

7 Ibid., p. 12.

8 Ibid.; in Carnap’s diaries we read on the tenth of November 1923 only “Russell’s works are here!” (Carnap (2022), p. 187).

9 This applies especially to Russell’s *Knowledge of the External World*, treated by Carnap in the Winter-Semester 1928/29 (Carnap’s diaries, 23rd of December 1928). Compare the list of Carnap’s lectures in Vienna, in Stadler (1997), p. 672, where it is announced as “Philosophische Übungen” only.

10 Neurath (1929/1981).

11 Ibid., p. 337.

celebrated his unbending pacifist stance in the First World War, calling him a “hero”.¹² Then, however, he went on accusing him of “petty bourgeoisie” on several occasions.¹³ This is not only because of Russell’s critical attitude towards Marxism, but also related to his polemics against aspects of the young Soviet Union.¹⁴

2.2 *From the 1929 Manifesto to the First Congress of Unified Science, Paris 1935*

Russell was in a transitional phase in the mid-1930’s. After he had spent a long time on his school projects and had published extensively on them establishing himself as a public intellectual, he now tried reconnecting with the development of scientific philosophy, the advancement of which he had not followed for some time. The groundbreaking results of Gödel, for example, had remained unknown to him. Because such works seemed to him “new and difficult to understand”, he had already toyed with the idea of leaving these discussions in the development of logic to a new generation altogether.¹⁵

Neurath’s invitation to the first congress for scientific Philosophy in Paris in 1935 was initially turned down by Russell for health reasons, but in the end he accepted. Although he did not give a full paper, he did at Neurath’s suggestion make remarks at the opening session on the question of how he in his philosophical beginnings had freed himself from Hegel’s influence.¹⁶ He also participated here and there in the debates as they progressed.

In the 1936 proceedings of the congress, Russell described positive surprises that the congress had brought him:

The Congress of Scientific Philosophy in Paris in September 1935 was a remarkable occasion, and for lovers of rationality, a very encouraging one. My first impression, on seeing the opening sessions, was one of surprise: surprise that there should be in the world so many men who think that opinions should be based on evidence. My second impression, on hearing the papers and discussions, was one of further surprise, to find that the opinions advocated actually conformed to this rule: I did not discover any of the signs of unfounded and merely passionate belief

12 Ibid., p. 338.

13 Ibid., pp. 340–343.

14 Ibid., p. 341; compare Russell (1920) and (1923).

15 Carnap diaries, 20th of September 1935.

16 Ibid., 15th of September 1935.

which, hitherto, have been as common among philosophers as among other men.¹⁷

He even expressed the hope that the meeting of logic and empiricism in the new “scientific philosophy” could create the intellectual mood “in which it is possible to find a cure for the diseases of the modern world”.¹⁸

In terms of content, the Paris congress had brought two main novelties for the logical empiricist movement: the foundation of an encyclopedia project and the first major disputes about semantics.

At the congress the plan of an *International Encyclopedia of Unified Science* (IEUS) was presented by Neurath, put to a vote by Charles Morris (besides Carnap a later co-editor) and accepted.¹⁹ That brought a long development to a happy end, which had started as Neurath’s plan for a “Volksbücherei” back in 1921, which was then endorsed by Einstein himself as a possible continuation of the Encyclopedia of the Enlightenment in the 18th century and its actualization for the 20th.²⁰

Another main topic of the congress was the discussion about the new philosophical discipline of semantics, which was presented by Polish participants like Alfred Tarski and Maria Lutman-Kokoszyńska and made a great impression especially on Carnap but also on the young Karl Popper. While the encyclopedia project was the focus of joint efforts of logical empiricists over the next 10 years, the discussion of semantics was to harbor the seeds of a split in the movement, as was especially evident in the correspondence between Neurath and Carnap during World War II years when Carnap published his first books on the subject.

If one wants to understand the disputes between Neurath and Carnap on semantics since 1935, which marks the outset of the process of their increasingly dramatic distancing from each other,²¹ one has perhaps to go back to 1930. For at that time Carnap met Alfred Tarski – the founder of this discipline shimmering between philosophy of language and logic – for the first time in Vienna, before he visited him in Warsaw in the same year where he gave a number of lectures.²² Carnap’s trip to Poland has been covered recently in a

17 Russell (1936/1996); see also Stadler (1997), p. 404f.

18 Russell (1936/1996), p. 121.

19 See Dahms (2018) for details.

20 Dahms (1996), p. 53 f.

21 This process culminated in 1944, when Carnap refused to sign Neurath contribution to the IEUS as editor. See Hegselmann (1985), p. 286f. and Dahms (2020), p. 89f. That episode will not be covered in this paper.

22 Carnap (1963), p. 60f.

paper by Anna Brozek.²³ At that time Carnap was still caught up in the idea of a purely syntactic approach (which he had, however, already called a “semantic” one). Neurath resisted this conceptualization from the beginning, partly with strange justifications, suspecting that even the “semantics” had typically Prussian undertones (see in this volume Höfer’s paper, sec. 1). Carnap turned to semantics in the proper sense only when he was able to study Tarski’s forthcoming groundbreaking publications on the subject in detail. After Tarski’s visit to Vienna, Carnap used the occasion to invite him to the First Congress for Unified Science in Paris in 1935.

At the congress it came to disputes between proponents and supporters of the new semantics like Tarski on the one hand and Neurath and Carl-Gustav Hempel on the other who wanted to stick to the “physicalist” syntactical point of view. The dispute can hardly be traced in Neurath’s report on the course of the conference (see below).²⁴

An overview of the discussions between the Polish delegation in Paris and its critics has recently been given by Wioletta Miskiewicz and by Jan Woleński in a volume of the French journal *philosophia scientiae* on the occasion of the 80th anniversary of the conference.²⁵ Woleński summarized the difference between the viewpoints of the two in a series of theses as follows:

1. Neurath
 - The use of the concept of truth should respect the logic of science.
 - We compare sentences with sentences, not sentences with something else.
 - The conception of truth as based on comparing sentences with “reality” is dangerous for empiricism and introduces metaphysics.
2. Carnap-Tarski-Kokoszyńska:
 - The semantic definition of truth is admissible and even correct.
 - Truth cannot be replaced by syntactical concepts.
 - Semantical concepts are useful in the logic of science.
 - Truth and confirmation must be distinguished.²⁶

Especially remembered in the lively debate in Paris was the bonmot of Louis Rougier, the organizer of the congress, who claimed that one expects a waiter

23 Brozek (2021).

24 Neurath (1935/1981).

25 Miskiewicz (2018) and Woleński (2018), in Bourdeau/Heinzmann/Wagner (2018).

26 Woleński (2018), p. 208.

in a restaurant to serve a dish after an order and not only to make a reference to some linguistic entity like a menu.²⁷ This little story, which was later on cited by Russell²⁸ shows that the dispute was not only about the concept of truth and its use in logic and empirical science, but also about even more far-reaching problems of the relation between language and the world (see below).

It seems that Neurath tried hard to play down the severity of the conflict in his report of the proceedings of the conference. There he mentions with approval Carnap's attempt to differentiate between truth and confirmation²⁹ and describes correctly and without any critique or even polemics Tarski's and Lutman-Kokoszyńska's presentation of the new science of semantics:

As "absolute concept of truth" they denote the classical concept of truth, according to which, as the saying goes, the truth of a sentence consists in its "correspondence with reality". This concept does not have to be rejected as unscientific, it could, if specified accordingly, serve as representative of those concepts which cannot be defined in its own syntax, but in an enlarged syntax. Lutman-Kokoszyńska showed how even the question of the criterion of truth and the question of "what is the real world like" can be understood, if interpreted appropriately, to include no pseudo-problems any more, by formulating them as follows: "Which synthetic sentences of a particular language are absolutely true?"³⁰

Eventual counterarguments against this standpoint, which came up in the following discussion (like the danger of metaphysics) were shortly named by Neurath in his report, but remained anonymous. One could not find out, that it was he who harbored those suspicions. When he explicitly described his own standpoint, he did this in a rather mild way. He conceded that the ideas of Tarski and Lutman-Kokoszyńska "might be of importance for the construction of scientific languages." On the other hand, he expressed his hope that the new semantics would not be seen as an attempt to eliminate a coherence theory of truth because nobody would defend such a stance in the logical empiricism movement in the first place. But then he continued:

27 Reported in Neurath (1935/1981), p. 666 f.

28 Russell (1940), p. 141; I will come back to this episode and Neurath's answer to this criticism.

29 Neurath (1935/1981), p. 664.

30 *Ibid.*, p. 665 (trans. H.-J.D.).

From a terminological point of view he [Neurath] holds that the term “true” could be reserved for *that* encyclopedia (among the many of those non-contradictory encyclopedias controlled by protocol sentences) one had decided to accept, such that “true” would be called each consequence of this encyclopedia and each new sentence added to it, “false” each contradicting sentence.³¹

With a view to the further dispute between Neurath and Carnap about semantics it seems interesting that Neurath was at that stage of the debate still prepared to use concepts of “true” and “false”, whereas he later on was intent on removing them – along with several other “dangerous” ones – from the scientific vocabulary altogether.³²

2.3 *Semantics at the Third International Congress of Unified Science, Paris 1937*

The smoldering conflicts between Neurath and Carnap on semantics were not settled until the run-up to another congress, namely the Third International Congress for Unified Science of 1937 (immediately before the IX. International Congress of Philosophy, both held in Paris). The main theme of that relatively small gathering (never more than 40 participants) was announced as “The International Encyclopedia of Unified Science”. Only a few months before the conference the University of Chicago Press had agreed to publish the Encyclopedia under the condition that 250 subscribers could be found. Therefore, this conference-theme was quite timely. As we can gather from the correspondence of the editors, not so much the project as a whole should be discussed in Paris, but more specifically the subjects of psychology and biology and their integration into the new encyclopedia. The editors’ hope was clearly to attract suitable authors for those topics. They were very happy to find the “house-psychologist” and future contributor to the IEUS for psychology in the person of Egon Brunswik. Another purpose of the congress was the unification of logical terminology. These two main issues were already mentioned in the invitation to the conference and accompanying publications.³³ They will be discussed in this paper only insofar as Russell was involved in the IEUS.

What needs to be described and discussed here is the content of two “private discussions” (Privataussprachen) not mentioned in the public schedule of the conference, which should be held according to Neurath’s proposal under

31 Ibid., p. 666 (trans. H.-J.D., emphasis and brackets added for easier understanding).

32 See Neurath (1944/1969), p. 51: “expressions avoided in this monograph”.

33 Stadler (1997), p. 422f.

almost top secret circumstances (very few participants and outside the official venue).³⁴ They were mentioned only briefly in the Neurath's preface to the proceedings of the "Enzyklopädie-Konferenz" after the Congress. One of these private sessions were introduced by Carnap and Neurath about semantics and another one by Carnap and Reichenbach about probability and confirmation.³⁵ The second is of interest here only insofar as the young philosopher and psychologist Arne Næss presented the results of his psychological findings about the meaning of the word "truth" in every day discourse.

Carnap had prepared a discussion paper for the first session during his journey to Paris after meetings with Hempel, among others, in Daverdisse (in the Belgian Ardennes mountains). He also discussed the subject in the train from Bruxelles, where Neurath joined him, to Paris. At the outset Carnap presented four theses in his paper about the semantic concept of truth:

- 1) it is correct and flawless;
- 2) it cannot be replaced by the "method" of the "logic of science" (i.e. the syntactical method);
- 3) it is useful and important;
- 4) it is in accord with the concept "true" of everyday language.³⁶

Only a few points can be highlighted here: Carnap shows with respect to his second thesis, that semantical concepts cannot always be eliminated or translated into syntactical ones; the term "true" cannot be replaced by "encyclopedical, scientifically acknowledged, believed, accepted", because the second always carries a relativization to a person or a group with it and a reference to a point on the time-scale.³⁷ In the fourth chapter "the concept of truth in common language" Carnap shows that in everyday discourse in a broad sense (which includes scientific argumentation) sentences containing the word "true" are logical equivalent to sentences from which the word was eliminated. This argument was used to show that the concept of truth was harmless and its use would not involve any metaphysical danger. The difference is only psychological: "true" is mostly used to state something as beyond doubt or express something more emphatically. That sounds like an anticipation of the later performative speech-act theory of truth by John L. Austin.

34 Neurath to Carnap, 3rd of June 1937.

35 See Neurath's preface in Schulte/McGuinness (1992), p. 202.

36 Carnap (1937), p. 1.

37 *Ibid.*, p. 4.

With respect to the controversy within logical empiricism, two camps are now identified in the fifth chapter “practical proposals”, namely first the “Poles, especially Kotarbiński, Tarski, Lutman and the Chicagoans, Carnap, Hempel, Helmer”.³⁸ The other side of the controversy is now pictured as a tiny minority: it consisted only of “especially Neurath, perhaps also Næss and others.”³⁹

In this portion of the paper Carnap confesses that he cannot hope to reach agreement between the two groups already during the conference and instead gives useful advice to both camps with a view to the future. Under all circumstances he wants to prevent “that present differences of opinion reach the public too early and are expressed in a too sharp manner (zu überspitzt)”.⁴⁰ So, the pro-semantic camp should see to it that in their future work a demarcation against metaphysics is always observed, whereas the sceptics towards semantics should maintain a waiting attitude and should in any case avoid to go public against the semantics project polemically.

Whether Neurath also delivered a discussion-paper about semantics for the conference I do not know. It could well be that he was very much distracted by the terminal illness of his second wife Olga who died a few days before the congress and let him ponder whether to travel to Paris at all.

No report on the proceedings of that discussion was published afterwards and unpublished sources like the correspondence Carnap/Neurath give only very few hints. In Carnap’s diary we find at least the following short entry:

In the afternoon my discussion with Neurath (so-called private, but public) about the semantic concept of truth. Tarski and Lutman defend semantics together with me. Arne Næss must concede, that 90–95 % of the interviewed treat the two sentences “...” and “...’ is true” as equivalent”. On the whole our arguments are more convincing now. But Neurath already – owing to the notes sent from Daverdisse [Carnap (1937)] and our long conversations – weakened his standpoint quite considerably.⁴¹

Now the very astonishing fact is that the whole Third Congress of Unified Science is covered in a report by participants of the conference whose presence one would not expect at all, let alone during the “private discussion” on semantics. These two were Theodor W. Adorno and Walter Benjamin from the exiled Frankfurt Institute for Social Research. Adorno had been sent by Max

38 Ibid., p. 10.

39 Ibid.

40 Ibid., p. 11.

41 Carnap diaries, 29th of July (Carnap (in preparation)).

Horkheimer as official representative of the institute. How it came to this situation need not detain us here:⁴² the academic tradition and also political leanings of the two camps – the logical empiricist and the critical theorists was – before spring 1937 – not that far from each other as one would expect from the later “positivism” dispute of the 1960’s.⁴³ At the Paris Third Congress a long discussion between Neurath and Adorno and afterwards an extended discussion between members and representatives of the two groups took place in order to discuss Horkheimer’s polemical article “The latest Attack against Metaphysics” against the logical empiricist.⁴⁴ In any case: the only more extensive report on the discussion on semantics in the Paris Congress of 1937 is contained in a paper by Adorno and Benjamin (which was published only in 2003!). The two write:

The discussion theme of the afternoon session on the 29th of July is the semantic concept of truth. Neurath is opposed to confront the statements of Logistics with “Being”: He only wants to compare sentences with sentences ... Here the connection of this new positivism with idealism can be grasped by hands.⁴⁵

On the opposite side stood – according to the report – the “alleged” proof by “the pole Tarski against the logicians that the traditional concept of truth could be maintained” without contradiction. But:

Neurath wishes to eliminate it nevertheless, because it could lead perhaps to antinomies. Carnap is of the opinion that “True” is unavoidable.

As Adorno notes: “The discussion seems to show me – insofar as I understood their secret language (*Geheimsprache*) – that the gentlemen were *not unified inter se*.”⁴⁶

So, it happened that the impression of a split in an important philosophical question among logical empiricists indeed reached two representatives of the

42 See Dahms (2021) for details.

43 See for the relations between logical empiricists and critical theorists of the Frankfurt school Dahms (1994).

44 See Dahms (1994), pp. 97–143 for a discussion of Horkheimer’s polemics and Dahms (2021) on all the international congresses during the NS-dictatorship and especially the logical empiricists Paris congresses of 1935 and 1937 and Adorno/Benjamin reports on both congresses of 1937 (the big IX. International one and the small third of logical empiricists).

45 Adorno/Benjamin (2003), p. 567 (trans. H.-J.D.).

46 *Ibid.*, p. 568 (emphasis added).

more general philosophical public, who furthermore became more and more antagonists of logical empiricism.

Adorno adds remarks on another aspect of the dispute about truth, namely psychological research used to judge the “adequacy” of different philosophical concepts of truth:

In a bold transition from the discussion about the concept of truth follows a talk of a Norwegian Arne Næss, who informs about empirical investigations he made with 117 test persons. These tests are meant to show which ones of the different conceptions of truth are present in the general public. According to the results not only the naive-realistic, but also the Aristotelian and relativistic concept of truth has found followers in all social ranks (Stände). Nothing is said about the social and educational preconditions of the test persons. ... His talk found especial acclaim. Carnap welcomed that the conceptual inventions of the Vienna Circle were confirmed by an authoritative psychological side.⁴⁷

But if one takes Næss's results seriously, which were subsequently published as a book,⁴⁸ one will find that different concepts of truth are used in everyday life and one is led to conclude that the adequacy of a philosophical explication of truth cannot be determined by empirical psychological means, at least not by such investigations alone.

3 Disputes between Neurath and Carnap about Russell's *Inquiry*

As we have seen, Russell had expressed himself favorably and even enthusiastically about the movement of logical empiricism after the first Paris Congress of 1935. He did not participate in the 1937 third Congress and one may speculate whether his estimate would have remained the same. In any case, as late as 1938 he seems to have been quite content with the development of the movement. He had published a positive review of Alfred Ayer's *Language, Truth and Logic* from 1936.⁴⁹ In the same year he also delivered his promised contribution to the first volume of the *IEUS*.⁵⁰ It seems that he took his task in his contribution

47 Adorno/Benjamin (2003), p. 568; see Næss (1938/2014) for a full picture of his psychological research about ordinary people's conception of “truth”.

48 Næss (1938/2014).

49 Contained in Russell (1996).

50 Russell (1938/1969).

“On the Importance of Logical Form” a little bit light-heartedly. In its three pages, he stressed the function of mathematical logic not for pure mathematics, but for various empirical sciences. So, he sketched briefly its application for some physical theories, but also for sociology and psychology. Without mentioning Carnap’s booklet *Scheinprobleme in der Philosophie* explicitly, he also commented upon the eventual mutual translatability of sentences built on a physicalistic basis and a psychological one – and the possible avoidance of the notorious idealism/materialism dispute. Russell ended his contribution with an outlook for the future:

The unity of science, which is sometimes lost to view through immersion in specialist problems, is essentially a unity of method, and the method is one upon which modern logic throws much new light. It may be hoped that the Encyclopedia will do much to bring about an awareness of this unity.⁵¹

Immediately thereafter, however, he began a more critical engagement with some positions within logical empiricism. It is expressed clearly in his article “On Verification”, his address at his second assumption of the presidency of the Aristotelian Society.⁵² There, in the second part of his remarks, he writes:

Certain philosophers – notably Neurath, Hempel and (less definitely) Carnap – have been led by the fear of metaphysics to a view very different from that which I have been expressing. They think that language can be treated in isolation, without assuming that it refers in any way to fact outside itself. What they call “Protocol statements” (i.e. the statements asserting empirical premisses) are not accepted because of any agreement with “Reality” (a nasty metaphysical word, to be avoided at all costs), but because they are “actually adopted by mankind, and especially by the scientists of our culture circle”.⁵³

His judgment is harsh:

I find it difficult to believe that these philosophers, who profess to be empiricists, can really mean what they say.

51 Ibid., p. 41.

52 Russell (1938/1996).

53 Ibid.

3.1 *Russell's Criticism of Neurath and Hempel*

The publication of Bertrand Russell's *Inquiry* in 1940 further aggravated the situation. Russell wrote in the preface that he was more in sympathy with the logical positivists than with any other existing school, but noted some divergencies with them already in the introduction.⁵⁴ But the father of empiricism who had been singled out in the manifesto of the Vienna Circle as one of the three leading embodiments of that spirit, dealt in detail with a doctrine which he understood – in his interpretation of Neurath – as a coherence theory of truth. According to this theory, first of all, the concept of truth had to be completely deleted from scientific vocabulary. Furthermore, it would only matter to examine the compatibility of newly added propositions with basic propositions recognized in a community of scientists. And even such basic propositions could be revised under certain circumstances.

Russell must have shuddered when confronted with such a theory of scientific acceptability, because it contradicted his most central convictions. I have in mind here his rejection of Hegelianism, against which he had rebelled during his own transition from British Neo-Hegelianism to Empiricism during his philosophical beginnings.⁵⁵ It may have struck him as especially strange that Neurath had asked him to sketch this abandonment of Hegelianism at the 1935 Congress in Paris; and now the same Neurath seemed to act as propagandist of a coherence theory (which seemed to Russell similar to the Neo-Hegelian Harold H. Joachim's),⁵⁶ to abandon a correspondence theory of truth and even call the latter one "metaphysical".

Russell thought, that for an encyclopedia such as *IEUS*, which was at the time under construction and was – according to his opinion – only designed to summarize existing theories, it might be sufficient to refer to already established bodies of knowledge.⁵⁷ So he wrote:

The man who is constructing an encyclopedia is not expected himself to conduct experiments; he is expected to compare the opinions of the best authorities, and arrive, so far as he can, at the standard scientific opinion

54 Russell (1940), p. 18ff.

55 See Russell (1944), pp. 9–11 for a short description of his Hegelian period and the reason for his abandonment of Neo-Hegelianism. A longer critique of Hegel can be found in Russell (1946), pp. 701–715.

56 He mentions his critique of Joachim's theory of truth (dating back to 1910) in Russell (1940), p. 133.

57 *Ibid.*, p. 135.

of his time. Thus in dealing with scientific question his data are opinions, not direct observations on the subject-matter.⁵⁸

For scientific work on the immediate research frontier, however, this idea is absurd, according to Russell. Here one needs the confrontation of propositions with immediate sensual experience by the individual scientists. It can happen that his findings contradict previously accepted opinions. Only if these results are reproduced they have a chance to grow from individual to public knowledge.

Finally, the coherence theory of truth propagated by Neurath (and formerly by Hempel too) was, according to Russell, downright dangerous: if it was only important to accept a proposition in agreement with other ones, that amounted to a return of philosophical idealism. That was one danger. But now in addition, if whole systems of propositions – in everyday life and even in science – differed from each other *in different cultural circles*, then one was going outside empiricism. The rejection of a social relativist theory of acceptability was a second corner-stone of Russell's empiricism, which we find even in some of his writings on matters most remote from scientific philosophy. So, in his *Marriage and Morals* we read:

The fact that an opinion has been widely held is no evidence whatever that it is not utterly absurd; indeed, in view of the silliness of the majority of mankind, a widespread belief is more likely to be foolish than sensible.⁵⁹

For Russell, empiricism is only compatible with a correspondence-theory of truth⁶⁰ and so he criticizes in his book every other approach to "truth", be it John Dewey's theory of "ascertained assertability", Hans Reichenbach's probabilistic theory and also the coherence theories of truth, "advocated by Hegelians and certain logical positivists".⁶¹ Russell's criticism became polemical, when he remarked – against some of those "certain logical positivists" – for instance:

The verbalist theories of some modern philosophers forget the homely practical purposes of every-day words, and lose themselves in a neo-neo-Platonic mysticism.⁶²

58 Ibid., p. 136.

59 Russell (1929/1976), p. 44.

60 Ibid (1940), Chap. 21.

61 Ibid., p. 272.

62 Ibid., p. 141.

Russell also did not miss the opportunity to insert a timely polemical remark on that relativism with respect to culture circles and their different communities of scientists, which played a role in Neurath's view of accepting sentences into an established body of knowledge. If it were so, Russell wrote, that "in a different culture circle another body of propositions may be accepted", then one might consider, if one thought this to be true, that "owing to this fact, Neurath is an exile".⁶³

This reads like a spiteful *ad hominem* remark. However, taking into account the fact, that even in the "hardest" sciences like mathematics and physics those national-socialist racist "German" programs had flourished since 1933 – not to mention corresponding programs in the humanities and social sciences – the polemics seem to make some sense.

3.2 *Neurath's Answer*

Neurath reacted to this criticism in the last pages of his article "Universal Jargon and Terminology" in the *Proceedings of the Aristotelian Society*.⁶⁴ There he acknowledges that Russell's books had "great influence within the Unity of Science movement and in the analysis of language" and quotes his expression of "sympathy with the logical positivists" which went further than "with any other existing school". But then he goes on to engage with some of the most polemical points in Russell's *Inquiry*. I can discuss here only two of them. Russell wrote:

The purpose of words, though philosophers seem to forget this simple fact, is to deal with matters other than words. If I go into a restaurant and order my dinner, I do not want my words fit into a system with other words, but to bring the presence of food.⁶⁵

Now Neurath tries to avoid the impression suggested by this example that the (linguistic) order of food aims at something outside of verbal reality. In order to achieve this, he rephrases the possible clash of the order of chicken with the mistaken serving of rabbit: "the word-thinking of Russell, "A chicken will appear" (in connection with his order) seems to be contradictory to his word-thinking: 'no chicken appeared.' That is all."⁶⁶

63 Ibid., p. 140.

64 Neurath (1941/1981).

65 Russell (1940), p. 141.

66 Neurath (1941/1981), p. 145.

Now I doubt that Russell would leave it in the given situation at a word-thinking: “Oh, interesting, I ordered chicken, but no chicken appeared (but a rabbit instead).” He would surely then call the waiter (and perhaps even the manager) of the restaurant, complain about the serving of the wrong dish, etc. So, “that is *not* all”: An order in a restaurant is a speech-act with a whole number of felicity-conditions which transcend a *single* persons “word-thinking”. It amounts to a sort of contract between *two* parties, the one who orders a dish and thereby promises to pay the required price and the other who offers the dish on his menu and thereby promises to deliver what is ordered. This procedure involves a whole number of interactions of the (not only “word-thinking”, but also speaking) client with the waiter and a whole lot of further occurrences when the contract is not fulfilled; if, e.g. the dish arrives late, is not of the promised quality or consists of something in part or on the whole completely different. Diminished or completely withheld payment (and nowadays in addition adverse commentaries in social media sites) may be the consequence.

The second example concerns Russell's criticism of Neurath's relativization of the acceptance of propositions and theories to social groups and environments: “In a different culture circle another body of propositions may be accepted; owing to this fact, Neurath is an exile. [...] In other words, empirical truth can be determined by the police.”⁶⁷

Neurath repeats in his reaction his opinion that it is impossible to contemplate “a fight between ‘error and truth,’ but only between different groups of thinkers”.⁶⁸ Even if an absolute truth existed and with it a superhuman entity which chaired disputes about truth, humans could only discuss diverging truth-claims with their human means. But is this really true, we could ask? I disagree!

Consider the following real-life examples: Could people and especially scientists from different groups, nations and backgrounds not come to the same conclusion, even when from very different ethical background and/or when discussing very controversial cases? Take the examples of three different “reasons” for starting mayor wars in the last 100 years, the alleged “polish attack of the German radio-station Gleiwitz” as what marks the spark for the onset of the Second World War in September 1939 by Nazi Germany, the so-called “Tonkin Attack in August 1964” that sparked the Vietnam War, and Saddam Hussein's alleged “Weapons of Mass Destruction” as the background for the unleashing

67 Russell (1940), p. 140.

68 Neurath (1941/1981), p. 147.

starting of the Second Iraq War in March 2003.⁶⁹ Are there any historians left who would dispute that in these cases, wars that costed many unthinkable lives – soldiers and civilians – were based on lies and fabrications? Or take the Holocaust: is the opinion of some deniers of equal worth on a par with serious historiography?

These examples show, that one does not need a “chair” filled with a figure of superhuman capacities to decide very controversial disputes between scientists. In most cases an agreement is reached by mutual criticism and exchange between scientists. In very special cases commissions can be useful to arrive at the truth regarding historical matters (like the truth commissions after the fall of the apartheid in South Africa or the schoolbook-commissions of Polish and German historians and geographers, which were founded in 1972 and exist to the present day).

3.3 *Neurath's and Carnap's Controversy about Russell's Inquiry*

Neurath knew from Carnap that the latter had been working for some time on books on semantics. In advance of these publications, Neurath tried to engage him in extensive discussions of Russell's book. Thus, in July 1942, he sent him a long letter in which he commented critically on Russell's *Inquiry* over several pages.⁷⁰ It is impossible to discuss these points here in any detail, because that would require a full-scale comparison of the book-passages picked by Neurath with his criticism. This might amount to enough material for another article. On the whole, Neurath's remarks boil down to a rejection of an “absolute truth”, which only God or a superhuman sitting in a judgment chair may know, whereas no human ever could get access to “this damned ‘absolute truth’”.⁷¹ Carnap did not reply in detail until March 15th, 1943, but then also at length.⁷² In this letter he also noted some points of divergence with Russell, among others the latter's “naive realism”. Other points were in Carnap's opinion not especially many and important ones. That may have to do with the circumstance that Russell's book was based in part on lecture notes Russell had prepared for

69 The last example shows how difficult it can be and how long it can take, before the truth gets its way: The French and German government were “not convinced” by the US’ “proofs” for Saddam Hussein's possession of weapons of mass destruction and consequently did not enter the second Iraq War, Spanish Prime Minister José Luis Zapatero pulled out his troops immediately after his election in April 2004 saying that one could not base a war on lies. Even Donald Trump admitted many years later that the Iraq War had been “a mistake”.

70 Neurath to Carnap, 17th of July 1942, in: Tuboly/Cat (2019), pp. 544–552, esp. pp. 546–549.

71 Ibid., p. 550.

72 Carnap to Neurath, 15th of March 1943, in: *ibid.*, pp. 575–579.

a seminar in Chicago attended by both Carnap and Charles Morris in 1938/39.⁷³ More importantly, Carnap had helped Russell with the proofs of *Inquiry* in a number of sessions in Harvard in autumn 1940.⁷⁴

Between Neurath's criticism of Russell and Carnap's reply to it now fell the publication of Carnap's book *Introduction to Semantics*,⁷⁵ the first one of a three-volume series. When he had it in his hands, it burst out of Neurath:

I am really depressed to see here all the Aristotelian metaphysics in full glint and glamour, bewitching my dear friend Carnap through and through.⁷⁶

In his book, Carnap agreed with Tarski that the semantic concept of truth is in accordance with the colloquial use of the word "true":

It seems to me that he (Tarski) is right in this assertion, at least as far as the use in science, in judicial proceeding, in discussions of everyday life on theoretical questions is concerned.⁷⁷

In the later development of logical empiricism, the controversy between Neurath and Carnap over the Russellian attack has not played a major role. Hempel, who changed camp from Neurath's to Carnap's standpoint during the conflict, inserted only a footnote in one of his articles on the dispute:

While, in the articles in *Analysis*, I argued in effect that the only possible interpretation of the phrase "Sentence S is true" is "S is highly confirmed by accepted observation reports", I should now reject this view. As the work of A. Tarski, R. Carnap, and others has shown, it is possible to define a semantical concept of truth which is not synonymous with that of strong confirmation⁷⁸

73 Russell (1940), preface.

74 See Carnap's diary entries; I wonder whether the polemical remarks against Neurath in *Inquiry* were already included in those proofs or added later on. If they were not added later, that could indicate that Carnap shared their content.

75 Carnap (1942), followed by his (1943) and (1947).

76 Neurath to Carnap, 15th of January 1943, in Tuboly/Cat (2019), p. 570.

77 Carnap (1942).

78 Hempel (1945/1965), p. 42, n. 49. He also stated that Russell's depiction and criticism of Neurath's standpoint as a coherence theory of truth was a misunderstanding.

Carnap mentioned the dispute with Neurath in the semantics section of his “Intellectual Autobiography” indirectly, when he noted: “To my surprise, there was vehement opposition even on the side of our philosophical friends” and mentioned “the skepticism and active resistance”⁷⁹ that the new discipline of semantics encountered in its early stages. He remarked with satisfaction that among younger readers the importance of semantical concepts for philosophical analysis was widely recognized later on.

It might well be that with the rise of the sociology of science, Neurath’s point of view, which puts emphasis on consensus among groups of scientists as a sort of substitute of truth, has become popular again (though few seem to be aware of his role as a predecessor).

4 Truth in Science and Everyday Life

4.1 *Truth in Science*

As we have seen, Carnap put emphasis on the adequacy of the explication of “truth” in his 1937 paper, especially in its chapter “on the conception of truth in everyday language” by comparing it with common use; and furthermore, he put the use in everyday life and science on an equal footing when he wrote:

I don’t speak here about the concept of truth of the metaphysicians, but about the one of the common language (*including its use by scientists*).⁸⁰

That is of course an oversimplification. With regard to science, the situation is much more complicated, above all because scientific theories – in contrast to newspaper reports or the taking of evidence in court – in addition to aiming at factual exactitude make a claim to generality as well as to explanatory and predictive power. Moreover, scientific theories usually are supposed to survive for a longer period of time (whereas everyday uses of “true” information in newspapers and in court are very often quickly forgotten). However, it can also happen that scientific theories that were previously considered true or hypotheses that were considered plausible are thrown overboard. Not only the examples of scientific revolutions, but also the development of science under time pressure – as in the case of the recent Corona crisis – provide good illustrations.

79 Carnap (1963), pp. 60–67, especially 61f.

80 Carnap (1937), p. 8.

Consider, for example, the shifting statements of scientists about the usefulness of face masks for pandemic containment.

Also in calmer stages of scientific change, the question of truth content of earlier paradigms and theories arises and in turn prompting one to ask whether it is not to be expected that the new theories themselves will also one day be discarded, and also here, therefore, one cannot have claims to strict truth either. Basically, in view of the indisputable fact of such scientific change, there is only the alternative to completely renounce the claim to truth and to rest content – like Neurath – with consensus among scientists in a given scientific community and their “accepted sentences” within the framework of an accepted encyclopedia, or to advocate a theory of truth-approximation, which understands the respective current state of science as an intermediate step on the way to a final truth. This latter view was represented by Karl Popper and his followers. The recent debate in the philosophy, history and sociology of science moves between these two poles. It would be presumptuous, in view of the enormous mass of publications on the subject, to enter this debate here.

4.2 *Truth in Everyday Life, Especially within the Media, Their Producers and Users*

It seems to me that the reference to the role of the concept of truth “in everyday life” in the debates between Neurath and Carnap about semantics in the decade between 1935 and 1945 is exceedingly important and, moreover, of special actuality. For the concept of truth stands today as a cornerstone and counterpoint in the heated political disputes about the ever more widespread phenomenon of fake-news in politics and journalism. So, the concept of truth should be examined also in terms of its adequacy from *these* everyday modes of use. As we have seen, the participants of the 1937 third Congress for Unified Science had listened to a paper by Arne Næss about the presence of different uses of “truth” in the general public. This psychological research could in the end not decide the dispute between Neurath and Carnap.

Perhaps another approach will be useful to shed light on the controversy and give hints with a view to the relevance of different theories of truth. I have in mind lessons which can be drawn not so much from the ways of truthful reporting, but by demasking false reports and fake-news. In order to do this I will briefly address and analyze a case of manifest un-truth and fake news *produced by a (formerly) famous journalist himself*. After all, some journalists occasionally spread lies (even without merely reproducing certain statements made by politicians).

It is certainly true that as consumers of media such as newspapers, websites, social media, etc., we usually have to rely on reports that have already passed

through a chain of transmission from eyewitnesses, interviews to reporters or correspondents to agencies, to editors, and to final inspection before publication. In this respect news, perhaps even more than scientific findings, have a component of truth as coherence. At all these control stations are entry gates for the emergence of fake-news, if the corresponding checking mechanisms fail. For this reason, it must be possible to reconstruct the transmission context, at least in principle, and to verify its reliability, at least in cases of suspicion. If doubts arise, the entire process of message generation may have to be traced back. In this case, eyewitnesses or pictorial sources are needed.

I propose to analyze the “Relotius case”, where a journalist who had received many awards – also international ones – had delivered huge quantities of completely invented reports (among others for the prestigious German journal *Der Spiegel*) over a time-span of five years. The entire case was only exposed because a co-author of one report on illegal immigration to the US at its southern border and the alleged violent activities of militias against this immigration became suspicious. So this man, Juan Moreno, who had covered the stream of immigration south of the US border, whereas Relotius wrote about the fight of militias after their arrival in US, took the trouble to investigate and to scrutinize the truth-content of this report in a very strict way. His journey from some doubt in one report to the conviction of a serial liar in many stories is instructive in itself, but can also teach us lessons about the controversial issue about coherentist, consensualist and correspondence theories of truth and the interplay of certain of their elements in fact-checking procedures in every-day life, in this case: in journalism. In my opinion, something about the adequacy of explications of the concept of truth can be learned from them.

It all started with doubt, namely that a leading militia member was depicted with different names, ages, family connections, professions, carriers, addresses, etc., in different media, in different newspaper reports.⁸¹ After these contradictions could not be removed to Moreno’s satisfaction,⁸² he decided to locate and meet the man and other members of his group himself in company with a witness, a photographer.⁸³ It turned out that Relotius had met none of those militia men, but “referred” to what he had “heard” from and experienced with them: namely (invented) trips with this group in order to capture illegal immigrants, including deadly violence.⁸⁴ After Moreno’s return to Germany followed

81 Moreno (2019) p. 172ff.

82 Ibid., p. 180.

83 Ibid., p. 169, 197ff.

84 Ibid., p. 206f.

the arduous task to persuade the documentarists and also the chief editors⁸⁵ of the journal *Der Spiegel*, that his disproof was correct and not caused by jealousy, etc. In the end a mail demonstrably falsified by Relotius in order to save his whole report,⁸⁶ led to the final overturn.

What can be learned from this episode? A coherentist view is not enough; in this case non-coherent elements led only to doubt, but not to refutation. Consensus alone did not help either, because there was a strong consensus within the big documentation section of the journal and within its upper echelons, that Relotius was right and Moreno wrong. It was the combined result of the assembled eyewitness reports (done by more than one man and documented by the photographer) and the proof of an enormous “anomaly”, a demonstrable ad-hoc produced falsified e-mail in the final stage of the investigation that decided the case. Those latter stages had all to do with correspondence or non-correspondence to facts. So in this case coherence was helpful, consensus could also have been (but was not),⁸⁷ and correspondence was by far the most important element. I think it would be worthwhile to analyze the fact-checking procedures in journalism and especially life-size examples, where they spectacularly failed, in order to determine the adequacy of competing theories of truth.

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85 Ibid., p. 226ff.

86 Ibid., p. 228, 234f.

87 One might say, that discussions about the truth of the Relotius/Moreno-report inside the *Spiegel's* staff were not “free of domination”, as some consensus-theories of truth demand. But the case here under consideration shows, how far this “idealization” leads away from reality.

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Carnap and Neurath as Critics of the *Tractatus*

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Abstract

We retrace the early reception of the *Tractatus* in the Vienna Circle, before analyzing the criticism of the *Tractatus* by Neurath and Carnap. While Neurath rejected vehemently the Tractarian picture theory of meaning and Wittgenstein's interpretation of elementary propositions formulated in a phenomenal language (Wittgenstein 1929), Carnap's criticism of the *Tractatus* was shaped by the need to develop a new logical syntax which goes beyond the one specified in the *Tractatus*. In this, he was not only influenced by Neurath's criticism of the *Tractatus*, but also by his conversations with Waismann about the need for and the nature of a new logical syntax.

1 Introduction

In 1931, Carnap and Neurath published a series of papers which included remarks sharply criticizing central positions of the *Tractatus*. In the *Logical Syntax of Language*, Carnap restated more explicitly his divergence from the Tractarian conception of philosophy and of logical syntax. Carnap's rejection of the conception of logic in the *Tractatus* is generally considered an essential step towards his tolerant view about language and logic, such as it is expressed in his "Principle of Tolerance". Indeed, the criticism of the *Tractatus* was essential for the view that a logical syntax is a matter of conventional choice. But Carnap's and Neurath's criticism of the *Tractatus* have to be placed into a broader context of the discussions of the *Tractatus* in the Vienna Circle since 1927. We will retrace the early reception of the *Tractatus* in the Vienna Circle, before looking at the criticism Neurath and Carnap addressed to central positions of the *Tractatus* in the early 1930s, at the moment when both developed their physicalist position.

In a first step, I will present a general overview of the early reception of the *Tractatus* in the Vienna Circle and of the main stages of the reading of that book within the Circle (section 2). In the two following sections (section 3 and 4) I will discuss Neurath's and Carnap's criticism of the *Tractatus* and place it within the context of this earlier reception.

2 Reading the *Tractatus*

The beginning of the reception of the *Tractatus*¹ is generally dated with a talk the mathematician Kurt Reidemeister gave in the Vienna Circle in autumn 1924, a talk mentioned by Schlick in his first letter to Wittgenstein.² But the interest in the *Tractatus* was already aroused earlier, despite the fact that the intense study of Wittgenstein's book within the Circle began only around 1924/25. After the Erlangen conference that Carnap and Reichenbach had organized in 1923 on Russell's logic and its application to epistemology, Carnap traveled for several weeks to the United States. In New York, he met several American mathematicians who told him about a new "mathematical philosophy" which, so they thought, was developing in England and the United States.³ In that context, the American mathematicians mentioned, among others, the *Tractatus* which had just appeared in its English translation the year before. As Carnap, together with Reichenbach and Schlick, was planning to edit a new journal on "exact philosophy", he was looking for potential authors for that publication. In this matter, he wrote Reichenbach about the philosophers connected to the new "mathematical philosophy", mentioning also Wittgenstein.⁴ Reichenbach forwarded the letter to Schlick, and the latter replied: "Wittgenstein, whose book is edited by Russell, lives here close to Vienna."⁵

Slightly later, Schlick wrote to Russell about the planned journal and mentioning that he wanted to ask Wittgenstein to write a paper for the journal (which apparently he never did ask). It seems that this first interest in Wittgenstein also triggered Carnap's first reading of the book. Indeed, Carnap read some parts of the *Tractatus* in Germany (Buchenbach), before his move to Vienna in 1926, but said later that he did not "very well understand" the book.⁶ In his *Intellectual Autobiography*, Carnap said about this first reading of Wittgenstein's treatise: "I found in it many stimulating and interesting points. But at that time I did not make the great effort required to come to a clear understanding of the often obscure formulations; for this reason I had not read the whole treatise."⁷

1 Wittgenstein 1922.

2 Schlick to Wittgenstein, Dec. 25, 1924.

3 See Limbeck-Lilienau 2010, pp. 94–96.

4 Carnap to Reichenbach, May 7, 1923.

5 Schlick to Reichenbach, July 14, 1923.

6 Carnap Papers RC 102-78-06 (Archives of Scientific Philosophy, Pittsburgh University).

7 Carnap 1963, p. 23.

After this first interest in Wittgenstein, the *Tractatus* gained once again focal attention due to Frank Ramsey and Hans Hahn. Indeed, Ramsey stayed for half a year in Vienna from March to October 1924.⁸ The mathematician Max Newman, then also in Vienna, introduced Ramsey to Hahn. Both met several times at the University and at the home of Wittgenstein's sister Margaret Stonborough.⁹ For several reasons, the young British philosopher must have attracted Hahn's attention. Since the 1900s, Hahn had been greatly interested in Russell's logic and in his philosophy of mathematics. Ramsey, just before his arrival in Vienna, had gone through the manuscript of the second edition of the *Principia Mathematica*.¹⁰ In Vienna, Ramsey was working on his long paper "The Foundations of Mathematics" (1925) where he used Wittgenstein's conception of tautologies to solve several of the problems of the *Principia*. He especially tried to reformulate logicism based on Wittgenstein's conception of logic as tautologies. Given Hahn's strong interest in Russell and his endorsement of logicism, it is quite plausible that Hahn and Ramsey discussed these matters and that Hahn noticed thereby the central importance of the *Tractatus* for logic and for the foundations of mathematics. In any case, once Ramsey had returned to England and had published there his paper, he sent it to Hahn and Schlick.

In the summer of 1924, Ramsey also met Schlick, a fact Schlick mentioned later in his first letter to Wittgenstein. At that time, Hahn was also preparing a seminar on Russell's *Principia Mathematica* for the winter term of 1924/25 (Ramsey could not attend as he left Vienna slightly before the beginning of that seminar). It must have been in this context that Hahn or Schlick asked their colleague Reidemeister to give a talk about the *Tractatus* in Vienna, at a time the Circle was just at its very beginning. Neurath noted later in his history of the Vienna Circle: "On the initiative of Hans Hahn, we began to read and to discuss carefully Wittgenstein's *Tractatus*."¹¹

The discussion of the *Tractatus* within the Vienna Circle can be divided into several phases: (1) the close reading of the *Tractatus* in the Circle (1925–27);

8 For a reconstruction of the introduction of the *Tractatus* in the Vienna Circle, emphasizing especially Ramsey's role in it, see Misak 2019, pp. 2–4.

9 Misak 2020, p. 166.

10 Misak 2020, pp. 172–173.

11 Neurath 1937/1981, p. 697. Though Feigl said that the initiative came from Reidemeister. He writes: "It was Reidemeister who in 1924, or perhaps 1925, suggested to us a project that was to become decisive in the development of the Circle's philosophical outlook. We read and discussed at length Ludwig Wittgenstein's *Logisch-Philosophische Abhandlung*", Feigl 1969, p. 60.

(2) the meetings of the “Tafelrunde” (“Round Table”),¹² i.e., meetings of a small group of members of the Vienna Circle with Wittgenstein himself (1927–28); (3) the increasing rejection of the positions of the *Tractatus* within the Vienna Circle due to the reception of Wittgenstein’s new ideas after his return to Cambridge (1929–31). During this third phase, the “Tafelrunde” was reduced to meetings of Wittgenstein with Waismann and Schlick only, at the exclusion of other former participants of the “Tafelrunde” (especially Carnap and Feigl).

We know relatively little about the close reading of the *Tractatus* in the Circle. While Carnap was still in Germany, Schlick had informed him that in the winter term 1925/26 his “Thursday-Circle” had begun to read the *Tractatus* “page by page”.¹³ From Carnap’s *Diary* entries, we know that this close reading continued for three semesters until February 1927 (Carnap joined these discussions only in May 1926).¹⁴ Relatively little is known about the issues raised in these discussions. We can get an insight into the impact of the *Tractatus* on some members from a letter of Schlick to Ernst Cassirer in 1927. Schlick wrote it shortly after the three terms of close reading of the *Tractatus*:

I went [...] through the school of the logic of Russell and Wittgenstein and since then, I apply to philosophical thinking the highest standards, so that I can only reluctantly force myself to read most of the philosophical production. I consider the *Tractatus logico-philosophicus* of Wittgenstein to be the most ingenious and most important achievement of contemporary philosophy. Unfortunately it is written in such a baroque style, that we needed in my philosophical circle (attended mostly by mathematical colleagues) three terms of joint reading before we began to understand [...], I have the firm belief that, through the stimulus of the new logic, philosophy reached a crossroad and that we approach Leibniz’ ideal of philosophy. We will have to draw a much sharper dividing-line than previously towards empty talk and questioning. We can quite unshakably rely upon the principle that all questions which have been correctly formulated can be answered in principle, either through logical analysis or through empirical assessment, and that “unsolvable problems” are only wrongly formulated questions.¹⁵

12 Wittgenstein called the group “Tafelrunde” in a letter to Schlick, Feb. 18, 1929. The meetings of the “Tafelrunde” took place separately from the meetings of the Vienna Circle, as Wittgenstein never was present in the latter.

13 Schlick to Carnap, Nov. 29, 1925 (trans. C.L.-L.).

14 Carnap 2022.

15 Schlick to Cassirer, March 30, 1927 (trans. C.L.-L.).

Naturally we cannot conclude from Schlick's enthusiasm that the other members of the Circle were similarly inspired by Wittgenstein, especially if we take into account the anecdote about Neurath's insistent objections against the "metaphysics" of the *Tractatus*.¹⁶ Nevertheless a certain number of central positions from the *Tractatus* were shared by most of the members of the Circle, including Neurath: the Tractarian view of logic, especially the view that logical truths are tautologies (TLP 6.1); the view that philosophy does not add any propositions to the meaningful propositions of the sciences (TLP 4.11 and 4.111); the view that the statements of metaphysics are meaningless (TLP 6.53) and the view that many of the apparently meaningful statements in philosophy are pseudo-propositions (TLP 4.1272). These claims seemed to have reached a consensus and they can be found in more or less similar formulations in the *Manifesto* of the Vienna Circle (1929).

First divergences about the *Tractatus* appeared openly at the time of the meetings of the "Tafelrunde" (1927–28). These meetings began with the first encounter of Schlick with Wittgenstein (in February 1927), followed by frequent meetings with Carnap, Feigl and Waismann.¹⁷ There are several reasons for these divergences. Wittgenstein, Ramsey and the members of the Vienna Circle tried to develop solutions to problems the *Tractatus* had left open and these solutions did not always reach consensus. Furthermore, at the time the members of the Circle began their direct discussions with Wittgenstein, the latter had already doubts about some of the positions defended in the *Tractatus*. This was due essentially to his discussions with Ramsey in 1923–24 and to the critical comments Ramsey had made about the *Tractatus*.¹⁸

A central conflict in the phase of the "Tafelrunde" meetings was the relation of logic to mathematics. Hahn and Carnap had followed Russell's logicist reduction of mathematics to logic. Some remarks of the *Tractatus* seemed to suggest support for logicism (for example proposition 6.2: "Mathematics is a logical method"). Ramsey's paper "The Foundations of Mathematics" (1925) tried to use Wittgenstein's conception of tautologies in order to extend it to mathematics. For Ramsey, mathematics was reducible to tautologies. Following Ramsey, the thesis of the "tautological character of mathematics" was adopted by the logicists in the Circle, so by Carnap and Hahn. It was proclaimed slightly later as a central thesis of the Vienna Circle in the *Manifesto*.¹⁹

16 Neurath confirms this anecdote in a letter to Carnap from June 16, 1945, published in Cat and Tuboly (2019: 640).

17 Feigl writes also about these meetings, see Feigl 1969, pp. 63–64.

18 See Ramsey 1923.

19 Carnap et al. 1929, p. 32.

But Wittgenstein himself had clearly distinguished in the *Tractatus* between the tautologies of logic and the expressions of mathematics, which he considered to be “equations”. He made it clear that equations are not tautologies (TLP 6.2). Furthermore, Wittgenstein explicitly rejected Russell’s logicist attempt to reduce natural numbers to classes (TLP 6.031).²⁰ So, contrary to Russell, Wittgenstein did not want a reduction of numbers to classes and contrary to Ramsey he did not want a reduction of mathematics to tautologies. These divergent views about the relation of logic to mathematics became clearly visible in the early meetings of the “Tafelrunde” (so in the summer 1927) and they were repeatedly discussed in the Vienna Circle.

In the third phase of the reception of the *Tractatus* (1929–31), the rejection of central theses of Wittgenstein’s book became more and more visible. Two critical strands developed now against the *Tractatus*: one coming from Wittgenstein and Waismann, the other from the physicalists.

On the one hand, Waismann stayed in close contact with Wittgenstein, who was now mostly in Cambridge. In their private conversations central positions of the *Tractatus* were revised or abandoned, as reported in Waismann’s notes.²¹ Waismann presented these revisions on several occasions in the Vienna Circle namely most prominently in a series of talks he gave in the Circle on the philosophy of Wittgenstein (1930)²² and slightly later through his so-called *Theses*. The *Theses* were a written comment on the *Tractatus*. They were extensively discussed in the Circle in 1931.²³ Carnap was strongly involved in these discussions of the revisions, both in the Circle and through his frequent private meetings with Waismann.²⁴

On the other hand, Carnap and Neurath developed in 1930 their physicalist position and in 1931, Carnap began to work on his *Logical Syntax of Language*. It is to be noted that the *Manifesto*, published in 1929, included multiple references to Wittgenstein all of which were positive and no criticism of Wittgenstein’s position was mentioned. But slightly later, in 1931, in Neurath’s and Carnap’s first papers on physicalism, central claims of the *Tractatus* were

20 On Wittgenstein’s view about logicism and classes, see Marion 1998, pp. 21–47.

21 McGuinness 1984.

22 This series of 7 talks were given in the Vienna Circle from May to July 1930. Carnap made extensive notes about the talks which have been preserved, Carnap Papers RC 102-76-10 (Archives of Scientific Philosophy, Pittsburgh University) and have been published in Stadler (2023: 332–44).

23 Waismann’s *Theses* are published in McGuinness 1984, pp. 233–261. The *Theses* were discussed in the Circle in February and May/June 1931, see the “Protocols of the Schlick Circle” in Stadler (2015, pp. 79–89 and pp. 97–107).

24 On Carnap’s discussions with Waismann, see Limbeck-Lilienau 2019.

strongly attacked.²⁵ A more extensive critical discussion of Tractarian positions was then included in the *Logical Syntax of Language* (1934).

In the context of the Vienna Circle, Carnap's and Neurath's criticism of the *Tractatus* seemed to be the most radical break with the *Tractatus*. But their criticism must be understood against the background of the revisions of the Tractarian positions formulated by Ramsey, Waismann and Wittgenstein before 1931. These revisions have introduced major changes to the original conceptions of the *Tractatus*. Carnap's and Neurath's criticism of the *Tractatus* cannot be sharply separated from these previous critical revisions of the *Tractatus*. In the next two sections, I can only focus on the criticism by the physicalists. For the revisions of the *Tractatus* in Wittgenstein's discussions with Waismann and its impact on the Circle, see Limbeck-Lilienau (2019 and 2023).

3 Neurath: against the *Tractatus*

Neurath and Carnap began to publish their papers on physicalism in 1931, which contained an implicit criticism of central theses of the *Tractatus*. Furthermore, at this moment, central positions of the Tractarian framework had already been changed, revised or abandoned due to discussions in the Circle and in its periphery. Carnap credited Neurath for his early and persistent objections to the *Tractatus*. It is difficult to reconstruct at which moment Neurath began to reject the *Tractatus* and at which moment he rejected which position; this is due to the fact that Neurath published his objections only in 1931 and later. Before that, Neurath did not mention Wittgenstein, with the exception of the positive remarks about him in the *Manifesto*. Carnap mentioned a certain number of Tractarian positions, which Neurath was "the first in the Vienna Circle" to criticize and to reject: the phenomenal language as the basic or primary language, the comparison of language and reality, i.e., the picture-theory of meaning, and the view of elucidation as pseudo-propositions.²⁶ Indeed these three aspects cover the main points Neurath would criticize in Wittgenstein's philosophy in his publications from 1931 onward. The first point, phenomenal language, is related to the post-Tractarian interpretation of elementary propositions which Wittgenstein had adopted in his paper on "Some Remarks on Logical Form" (1929), the issue of the picture-theory and of elucidations touched on the central issue of the "metaphysics" of the *Tractatus*. Indeed

25 Neurath 1931 and 1932; Carnap 1931.

26 Carnap 1931, p. 452 and 1934/1937, p. 283.

these “metaphysical” remnants in Wittgenstein’s philosophy were a recurrent worry for Neurath. I will first describe Neurath’s criticism in more detail, before situating it within the general discussion of the *Tractatus* in the Circle.

Neurath’s objections to the *Tractatus* were first formulated in two of his physicalist papers: “Physicalism” (1931) and “Sociology and Physicalism” (1932). Carnap acknowledged the importance of Neurath’s criticism in his own first physicalist paper.²⁷ Neurath’s criticism here and in other papers can be summarized in the following points: we do not need elementary propositions, but only empirically given protocol sentences; there is no need of a “phenomenal language” in order to formulate the protocol sentences, a physicalist language is enough; sentences are not justified through the confrontation of language with reality; Wittgenstein’s conception of philosophy as “elucidations” has to be rejected, as elucidations are a metaphysical and mystical element in his philosophy; and contrary to Wittgenstein’s claim, it is possible to speak about language and its logical form. This last point also meant that Neurath completely rejected Wittgenstein’s distinction between “showing” and “saying”.

In a long passage on Wittgenstein in “Sociology and Physicalism”, Neurath expressed his main objections in a condensed form:

In his “elucidations”, which may also be characterized as “mythological introductory remarks”, Wittgenstein seems to be attempting to investigate, as it were, a pre-linguistic state from the point of view of a pre-linguistic stage of development. These attempts must not only be rejected as meaningless; they are also not required as a preliminary step towards unified science. One part of language can, to be sure, be used to discuss other parts; but one cannot make pronouncements concerning language as a whole from a “not yet linguistic” standpoint, as Wittgenstein and certain representatives of the “Vienna Circle” seek to do. A part of these endeavors, although in a modified form, may be suitably incorporated into scientific work. The rest would have to be discarded. Nor may language as a whole be set against “experience as a whole”, “the world” or “the given”.²⁸

Let us look first at what appeared to Neurath as the metaphysical rests in the *Tractatus*, especially the conception of philosophy as an activity of “elucidation” before we address his objections against elementary propositions.

²⁷ Carnap 1931. This same paper triggered a violent reaction of Wittgenstein, including the accusation of plagiarism, see Hintikka 1993.

²⁸ Neurath 1932/1959, p. 285.

Though the Tractarian framework was already strongly transformed through the discussions in the Circle, his objections pushed the transformation of the Tractarian framework one step further. This was the case both concerning the conception of logical syntax, as well as concerning elementary propositions.

What were Neurath's objections against Wittgenstein's metaphysics of "elucidations"?

Neurath explicitly emphasized Wittgenstein's role in the rejection of metaphysics and his influence on the Circle's anti-metaphysics. He underlined that logical analysis was a means to get rid of meaningless metaphysical "pseudo-propositions", as Wittgenstein had stated in the *Tractatus* (TLP 6.53). Indeed, Wittgenstein said there that "the right method in philosophy would be this: To say nothing except what can be said, i. e., the propositions of natural science", while all other statements would be relegated to meaningless metaphysics. Despite this positive reference to Wittgenstein's "right method", Neurath rejected Wittgenstein's conception of "elucidations" and his distinction between what can be said and what can only be shown. The meaningful propositions, that is, the propositions of the natural sciences say something (about facts), while at the same time they show the logical form of the facts because they share that form with them. The central Tractarian claim that we cannot speak about the logical form (of propositions and language) was rejected by Neurath. Several reasons may have motivated this.

Since the beginning, Neurath seemed to have rejected the picture theory of meaning. He rejected the view that "language pictures reality"²⁹ and therefore also the Tractarian position that language and reality share a logical form. Furthermore, he thought that linguistic expressions (written signs or sounds) are facts like any other physical fact. It is therefore possible to speak about them like about any other physical fact. It is also possible to speak about the form of sentences no less than it is possible to speak about the form of tables. For Neurath, there was, therefore, no need for inexpressible logical forms and philosophical "elucidations" which aim to "show" the inexpressible. Wittgenstein would agree that we can speak about propositional signs (written signs or sounds) as well as about their configuration. But for Wittgenstein, it was not possible to speak about what a sentence shared with the fact it represented, namely its logical form. In a certain sense, this was also impossible for Neurath, simply because there was no logical form a sentence shared with

29 If we follow Rose Rand's famous overview of the "Development of the Theses of the Vienna Circle" relative to the *Tractatus* (Stadler 2015, pp. 143–145), Neurath rejected the thesis "Language pictures reality" already before the reading of the *Tractatus*.

facts. For him, however, language speaks only about physical facts and their physical features and there was nothing we could not speak of.

Neurath's complete rejection of the picture theory and the implications it had for the saying/showing distinction meant also a complete rejection of Wittgenstein's theory of meaning and representation. Despite Neurath's radical rejection of this theory, it was not clear what should replace such a theory. Neurath indicated only in a very fragmentary way that a kind of behaviorist theory of language was sufficient to explain how language describes and represents something, how one sentence can be said to have the same meaning as another, or how meaningful sentences can be distinguished from meaningless ones.

Naturally Wittgenstein's picture theory and his theory of logical form was also at the center of his conception of elementary propositions. In the Vienna Circle, Hans Hahn had attacked the idea that logical analysis led to elementary propositions which cannot be further analyzed into simpler propositions.³⁰ Wittgenstein's idea was that an elementary proposition is composed of names (primitive signs) which designate simple objects (TLP 2.02). The configuration of such names in a sentence is a picture of a state of affairs. Once we get to the names which designate simple objects, the analysis stops. In his paper "Some Remarks on Logical Form" (1929), Wittgenstein claimed that elementary propositions should reflect the basic structure of the phenomena (contrary to the position in the *Tractatus*). This interpretation of elementary propositions as propositions about the phenomena was extensively discussed in the Circle. As the logical form of elementary propositions was supposed to reflect the structure of the phenomena, an analysis of the phenomena was an essential task. For Neurath, there was no need for such a task. There was no need to analyze complex propositions into propositions which immediately pictured states of affairs. The rejection of the picture theory implied also a rejection of such an analysis into elementary propositions, i.e., pictures of states of affairs. Furthermore, the basic empirical statements should not be formulated in a phenomenal language, but in a physicalist language speaking about observable objects and their properties.

Neurath's criticism of a phenomenal language was certainly essential for Carnap and his abandonment of a phenomenal language. But it is important to note that at the time Neurath published his criticism of a phenomenal language, Wittgenstein had already abandoned that idea which he had initially

³⁰ Hahn in a discussion in the Vienna Circle on Feb. 5 and 12, 1931, see Stadler, 2015, pp. 80–83.

formulated in his 1929 paper. Indeed, in the *Philosophical Remarks* (finished in April 1930) he wrote:

I do not now have phenomenological language, or 'primary language' as I used to call it, in mind as my goal. I no longer hold it to be necessary. All that is possible and necessary is to separate what is essential from what is inessential in our language.³¹

At the time, Carnap was still defending the view that the basic empirical statements are to be formulated in a phenomenal language. It is certain that Neurath's criticism of a phenomenal language had a strong effect on Carnap, but such a language was not a dogma anymore in the Circle.

How can Neurath's criticism of Wittgenstein be evaluated in the general context of the discussions of the *Tractatus*? Like the Wittgensteinians and like Carnap, Neurath thought that the task of philosophy consisted in the development of a logical syntax. But at the same time, he completely rejected the picture theory, a theory Wittgenstein, Waismann and Carnap still accepted in one form or another. Neurath's vehement rejection of the picture theory had two consequences: there was for him no need to investigate the nature of elementary propositions, which were thought to be pictures of states of affairs and were thought constitutive of all propositions. Firstly, one could begin with the empirically given statements of the sciences instead of looking for elementary propositions. Secondly, the logical syntax could speak about language and its form. There was no need for Wittgenstein's distinction between what could be said and what could only be shown. There was no need for mysterious "elucidations". There was no difference between the statements of science and the statements of logical syntax. Logical syntax was just one branch of the sciences with no special status. The mystical aspect of elucidations disappeared.

But this different conception of a logical syntax did not imply logical pluralism for Neurath, nor did he accept a meta-language. It seems that for him there still was one logical syntax. And despite the fact that the logical syntax described language and its forms, this had to be done in the object language itself.

31 Wittgenstein 1984, p. 51.

4 Carnap: Overcoming the *Tractatus*

Carnap's view of the *Tractatus* at the time he was working on the *Logical Syntax* was shaped both by his physicalist alliance with Neurath and by his frequent discussions with Waismann. His alliance with Neurath in the common project of physicalism led to the rejection of the picture theory and of a phenomenalist basis for protocol sentences. His extensive discussions with Waismann led to the project of a new logical syntax and to the insight that the version of logical syntax presented in the *Tractatus* had to be radically revised.

I will briefly describe here the impact of his conversations with Waismann, which are described in more detail in Limbeck-Lilienau (2019 and 2023). The *Tractatus* had claimed that elementary propositions must be independent one from another, which meant that from one elementary proposition we cannot infer another one (TLP 6.3751). Or in conversations with Waismann, Wittgenstein began to strongly doubt this claim, especially due to the so-called "color exclusion problem": if this spatio-temporal spot is red, this implied that it cannot for example be green. Therefore, color attributions cannot be elementary propositions, but the problem could generalize to any other kind of property. But if elementary propositions were not independent, then the logical syntax of the *Tractatus* would need substantial revision.³² In conversations with Waismann, Carnap was thinking about possible revisions of such a logical syntax. Wittgenstein himself had proposed such a revision in his 1929 paper. Not only the need for a new logical syntax was felt, but further questions about the nature of a logical syntax were raised: Was there only one possible syntax or several? And how could the choice of syntax be justified? Was it possible to freely choose a syntax and freely lay down its rules?³³ These questions were the consequence of a substantial revision of the Tractarian position as reflected in Waismann's *Theses* and their discussion in the Circle. Discussions of these questions mainly took place between 1929 and 1931, before Carnap left Vienna for Prague.

32 Due to the dependence of elementary propositions, a conjunction of propositions such as "X is red" and "X is green" is impossible, although "X is red and X is green" is not a contradiction. Therefore, a syntax is needed which excludes such impossible combinations, a syntax which reflects the internal dependence of elementary propositions. For a detailed analysis of the need for a new logical syntax given the dependence of elementary propositions, see Waismann (1939/40).

33 These questions were for example raised in the meeting of the Vienna Circle from Feb. 12, 1931, see Protocols of that meeting in Stadler 2015, pp. 81–85, see also Limbeck-Lilienau 2023.

After 1932, once in Prague, Carnap repeatedly discussed Wittgenstein's Tractarian positions. He thought that the *Tractatus* embodied two forms of "absolutism" which are to be rejected: on the one hand the absolutism of elementary propositions, the "given" and a phenomenal language; on the other, the absolutism of a single language.

The first absolutism was initially mentioned by Carnap in "On Protocol Statements" (1932/33). The rejection of this absolutism was linked to Neurath's rejection of the picture theory and of the phenomenal basis for protocol sentences. The second absolutism was most extensively discussed in the *Logical Syntax*.³⁴ It was not only connected to the discussions with Neurath, but also to his long conversations with Waismann about the nature of syntax in the *Tractatus*. Carnap's view of the *Tractatus* at the time he was working on the *Logical Syntax* can be seen as a synthesis of these two strands of criticism coming respectively from Neurath and from Waismann. I will briefly discuss these two absolutisms, before situating Carnap's position within the general discussion of the *Tractatus* in the Circle.

Since at least 1929, when Wittgenstein published "Some Remarks on Logical Form", Carnap had shared with the latter the project of an analysis of the logical form of elementary propositions formulated in a phenomenal language.³⁵ In the Vienna Circle the question of the logical form of such propositions was repeatedly discussed. Carnap defended the view that elementary propositions had a relational form, while Wittgenstein and Waismann remained skeptical about this claim.³⁶ Still in his first paper on physicalism, Carnap thought that the form of a protocol sentence depended on the form of experience and of "the given". The protocol language was still conceived as a "phenomenal language" or a "language of experience".³⁷ This position followed Wittgenstein's suggestion from 1929, that an analysis of the phenomena would lead to the logical form of elementary propositions. It is only in his response to Neurath's conception

34 Carnap speaks about the "absolutism of basic propositions" in Carnap, 1932/33, p. 228. In the *Logical Syntax* he discusses "Wittgenstein's absolutist conception of language", see Carnap 1934/37, § 52 and § 73. Awodey and Carus (2009) describe these absolutisms as "Wittgenstein's prison".

35 In Rose Rand's survey about the *Tractatus*, Carnap initially endorsed the view that there are elementary propositions ("Atomsätze"), while Neurath always rejected it, see thesis 13 in Rand's survey, Stadler 2015, p. 144.

36 For the relational option, see Rose Rand's survey, thesis 16, endorsed by Carnap during the *Tractatus* reading: "The atomic sentences have the form of a relation, e.g. the relation of memory between two names, which designate experiences." For Wittgenstein's skepticism about that option, see McGuinness 1984, p. 42.

37 Carnap 1931, pp. 438–439.

of protocol sentences, “On Protocol Sentences” (1932/33), that Carnap rejected elementary propositions and abandoned the project to analyze their form in relation to the structure of the phenomena. In direct reference to Neurath’s criticism, Carnap rejected now the “absolutism of the ‘given’, of ‘experience’, of the ‘immediate phenomena’”, which took the form of an “absolutism of basic propositions [“Ursätze”] (‘elementary propositions’, ‘atomic propositions’).³⁸ Here, Neurath’s criticism of the *Tractatus* and of a phenomenal basis was an essential factor in Carnap’s abandonment of elementary propositions.

Let us look at the second absolutism. In the *Logical Syntax*, Carnap had analyzed more extensively his disagreement with the *Tractatus*. His main focus there was “Wittgenstein’s absolutist conception of language”³⁹ and more generally his difference to Wittgenstein’s conception of syntax. Before we look at these differences, it is important to emphasize that Carnap also expressed his agreement with Wittgenstein on essential points. He agreed with Wittgenstein that philosophy was the analysis of language and he agreed that such an analysis was a purely formal logical analysis, that is, an analysis of language was not supposed to take into account the sense (“Sinn”) or the reference (“Bedeutung”) of expressions.⁴⁰ But besides this agreement, Carnap mentioned in § 73 of the *Logical Syntax* two theses of Wittgenstein that he vigorously criticized and rejected: (1) there cannot be propositions about the logical form of sentence, therefore no syntax can be formulated and (2) philosophy consists in “elucidations”, that is, in pseudo-propositions.

Carnap’s argument in the *Logical Syntax* against the second thesis was that “elucidations” did not permit a purely syntactic treatment of logic. The thesis about “elucidations” still blended the project of a logical syntax with considerations about the meaning of expressions. For Carnap, “elucidations” were still connected to considerations about the “sense” (“Sinn”) of expressions. Carnap made special reference here to Schlick’s interpretation of Wittgenstein’s conception of philosophy as an “activity of giving meaning”.⁴¹ In his programmatic paper “The Turning point in Philosophy”, Schlick had defined philosophy as “a system of acts” which consisted in the “activity through which the meaning of statements is revealed or determined”.⁴² For Schlick “meaning” is revealed through “acts of verification”.⁴³ Carnap objected that logical syntax could be

38 Carnap 1932/33, p. 228.

39 Carnap 1934/37, § 52.

40 Carnap 1934/37, § 73.

41 Schlick 1930, p. 56. Wittgenstein’s view as stated in the *Tractatus* was that “Philosophy is not a theory but an activity”, namely “the logical clarification of thoughts” (TLP 4.112).

42 Schlick 1930, p. 56.

43 Schlick 1930, p. 55.

treated in a purely formal way once this reference to mysterious “elucidations” and acts of “giving meaning” was abandoned. Only when these “elucidations” were abandoned could a logical syntax be treated as a purely formal calculus. And once syntax was conceived as a purely formal calculus could it be the object of a conventional choice. Naturally, it is highly questionable that Schlick’s interpretation of philosophy as acts “giving meaning” is an accurate representation of Wittgenstein’s position. But besides this, Carnap thought that only the rejection of “elucidations” permitted a free choice of logical syntax and therefore the abandonment of Wittgenstein’s absolutist conception of language.

We saw that Carnap was involved in the discussions which led Wittgenstein and Waismann to question the Tractarian view of a logical syntax and to consider different syntaxes. Nevertheless, it is not completely clear to what degree Carnap was aware of this. In the *Logical Syntax* he said that there is “a multiplicity of possible languages” while “Wittgenstein speaks always about ‘the’ language”.⁴⁴ So, at the time of the *Logical Syntax*, he thought there was a strong contrast between his logical pluralism and Wittgenstein’s “absolutism” about language. But in the “Preface” to the *Logical Syntax*, Carnap mentioned that “in opposition to Wittgenstein’s former dogmatic standpoint, Professor Schlick now informs me that for some time past, in writings as yet unpublished, Wittgenstein has agreed that the rules of language may be chosen with complete freedom.”⁴⁵ At least at the time of the publication of the *Logical Syntax*, Carnap was aware that Wittgenstein had abandoned his absolutism about language. Carnap’s *Logical Syntax* is certainly a rejection of the Tractarian view of logic, but it is much less a rejection of the view Wittgenstein (and Waismann) had around 1930. It can be seen as synthesis of Neurath’s criticism of the *Tractatus* and the revisions of the Tractarian position in the Wittgensteinian wing of the Circle.

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44 Carnap 1934/37, § 67.

45 Carnap 1934/37, p. xvi. Uebel (2009) analyzes how Schlick tried to remind Carnap about earlier discussions of tolerance by Wittgenstein and Waismann. Uebel also discusses whether Carnap may have been influenced by these earlier discussions mentioned to him by Schlick but thinks that he was not.

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Isotype, Logical Empiricism, and the Scientific World-Conception

Christopher Burke and Günther Sandner

Abstract

This chapter explores connections between Isotype – the work in visual education directed by Otto Neurath – and Logical Empiricism, and it examines Isotype in the context of the Vienna Circle’s Scientific World-Conception. In particular, the “picture language” of Isotype is compared to the so-called “picture theory” of meaning contained in Wittgenstein’s *Tractatus Logico-Philosophicus*, a text which was examined in great detail during Vienna Circle meetings attended by Neurath. The Circle’s debate about “physicalist” language provides an illuminating context for considering differences in Neurath’s approaches to verbal language and to pictures. As an applied method of educational, graphic design, Isotype reflects the pragmatic and political dimensions of the Scientific World-Conception. Specific examples of Isotype statistical graphics are examined to assess the claims made for their factual accuracy.¹

During precisely the same period that Otto Neurath was active in discussions of linguistic philosophy at the Vienna Circle (1925–34), he also directed the development of a “picture language” (Isotype) at the Social and Economic Museum of Vienna. It may be illuminating to explore parallels between these two areas of his work; Neurath himself considered “cross-connections” to be fruitful. A related example is provided by Elisabeth Nemeth’s examination of the connections between visual education and Neurath’s economic theories.² But how does Isotype relate to the Scientific World-Conception (as defined in the Vienna Circle manifesto of 1929) and to Logical Empiricism?

These areas have often been considered separately, both by contemporaries and by subsequent researchers. Philosophers of science were not generally

1 Citations in this chapter are taken from published English translations where possible; all other translations are by Christopher Burke.

2 Nemeth 2019.

interested in visual matters:³ Andreas Roser has pointed out the prejudice that “philosophy has to do with concepts not pictures”, and that consequently “sketches, pictograms or drawings are irrelevant in terms of the logic and philosophy of language”.⁴ Similarly, philosophical questions rarely play a role in the theory and history of graphic design. Nevertheless, literature about Neurath and Logical Empiricism frequently formulates the almost self-evident assumption that pictorial language and Neurath’s philosophy of science are, as it were, two sides of the same coin.

But, if this is so, then why did Otto Neurath’s colleagues at the Social and Economic Museum have little or nothing to do with Logical Empiricism: for example, Gerd Arntz, Rudolf Modley, or Friedrich Bauermeister, to name only a few?⁵ Marie Reidemeister, a key figure at the Social and Economic Museum as the principal “transformer” of scientific data into graphic form – and Neurath’s confidante & later (third) wife – was not permitted to attend meetings of the Vienna Circle, whereas Olga Hahn (Neurath’s second wife from 1912–37), as a mathematician and logician, was a member. No other member of the Vienna Circle was seriously interested in the Vienna Method of Pictorial Statistics (as Isotype was called in its initial phase).⁶

Neurath himself lamented this separation. In 1939 he wrote to Susan Stebbing, the British philosopher who would become president of the Isotype Institute in Oxford, asking her to review his two books *International Picture Language* and *Basic by Isotype*: “I seek always a reviewer for these books, but nobody of our people who are interested in Logic are interested in Visual Education and ISOTYPE too. You are the first; I am very glad that you are full of educational ideas.”⁷ This hints at the centrality of education as the aim of Isotype work, which separates it from pure philosophy.

3 A significant exception is Daston/Galison (2007).

4 Roser 1996, p. 12.

5 During 1932, Neurath discussed philosophical ideas with junior co-workers at the Vienna Museum, Marie Jahoda, Oskar Umrath, and Rudolf Brunngraber, and he organized seminars at the end of the working day (which Rudolf Carnap also attended) to attempt to render Freud’s theories in physicalist language. See Jahoda (1982), and Carnap (1963), p. 58.

6 Carnap (2022) recorded in his diary (8 July 1933) that Neurath, despite international success, felt “like a poor little lamb [*armes Haserl*] and lonely, because he cannot talk about his things, especially picture statistics, with a real expert”.

7 Neurath to Stebbing, 8 April 1939 (Otto Neurath Nachlass, Vienna Circle Archive, Haarlem [ONN]). In an earlier letter (9 March 1935, ONN), when *International Picture Language* was in preparation, Neurath told Stebbing that it would show “the connection between my logical standpoint and my promotion of the picture language as a helping language for education and information”.

1 Context and Characteristics of Isotype

The Vienna Method of Pictorial Statistics originated at a time when visual communication began to have a stronger influence in public life. Neurath predicted: "Our age will one day be called the age of the eye."⁸ It was also the age of democratization (in particular universal suffrage from 1918 onwards), accelerated industrial development, the beginnings of the welfare state, widespread general education, and the increasing interlocking of economy and politics with scientific expertise. Visualization came to be recognized as an effective method of conveying information, both in print media and in film. "Modern man is first of all an *ocular being*", declared Neurath:

Advertising, the educational billboard, cinema, illustrated newspapers and magazines are broadly responsible for the education of the masses. Even those who read many books are inspired more and more by images and series of images. ...

Beyond that, *image-based pedagogy is a means to open otherwise unattainable educational possibilities for less educated adults who tend to be more susceptible to optical stimulation, and for disadvantaged youth.*⁹

Neurath's second point here makes clear the Vienna Method's context in inter-war "Red Vienna", where Social Democratic policy dominated with an emphasis on housing, health and social hygiene, education and schooling, and last but not least, worker education. The aim of the Vienna Method of Pictorial Statistics was to educate workers about social and economic connections: statistical charts were important in this respect, although the Social and Economic Museum also displayed other kinds of charts (biology, accident prevention, etc; see Figure 1). As Otto Neurath made clear in numerous texts written during this period, he saw the Vienna Method as part of a larger historical movement to enable the working class to safeguard their political interests through education. The working class was disadvantaged by the prevailing educational system but, as Neurath repeatedly asserted, its members were particularly responsive to statistical correlations and visual education. Through the factual and quantitative images of the Vienna Method, the workers could quickly catch up in terms of education, and even achieve a higher level of argumentation in some subjects than the bourgeoisie.¹⁰

8 Neurath 1930/1, p. 154.

9 Neurath 1931d, p. 115 (italic in original).

10 Neurath 1929, p. 139.



FIGURE 1 Accident prevention warning made at the social and economic museum of Vienna, c.1927

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Around 1935, after a core team from the Vienna Museum relocated to the Netherlands, the method was renamed Isotype (International System of Typographic Picture Education). This name can also be interpreted via Greek as meaning “always using the same type”, reflecting the central rule of Isotype that pictures should not be increased in size to show an increase in quantity, but instead pictogram units should be repeated in greater numbers, at the same size (Figure 2). These pictograms should be simple and self-explanatory, not expressing more than necessary in themselves. (This hints at an influence of logical notation on Isotype, which will be explored below.)

In addition, Isotype was characterized by a utopian perspective, with a clearly defined goal to develop an “international picture language”. For Neurath, Isotype was always something unfinished, beyond the rules and practices that had been established. It never had a fully-articulated theory – at most a theoretical framework – and neither was there an Isotype curriculum that could be easily learned and applied by anyone. Working with Isotype required competence and aptitude to apply its principles to different topics and contexts. Long training and a well-coordinated team of varied specialists was necessary.

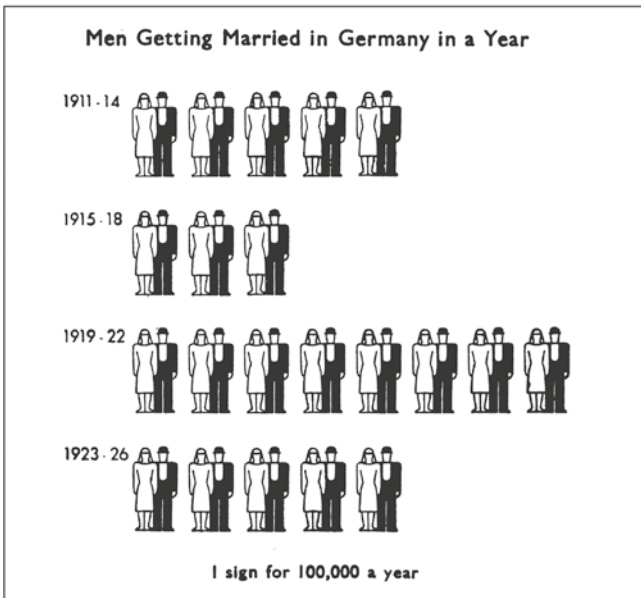
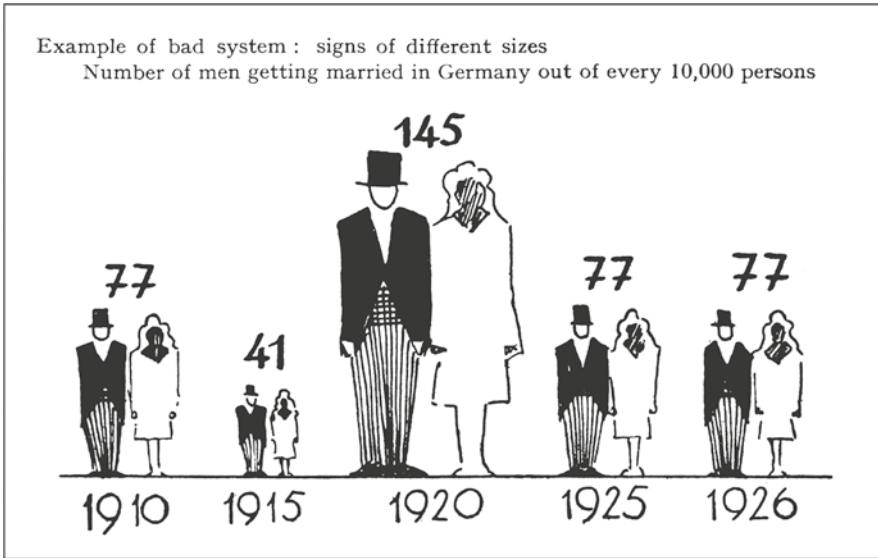


FIGURE 2 “Bad” and “good” methods of depicting the same data, according to Otto Neurath in his book *International Picture Language*, 1936. Neurath’s objection to the first method is that the viewer is not sure what to compare visually: the height of the pictures or their area. The second example shows the first principle of Isotype: “A sign is representative of a certain amount of things; a greater number of signs is representative of a greater amount of things.” (P. 73)

2 Isotype and the Scientific World-Conception

If we consider Isotype in relation to the manifesto “The Scientific Conception of the World: the Vienna Circle” (1929), written mainly by Neurath and Rudolf Carnap, significant affinities are apparent in the orientation towards empiricism, logic, and anti-metaphysics, but also in the emphasis on collective work and popular education. Isotype is implicated (along with “physicalist” verbal language) in the Vienna Circle’s search for “a neutral system of formulae, for a symbolism freed from the slag of historical languages”.¹¹ Ellen Lupton, one of the few graphic design historians to seriously address the theoretical background of Isotype, drew from this the conclusion that “Isotype is a popular version of logical positivism”.¹² It should be observed, however, that Neurath himself never stated the connection so explicitly.

The Vienna Method of Pictorial Statistics is not mentioned specifically in the manifesto, despite the fact that it had reached a certain point of maturity by the time that text was written (the foreword is dated August 1929). In the appended bibliography of writings by Vienna Circle members, Otto Neurath is listed as “director of the Social and Economic Museum” and his forthcoming book *Bildstatistik nach Wiener Methode in der Schule* (Pictorial statistics by the Vienna Method in school) is given the optimistic date of 1929 (described then as “in preparation”, and finally published in 1933). Neurath described it as follows:

Contains pointers to the affinity between the Scientific World-Conception and the transformation of the present; the connection between statistics as numerical description, their pictorial representation, and the pursuit of a systematic “grid” [*Raster*] by means of the Scientific World-Conception.

This makes clear that the Vienna Method of Pictorial Statistics aligned with a certain political and activist slant in the Vienna Circle manifesto. No doubt Neurath considered it one of the “intellectual tools” which had to be fashioned “for everyday life, for the daily life of the scholar but also for the daily life of all those who in some way join in working at the conscious re-shaping of life”.¹³ Isotype was already proving itself as such a tool, applied to educational exhibitions and publications, and in Viennese school curricula.¹⁴ In this sense,

11 Neurath 1929a, p. 306.

12 Lupton 1986, p. 49.

13 Neurath 1929a, p. 305.

14 See Wulz 2023.

Isotype is an enactment of the Scientific World-Conception, which Donata Romizi has characterized as “not so much a theory but rather an epistemological attitude” that became political through “public manifestation”. Romizi adds that the distinction between “political” and “party political” is crucial in this respect.¹⁵

Yet some hints toward a party-political agenda for the Scientific World-Conception are contained in an article published under Neurath’s name alone in the Social Democratic newspaper *Arbeiter-Zeitung*, with exactly the same title as the manifesto: “Wissenschaftliche Weltauffassung”, but without the subtitle “Der Wiener Kreis”; indeed, neither the Vienna Circle, nor any of its members are mentioned in Neurath’s article (nor is the Verein Ernst Mach, which Neurath had developed as the public face of the Circle). Appearing on 13 October 1929, the newspaper article (Figure 3) was published immediately after the manifesto and the Prague Conference where the Scientific World-Conception was introduced. While the content is similar to that of the manifesto, one immediately notices that a different audience is being addressed. Neurath stressed above all the importance of the modern workers’ movement and the consciousness it could develop in opposition to metaphysics and theology.¹⁶

Again, in this essay, pictorial education is not explicitly mentioned, but Neurath pointed out that the workers’ movement had created an impressive system of workers’ education, and that many representatives of the Scientific World-Conception contributed to it. The Social and Economic Museum of Vienna was naturally a prime example of this and the Vienna Method of Pictorial Statistics is implicated here:

In this, enlightenment about people and things, about connections of all sorts, plays a decisive role. Excitation of enthusiasm and the portrayal of glorious goals do not come to the fore, rather much more the purely factual description of that which is.

Pictograms of “people and things” were essential components of Isotype, and were arranged in its charts to convey “connections of all sorts” – social, economic, and biological.

15 Romizi 2012, pp. 210, 234.

16 Neurath also wrote articles about the Vienna Method from 1925 for publications of the Social Democrats and of the labour movement.

The questions raised by Neurath in the *Arbeiter-Zeitung* were not philosophical but political and economic:

We are not concerned with fathoming secrets about the spirit of the world [*Weltgeist*], nor with ascertaining man's place in the cosmos, but rather with the behaviour of organised masses of people and its influence on happiness and unhappiness, on living conditions and length of life.¹⁷

Happiness was an enduring theme for Neurath, although it was never directly addressed in Isotype, which excluded emotion from its pictographic style and depicted objects instead of concepts; "living conditions and length of life", on the other hand, were frequent subjects of Isotype charts, for which data was readily available (Figures 13 and 14). Other questions Neurath raised as relevant to the Scientific World-Conception were: "How do [financial] crises arise? How do epidemics arise? When millions of people are killed in wars, is there then no unemployment within the capitalist order or does it arise independently of the number of people available?"¹⁸ (Figure 11). Similar questions were posed in an article Neurath wrote in the previous year about "Colonial-political enlightenment through picture statistics":

The worker has an interest in knowing how war and peace can depend on the oil conflict between Standard Oil and the Shell group, and wants to understand how it happens that the Soviet Union makes its oil available to the USA when it stands against the capitalist world in other ways. ... It is not sufficient to learn about the interdependencies, one must know how to assess their relative implications, the extent of the individual measures and movements!¹⁹

Such issues were addressed in the 100 pictorial charts about many interrelated subjects of population, energy supply, industrial production, and trade in the atlas produced at the Social and Economic Museum, *Gesellschaft und Wirtschaft* (Society and economy, 1930; Figures 4 and 5).²⁰

17 Neurath 1929b, pp. 345–346.

18 This question may have been praying on Neurath's mind at that very time due to the problem of making a chart about unemployment for *Gesellschaft und Wirtschaft* (see below and fig. 11).

19 Neurath 1928, pp. 128–129.

20 The cover and title page list the subjects shown as "forms of production, social order, cultural stages, and standards of living".

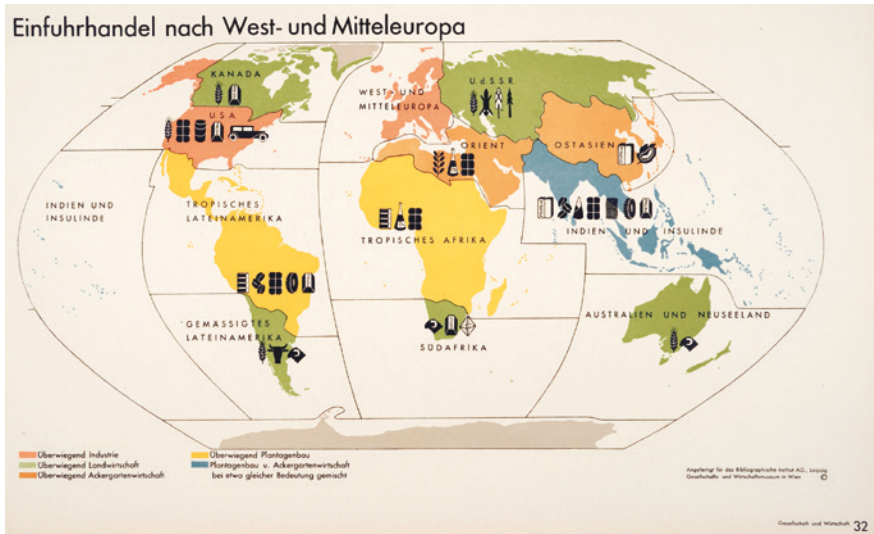


FIGURE 4 “Import trade to Western and Central Europe”, *Gesellschaft und Wirtschaft*, no. 32, 1930

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in the manifesto, but it was ridiculed by other Circle members. “Physicalism” began to be used by Neurath and Carnap simultaneously in early 1931 as a description of the Circle’s approach to language;²³ and Neurath was perhaps first among the “inner circle” to use “Logical Empiricism” in print at the end of 1931 (he began to use it more consistently from 1935, after the Circle had already begun to dissipate, Neurath himself having fled Vienna the previous year).²⁴ Considering his talent for inventing such terminology, it is remarkable that Neurath was reticent about linking Isotype specifically with any of these terms. Isotype was differentiated from the verbal discourse of scientific

23 Uebel 2007, p. 170.

24 Hans Hahn teased Neurath about *Einheitswissenschaft* with the play on words “Einheits-Wissenschaft” (heated-up science); see letter Neurath to Carnap, 16 June 1945, in Cat and Tuboly (2019), p. 640. Neurath first mentioned “Logical Empiricism” in “Physikalismus”, *Scientia* 50/1931, p. 297 (translated as Neurath 1931c). Neurath (1946, pp. 500–501) explained: “Schlick and others have been fond of ‘Radical Empiricism,’ a term used by William James. ... I succeeded in getting the word ‘radical’ dropped entirely, as far as I can see; but I have been less successful in promoting ‘Logical Empiricism’ instead of ‘Logical Positivism,’ a term much liked by many friends and critics. Not being a pedant I can bear that.” Both Carnap and Neurath resisted “Logical Positivism” due to its evocation of

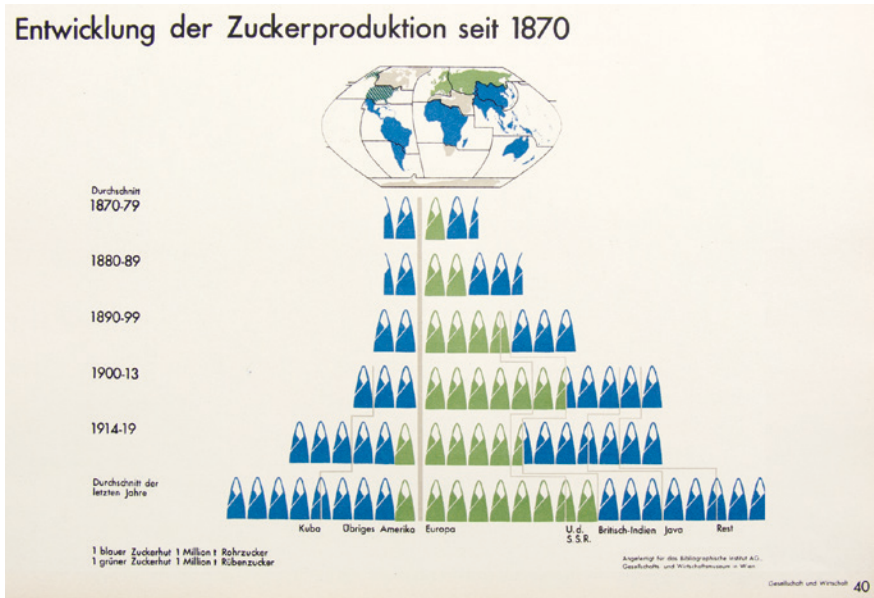


FIGURE 5 “Development of sugar production since 1870”, *Gesellschaft und Wirtschaft*, no. 40, 1930

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philosophy by the element of graphic design: it was an applied, practical activity, not principally theoretical.

Isotype itself is not a science but a method of visual education. However, as Neurath repeatedly emphasised, it should be based on science: its object was to transform scientifically collected, verified data into images; or, as Neurath himself put it in his book *International picture language*, “turning the statements of science into pictures”.²⁵

Comte and earlier positivism; yet a cue had been given in the Vienna Circle manifesto, which stated: “We have characterised the *scientific world-conception* by two features. First it is *empiricist and positivist*” (Neurath 1929a, p.309). See Stadler (1991); see also Philip Frank’s reflection on terminology in *Erkenntnis* 5, 1935 (p. 4), where he neglects to name Neurath as a representative of the “antimetaphysical movement” in Central Europe. It should be pointed out that “Logical Empiricism” was coined by Eino Kaila in 1926 (Uebel 2013, pp. 60, 72).

²⁵ Neurath 1936a, p. 8.

3 Key Texts about Isotype

Alongside numerous, short essays, *International picture language* (1936) is the second of three lengthy texts written by Neurath about visual education. It is partly an English adaptation of his first book on the subject *Bildstatistik nach Wiener Methode in der Schule* (1933). The third text is “Visual education: humanisation versus popularisation” (1945), a book he wrote at the end of his life, which was published posthumously.²⁶ Many clues can be found in these writings about connections with other areas of Neurath’s work.

Bildstatistik nach Wiener Methode in der Schule is not only about the experience of introducing pictorial statistics into school curricula; it is also a detailed document of the intentions and working methods developed during the formative years of Isotype. Neurath aligned it firmly with the anti-metaphysical standpoint of the Vienna Circle, and suggested that it was a kind of return (on a higher level) to the beginnings of language:

What a triumph it was when we freed ourselves from the bounds of picture-script; what a triumph when language, in its flexibility and multi-formity, adapted itself to all the demands of scientific work, as we learned to master this instrument of logic. Of course, liberation from pictures also led to mistakes, into the realm of the senseless. Nominalization gave rise to ever newer problems. In particular, the German language leads us into such metaphysical errors: it allows extensive discussions about “das Nichts, welches nichtet” (Heidegger), about “Being”; as if “Being” could be used in a sentence in the same way as sword or table. Pure picture-script does recognize a sword and a table, but not Being.²⁷

In this text, Neurath frequently referred to charts made with the Vienna Method as *Merkbilder* – “memorable pictures” that give a “rough orientation”, which may be supplemented by more detail on closer inspection.

26 For Neurath (1945, p. 257), humanization was “a procedure from the simplest to the most complicated”, whereas popularization was the opposite, a top-down process of simplifying complexity.

27 Neurath (1933), p. 269. Neurath misquoted Heidegger here: the original phrase from *Was ist Metaphysik* (1929) was “Das Nichts selbst nichtet” (a literal translation is “The nothing itself nothings”). This particular utterance had been criticized for false logic by Neurath’s Vienna Circle colleague, Rudolf Carnap (1931).

Of course, no fact-picture can show the subtleties or provide as much information as the extensive formulas of physics or other similar tools of highly developed science. The certainty and immediacy of educational effect is acquired at the expense of a certain coarseness; but such a price must be paid for all pedagogy.

He was careful not to overstate the potential of visual education, but he believed that the rough orientation it provided is often more important in “practical life” than comprehensive knowledge. These were not mutually exclusive, moreover: “It is a widespread error to think that simplified early orientation prejudices against further scientific education. On the contrary, it offers a solid foundation on which to build.”²⁸ This parallels Neurath’s proposal of a “physicalist everyday language” which could be learned by children, who could then advance to a more specialized language of science.²⁹ “Fact-pictures” [*Sachbilder*] were similarly a “bridge” between different levels of learning, and had the added advantage of being “independent of linguistic borders” – they were “*international* from the outset”.³⁰

Neurath also stressed the importance of statistics for the Vienna Method: “All discussions of social and economic questions link to statistical data. Planned control of healthcare, transport, production and consumption today needs extensive statistics.”³¹ This accords with Neurath’s intention, stated in the Vienna Circle manifesto, to explore statistical representation in his book as part of the Scientific World-Conception.

Neurath’s second book about Isotype, *International picture language* (Figure 6), was written soon after the National Socialist takeover of power in Germany, and after he and some colleagues from the Vienna Museum fled Austro-Fascism to The Hague. In the book, Neurath suggested that “the question of an international language” was important in supporting international developments to counteract the “warring interests and broken connections” of that time. In this spirit, the text of *International picture language* was written in Basic English, Charles Kay Ogden’s stripped-down version of the English

28 Neurath 1933, pp. 271–273.

29 Neurath 1931e, p. 64.

30 Neurath 1933, pp. 271–272. By comparison, the “universal jargon” of unified science was surely hampered in its “universality” by language barriers. Neurath (1946, p. 500) explained: “The English Universal Jargon would, therefore, be translatable into the French Universal Jargon or into the Esperanto Universal Jargon.” That sounds deceptively simple, and the quest for a logical syntax of language is complicated by translation, which is not strictly a science.

31 Neurath 1933, p. 274.

language intended as a lingua franca. Indeed, Marie Reidemeister formed the acronym Isotype (International System of Typographic Picture Education) by analogy with Ogden's Basic (British American Scientific International Commercial).³² Neurath reflected on the connection between the two:

The ISOTYPE picture language is not a sign-for-sign parallel of a word language. ... the uses of a picture language are much more limited than those of normal languages. It has no qualities for the purpose of exchanging views, of giving signs of feeling, orders, etc. It is not in competition with the normal languages; it is a help inside its narrow limits. But in the same way as Basic English is an education in clear thought – because the use of statements without sense is forced upon us less by Basic than by the normal languages, which are full of words without sense (for science) – so the picture language is an education in clear thought – by reason of its limits.³³

The limited vocabulary of only 850 words in Basic English corresponds to Neurath's oft-made suggestion that avoiding certain problematic words aids clarity in language. Despite some awkward phrases forced by the restrictions of Basic, *International picture language* remains Neurath's pithiest exposition of Isotype.

Here, again, he stressed the connection of Isotype to science: "Its rules are the instruments for putting together the work of science and the work of design." By implication, these rules adhered to a certain logic: "It is against the rules to make changes without any reason. Every change has, in addition, to say something." He criticized the flawed logic of graphs featuring a curved line drawn between points of data: only the data points have content – "the curve has no sense at all".

Every process, however simple, has to be in harmony with the rules of logic and mathematics. No process, however clear-cut, and however well based on science and delicate thought, will have any value for science or for education if it is not in harmony with the rules of this poor logic and mathematics.³⁴

32 Neurath and Kinross (2009), pp. 47–49. By her account, Marie Neurath co-wrote the text of *International picture language*.

33 Neurath 1936a, pp. 18–20.

34 Neurath 1936a, p. 66, pp. 103–104.

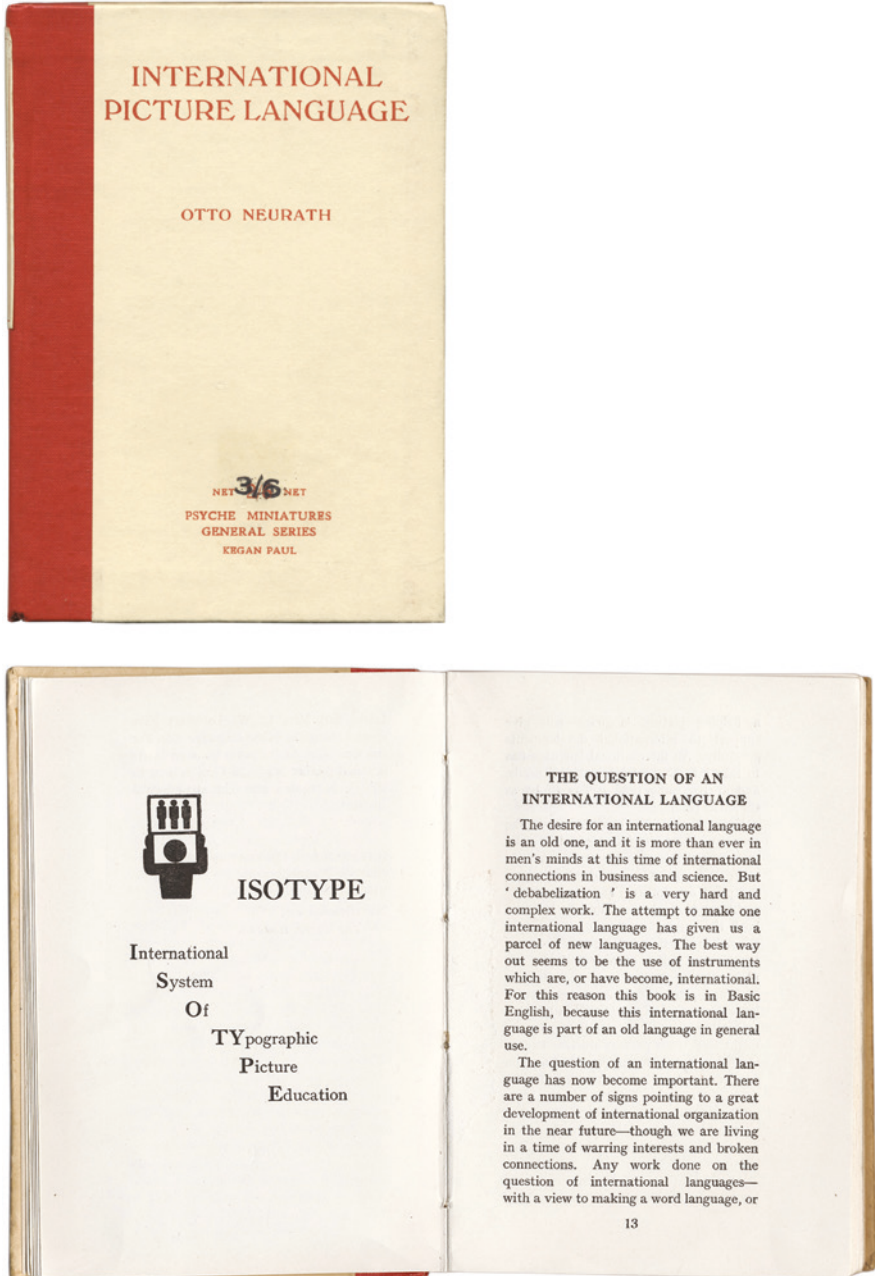


FIGURE 6 Cover and pages from *International Picture Language*, 1936
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As always, Neurath emphasized that there should be a continuum between basic knowledge and specialized expertise. This was a principal theme of “Visual education”, a book that remained unpublished at the time of his death: “In visual education there is no clear split into science and the humanities, nor into lower and higher knowledge.” He gestured back to the anti-metaphysics of the Vienna Circle:

Subjects that depend on verbal expression only cannot be taught by pictures, e.g. theological or philosophical doctrines. It is however perfectly possible to make quite complicated matters of fact intelligible by pictures. ...

This is the great advantage of visual aids: They appeal to adults and children, literates and illiterates equally – they are universal. ...

visualisation itself is not a protection against metaphysical speculation but it makes it simpler to maintain an empiricist attitude.³⁵

“Visual education” was written towards the end of the Second World War, and in it Neurath re-emphasizes the possibilities for international understanding, which he had put forward in *International picture language*:

The same visual aids which speak to us here in Britain may reach the Americans, the Russians, the Africans, the Indians, and the people in the Far East. Such a feeling prepares the environment for a kind of World Health Education which could support and be supported by the great international health services already in action.³⁶

Neurath paid tribute to Leibniz as a forerunner of Logical Empiricism, and, in doing so, asserted the primacy of visualization in his conception of it: “The two main features of Logical Empiricism seem to have been contained in him [Leibniz], namely logical analysis and visualisation.”³⁷ Leibniz’s unresolved project of a *characteristica universalis* was a persistent touchstone for Neurath, although he never claimed that Isotype provided a solution to it. In “Visual education”, he clarified that “The Isotype technique is a kind of auxiliary visual language combined with additional remarks in other languages.”³⁸ He responded to criticism that unification of language implied a certain

35 Neurath 1945, pp. 262–263, 278.

36 Neurath 1945, p. 263.

37 Neurath 1945, p. 274.

38 Neurath 1945, p. 332.

absolutism, commenting that “standardization of language does not imply a standardization of statements”:

We may create certain conventions in language without unifying the laws: a world language does not imply a world dictatorship but may help world understanding. For a democratic society it is important to have a common knowledge in a common language.³⁹

Neurath expressed similar sentiments in his “visual autobiography” *From Hieroglyphics to Isotype*, in which he further clarified that Isotype was not really a language but an “international language-like technique”.⁴⁰

4 Isotype and Logical Empiricism

Firstly, it should be pointed out that Isotype originated around 1923 – with the final exhibition of the Austrian Settlers’ Association and the subsequent Siedlungsmuseum – and was worked out in practice from the beginning of 1925, with the opening of the Social and Economic Museum.⁴¹ It reached a distinct point of maturity with the publication of *Gesellschaft und Wirtschaft* (1930). So, the Vienna Method (which became Isotype) largely predates the “public phase” of the Vienna Circle and the publications in which its members worked out Logical Empiricism.⁴² However, during its “private phase”, contemporaneous with the development of the Vienna Method, the Vienna Circle meetings hosted an intensive reading (twice, in fact, between 1924 and 1927) of Ludwig Wittgenstein’s *Tractatus logico-philosophicus* (1921). Neurath’s resistance to the text is infamous (disruptively pointing out the many incidences of what he considered metaphysics), but he admitted the importance of the *Tractatus* as a stimulus for Logical Empiricism:

39 Neurath 1945, pp. 319, 331.

40 Neurath 2010, p. 119. Before leaving Vienna, Neurath referred to the Vienna Method as a picture-script [*Bilderschrift*], never as a language. “Picture language” seems to have been forced on him partially by the limited vocabulary of Basic English, which does not include “script”.

41 The first experiments with pictorial statistics occurred at the Museum of War Economy in Leipzig, which Neurath directed between 1918 and 1919. No images have survived of this work. See Sandner 2014, pp. 91–98, and Vossoughian 2008, pp. 49–54.

42 The evolution of Isotype, in graphic terms, is well documented by Kinross 2013.

The Vienna Circle invested a lot of effort in extracting the logical core from the *Tractatus* which had been so highly praised by Russell, so as to free it from its metaphysical shell. Directly and indirectly, this yielded extremely noteworthy results, in particular the one that logic was now understood as the syntax of language.⁴³

Neurath may have absorbed something from discussion of Wittgenstein's ideas that could have contributed (if only negatively) to a theoretical background of Isotype, so it may be instructive to contrast their respective views on pictures and language. In particular, Wittgenstein's so-called "picture theory" of language offers an obvious point of potential overlap. In the *Tractatus*, Wittgenstein wrote:

- 2.1 We make to ourselves pictures of facts. ...
- 2.12 The picture is a model of reality. ...
- 2.141 The picture is a fact. ...
- 3 The logical picture of the facts is the thought. ...
- 3.01 The totality of true thoughts is a picture of the world.

The meaning of these gnomic statements is still open to debate: Wittgenstein does not seem to be referring principally to mental pictures but instead to be using "picture" as a metaphor for linguistic expression.⁴⁴ Concrete, graphic pictures seem not to have been his concern, whereas Neurath and his colleagues (particularly the artist Gerd Arntz) were actively concerned with making these in order to show social and economic connections. There seems to be no apparent kinship between Wittgenstein's theory and Neurath's reasons for developing a practical method of pictorial communication. While Neurath accepted Wittgenstein's "extremely fruitful approach to the radical analysis of language", he rejected the accompanying "mystical metaphysics", citing *Tractatus* 2.12 (above) as an example of it.⁴⁵

It was Friedrich Waismann who took on the burden of exegesis for Wittgenstein's ideas within the Vienna Circle. At its meeting on 26 February 1931, Rose Rand recorded him as saying that "we make pictures of facts for ourselves." Hans Hahn replied: "We also make pictures for ourselves of states of

43 Neurath cited in Stadler 2001, p. 217.

44 Wittgenstein himself later questioned the concept of an "inner picture"; see Mitchell 1988, p. 367.

45 See the mammoth footnote 2 in Neurath 1933c, p. 274.

affairs which are not facts". Neurath responded: "Why do you speak of an 'inner picture'?"⁴⁶ Rand did not record anybody having already referred to an "inner picture", but it is revealing that Neurath interpreted Wittgenstein/Waismann's "pictures" as such, and he clearly considered them spurious. In his work at the Social and Economic Museum, Neurath was more concerned with "outer pictures" for specific purposes of communication.

Further ambiguity arises in the *Tractatus* from Wittgenstein's distinction between "showing" and "saying". He stated (4.1212): "What *can* be shown *cannot* be said."⁴⁷ Intriguingly, some later remarks made by Neurath about the practice of Isotype evoke Wittgenstein's distinction, but in more concrete terms. In an essay ambitiously titled "The pedagogical world-importance of picture-statistics in the Vienna Method", Neurath asserted: "What can be shown with a picture, one should not say with words."⁴⁸ In a letter to an American colleague, with whom he had collaborated on educational posters about tuberculosis, he wrote: "It is hopeless, my dear [,] to *explain* Isotype to anybody – it is important to get the possibility to *show* it."⁴⁹ Yet the shift of attitude from Wittgenstein's apodictic phrasing is significant: in the *Tractatus*, Wittgenstein was preaching to the converted, by his own admission; Neurath's aim with Isotype was pragmatic and educational – he advocated the use of whichever mode of communication was most appropriate for a certain context.

"Our approach, free from metaphysics, has nothing to do with Wittgenstein's concept of meaning", declared Neurath about the Vienna Circle's quest for a "physicalist" language that would describe only spatio-temporal matters.⁵⁰ It was exclusively verbal language that was the subject of the dense debate about "protocol sentences" during the early 1930s, but Neurath suggested that pictorial statistics offered a fitting alternative for describing the world based on empirical observations:

46 Rand's protocol of meeting on 26 February 1931, in Stadler 2001, p. 257.

47 See Hintikka 1997, p. 167, for a reflection on what Wittgenstein meant by "showing".

48 Neurath 1933b, p. 243. A similar phrase resurfaced in his later writings: for example, in Neurath 1945, p. 328: "If it is not applied too pedantically, the slogan 'What you can present by means of pictures do not express by means of words,' might be promoted."

49 Neurath to H.E. Kleinschmidt, 8 July 1945 (Otto & Marie Neurath Isotype Collection, University of Reading [1C] 1/46). Another interesting parallel is *Tractatus* 4.016 "In order to understand the essence of the proposition, consider hieroglyphic writing, which pictures the facts it describes. And from it came the alphabet without the essence of the representation being lost." Neurath did think that something had been lost in that development; see Neurath 2010, p. 104.

50 Neurath 1934, p. 109. Intriguingly, Wittgenstein marked "Plagiat L.w." against a footnote in his own copy of Carnap's essay "Die physikalische Sprache als Universalsprache der

This means of course a retreat to some extent from the predominant scholastic tradition based on words and concepts, which often works against an empirical attitude, while pictorial education favours empiricism. Pictorial statistics operate from the outset with *spatio-temporal patterns*, while in verbal language the possibility exists of using *senseless links*, which can often only be got rid of with difficulty.⁵¹

Neurath's position on physicalist verbal language is fraught with ambiguity. It is difficult to reconcile his view that "The language of physicalism is nothing new as it were; it is the language familiar to certain 'naive' children and peoples"⁵² with his advocacy of a rarefied discourse in which "statements are compared with statements, not with a 'reality', not with 'things'.⁵³ This latter position earned him criticism from Schlick and Carnap for resembling a coherence theory of truth.⁵⁴ Surprisingly, Neurath believed that such linguistic self-reflexivity ensured empiricism, but it seems to be a contradiction of the principle set out in the Vienna Circle manifesto that "there is knowledge only from experience, which rests on what is immediately given".⁵⁵ In clarifying his position on Physicalism, Neurath asserted: "it is impossible to turn back behind or before language".⁵⁶ Isotype, however, is an attempt to do so, to circumvent the semantic problem of ineffability in verbal language: instead of using the symbolic notation of a Western alphabet, Isotype uses the iconic notation of a picture-script. The individual pictograms are homomorphic with real beings and objects.⁵⁷ Isotype depiction therefore required a suspension of epistemological doubt in accepting that there *is* a real world made of physical objects to which the pictograms corresponded (as such it is nearer a correspondence model of truth). Creating and "reading" the pictograms relied on a common, visual experience of physical objects in order for the iconic signification to work. When such an experience is lacking, it does not work: for example, the

Wissenschaft" (1932, p. 452), in which Carnap credits Neurath's view and summarizes it. Reproduced in Limbeck-Lilienau/Stadler (2015), p. 237.

51 Neurath 1931, p. 190. An alternative translation appears in Neurath 1931d.

52 Neurath 1931e, p. 66.

53 Neurath 1934, p. 108. On Neurath's "syntacticism" see Anderson 2019.

54 Sigmund 2017, p. 302.

55 Neurath 1929a, p. 309.

56 Neurath 1931c, p. 54.

57 Strangely, Neurath wrote to Susan Stebbing that, in the book *Basic by Isotype* (1937), he "intended to show that certain arrangements of words are 'isomorph' to arrangements of visual elements" (letter Neurath to Stebbing, 8 April 1939, ONN). In fact, this book shows the opposite – that there is no exact correspondence of form between the two modes

pictogram for sugar depicts a sugar loaf (Figure 5), which would have been familiar to viewers around 1930 but is unknown to subsequent generations accustomed to pre-packed, granulated sugar. So, in their case, the picture has to be learned instead as a symbol for what it represents. It no longer resembles what it represents, and ceases to be a “speaking sign”, as Neurath called Isotype pictograms. If a new Isotype chart about the sugar trade were to be made today, it might be necessary to design a new pictogram for sugar – no easy task. Some objects (such as a potato) resist schematic typification (Figure 7).

Such an analysis relies on the triadic definition by Charles Peirce as the basis for semiotic theory: icon, index, and symbol. Neurath no doubt knew of Peirce’s work on logic, but it is unclear whether he digested Peirce’s semiotic theory; Neurath made no specific reference to it in his writings or correspondence. Indeed, when writing in English about Isotype, he persisted in calling the pictograms “symbols”, although they were principally icons.⁵⁸

5 “Logic” and “Syntax” of Picture Script

Jordi Cat has argued persuasively for the centrality of Neurath’s early writings (with Olga Hahn) on algebraic logic to an understanding of his later work in economics and visual education. “Before he [Neurath] became concerned with the logic of image, and its social value, he was concerned with the image

of representation (Burke 2011, p. 35). Pietarinen (2011, p.73) argues that “pictures possess special *iconic* qualities that linguistic assertions by and large lack, which shows up in the ‘greater effect’ of the former by virtue of them being closely related to our actual cognitive structures and processes of thinking and reasoning.” Yet Pietarinen’s attempt, in his otherwise perceptive essay, to reconfigure an Isotype health warning about rickets with an “added logical structure of connectives” robs the example of its rhetorical power, which relies on simple comparison by adjacent placement.

58 Neurath discussed use of the term “semiotic” with Charles Morris for the *International Encyclopedia of Unified Science* (Burke 2011, pp. 41–42). It is likely that Neurath came across Peirce’s “Prolegomena to an apology for Pragmaticism” (1906), or the effective summary of it in Ogden/Richards (1923). Peirce (1906) set out a diagrammatic “System of Existential Graphs”: his idea of iconic representation related more to diagrams than pictures, and at one point he seemed to equate icons with syntax (p. 513). In discussing Peirce’s views that a diagram can show logical relations, Ambrosio (2014, p. 263) comments that he should not be seen as “a precursor of the concept of observability that the Logical Positivists, later on, would associate with the meaning of ‘observation statements’”; but she goes on to explain that “diagrams – and indirectly, iconic signs – for Peirce are an opportunity to reflect on the evidential status of logical relations. ... relations are *discovered* through the very process of constructing and inspecting a diagram.” This could serve well, also, to describe the purpose of Isotype charts to reveal connections.

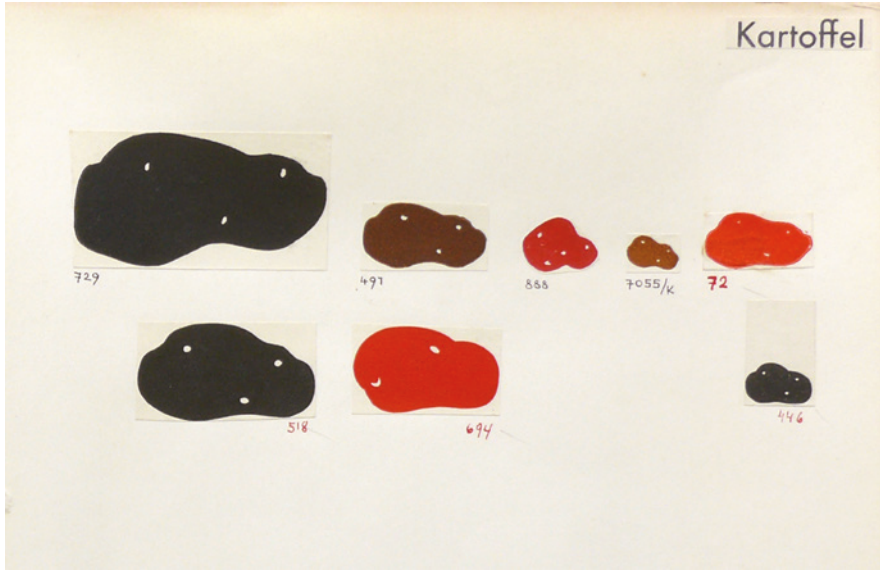


FIGURE 7 Page for “potato” from the picture “lexicon” prepared at the Social and Economic Museum of Vienna, c.1930
OTTO AND MARIE NEURATH ISOTYPE COLLECTION, UNIVERSITY OF READING

of logic”, as Cat neatly puts it.⁵⁹ Indeed, under Neurath’s direction, Isotype acquired certain rules that aspire to logical notation: this relates particularly to the statistical charts, or “number-fact pictures” [*Mengenbilder*] as Neurath called them. It was essential that “The same thing is represented by the same sign”⁶⁰ – in other words, that pictograms were assigned consistent meaning. This was explained in the foreword to the atlas *Gesellschaft und Wirtschaft*:

The unity of conceptual representation requires unity of pictorial depiction. Each chart can be compared with the others, and by comparison new connections become apparent, which a single chart could not show. The design of each individual chart always takes the totality into account: *where the same object is represented, the same symbol is used*, essential divergences in form mean divergences in content.⁶¹

59 Cat 2019, p. 303.

60 Neurath 1931a, p. 192.

61 Neurath 1930, p. 144 (*italic was bold in original*).

This applied also to the use of colour, which was generally not used naturalistically, but symbolically, although Neurath admitted that sometimes the same colour must be used for different means, as there were only so many available (especially in terms of print production).⁶² In many cases, choice of colour was “no longer a question of logic, rather one of pedagogical tact”.⁶³

The design of the individual pictograms had to be so simple that they could be lined up like the characters of a picture-script. Neurath sometimes referred to them as being like “letters”, but they are more like lexical units – indeed he called the repertory of signs a “Lexikon”. In terms of their design, he felt that “The charms of painterly qualities are a diversion”:⁶⁴ “Nothing is more dangerous than a pictogram that says more to some visitors than one really wished to express.”⁶⁵ The internal guidelines of the Vienna Museum stated: “The pictogram may not denote more than is necessary to the statement of facts for which it is chosen” – certainly a Logical Empiricist approach. They were often silhouettes, without internal detail; as such they could have symbols superimposed on them as qualifiers, similar to an adjective’s relation to a noun (Figure 8).

In terms of the syntax of combining modular, repeatable picture-units, Otto Neurath explained this in relation to a logic of visualizing mathematical quantities (Figure 9). After initial years of experiment, it was resolved to line them up in horizontal (not vertical) rows, with time running on the vertical axis. Taking into account the verbal elements always present in Isotype charts, they conformed to the reading direction of Western script: top-to-bottom, left-to-right – although a major difference is that a viewer’s gaze is not locked into linearity as with verbal language. Neurath also pointed out that horizontal lines of pictograms corresponded to the way that objects appeared in reality: “People, animals, cars all move horizontally over the earth’s surface.”⁶⁶ A certain mimetic quality had to be retained, but Isotype was schematic in its pictorial style, creating an artificial space typically devoid of depth – perspective and three-dimensional depiction were avoided (Figure 10).

The deceptive simplicity of Isotype pictograms and the structuralist element of their syntax meet the requirement of the Scientific World-Conception (as defined by Romizi) for “conceptual and linguistic clarity”.⁶⁷ The “constructivist

62 Neurath was however keen to use colours “physiologically” on maps, in agreement with cartographic adviser to the Vienna Museum, Karl Peucker. See Neurath 2010, pp. 107–108.

63 Neurath 1927, p. 187.

64 Neurath 1926, p. 60.

65 Neurath 1926a, p. 55.

66 Neurath 1933, p. 287.

67 Romizi 2012, p. 216.



FIGURE 8 One of the pages for “man” from the picture “lexicon” prepared at the Social and Economic Museum of Vienna, c.1930
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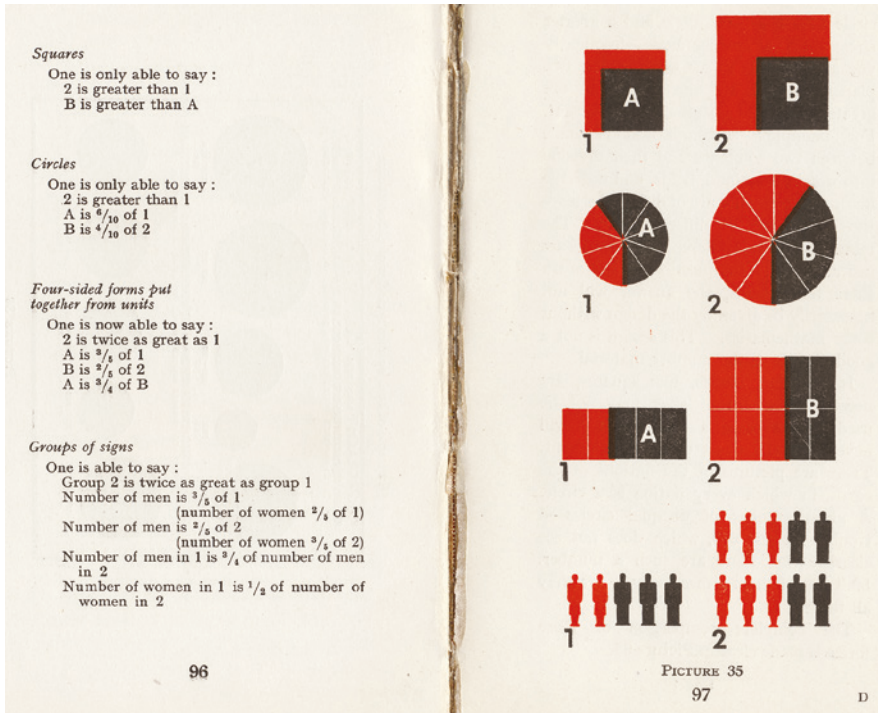


FIGURE 9 Pages showing the principles of Isotype picture statistics, from *International picture language*, 1936

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dimension” and “visual simplicity” are identified by Peter Galison as features of Isotype in sympathy with the Bauhaus approach to design. In his stimulating examination of parallels titled “Aufbau/Bauhaus”, Galison discerns “transparent construction” as a commonality in Logical Empiricism and modernist design, and asserts that Isotype “was essentially a linguistic and pictorial form” of this.⁶⁸ Yet the linguistic parallel should not be over-emphasized, as Neurath himself remarked late in life:

Let me add that this whole problem of making a Universal Jargon was put before me in a different shape when I was working out together with my collaborators an International Picture Language for educational

68 Galison 1990, p. 723. Isotype was not “constructivist” in the art-historical sense of tending towards abstract, geometrical form, which was typical of the Bauhaus. Gerd Arntz consciously distinguished his style, applied to Isotype pictograms, as “figurative constructivism”. See Benjamin Benus, “Figurative constructivism and sociological graphics” in Burke/Kindel/Walker 2013, p. 217.

purposes. ... The rules of picture writing are different. Starting with “icons” implies far-reaching limitations of language, but these limitations sometimes eliminate much danger.⁶⁹

Isotype and Physicalism may both have been efforts in the search for a “universal language” but, as Neurath states here, they took *different* shape. Indeed, he hinted that the pictorial equivalent of the Vienna Circle’s “protocol statements” would be the “visual [*optisch*] protocol” of photography, recording all details.⁷⁰ The personalized aspects and contingent details of photography are precisely what Isotype was designed to avoid: the Social and Economic Museum had a talented photographer on its staff (Walter Pfitzner), who documented housing projects and industrial processes, but Isotype charts almost never featured photographs. “We could not photograph social objects even if we tried”, Neurath explained, “They can be demonstrated only through symbols”.⁷¹ Pictograms were usually symbolic (as well as iconic in semiotic terms) in standing for a greater quantity. They were generic, representing classes and types of people, animals, and things, in order to articulate social and economic connections. Neurath suggested that “Quantity-pictures are to some extent renditions of objects from the sociological collection”,⁷² and to this end:

a fact-picture [*Sachbild*] prepared on the principles of picture-script should only contain such elements as are scientifically necessary in a systematic description. The fact-picture is hereby conceived in opposition to the naturalistic picture, to the photograph. ... The Vienna Method will, when possible, transform a spatial order into a flat order. ... to strive for spatial order for its own sake is uneducational.⁷³

Again, this emphasizes that the aim of Isotype was primarily educational, not philosophical. Protocol sentences (as defined by Neurath, at least) had to

69 Neurath 1941, p. 218. Jordi Cat (2019, p. 283) characterizes pictorial statistics and physicalist language as representing “the two faces of the Leibnizian algebraic project: the language against the calculus”. Cat (2019, p.319) also mentions Neurath’s familiarity with the work of J.H. Lambert, the second volume of whose *Neues Organon* was titled *Semiotik* (1764). Neurath seems to have disliked the term semiotic, and dismissed Lambert as a “mediocre thinker” overshadowed by Kant (Neurath 1936, p. 687). Nevertheless, some of Neurath’s ideas about language and symbolism are prefigured in Lambert’s book.

70 Neurath 1931, p. 180. Neurath also suggested that a photographic portrait could help to replace the fuzzy term “Otto” in protocol statements.

71 Neurath 1933a, p. 462.

72 Neurath 1931, p. 185 (this sentence is italicized in the original).

73 Neurath 1933, pp. 269–270 (italic in original) and p. 290.

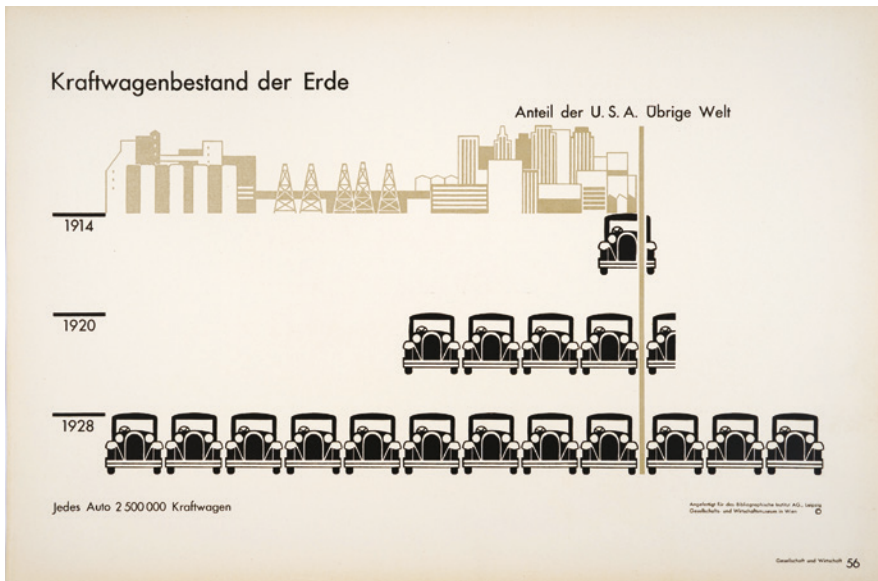


FIGURE 10 “Number of automobiles in the world”, *Gesellschaft und Wirtschaft*, no. 56, 1930
 OTTO AND MARIE NEURATH ISOTYPE COLLECTION, UNIVERSITY OF READING

include details of who made the statement and when, in order to be a stable record of observation; Isotype, on the other hand, was mostly based on statistical data, which Neurath valued as a means of building sociological knowledge beyond personal observation. Early charts in the Vienna Method included source tables of numerical data, but these soon disappeared, and were only included where possible, in the appendix to a publication, for example. Isotype acquired its visual strength partly by exclusion of background detail. Knowing what to omit was an educational skill in Neurath’s view. The selective decisions made by the transformer and designer in the Isotype method were not explicit; the responsible “maker” of the final charts was always given by the credit to one of the producing organizations: the Social and Economic Museum of Vienna, the Mundaneum, or the Isotype Institute.

How does this tally with Neurath’s requirement that Isotype be based on science and empirical facts? “To remember simplified pictures is better than to forget accurate figures” was an oft-repeated catchphrase of Neurath’s to justify the inevitable reduction of precision when making the graphics striking and memorable.⁷⁴ This approach and its results were criticized by contemporaries,

74 Neurath 1933a, p. 462.

but the Isotype team always maintained that the statistical images were accurate.⁷⁵ An example that may lead us to question this is the chart “Unemployed” from *Gesellschaft und Wirtschaft* (no.87, 1930; Figure 11).

This shows a striking comparison between three countries, which provokes the question: were there really no unemployed in France between 1920 and 1928?⁷⁶ But, on reading that one pictogram = 250,000 unemployed, viewers would assume that the figures did not reach that amount, or even half of it, having perhaps noticed that it was common in other charts to halve pictograms. Unemployment in France was indeed low compared to Britain and Germany during these years: the numbers of unemployed who registered for support and who applied unsuccessfully to work-placement agencies were too low to register on the scale of the Isotype chart; but the national census for 1921 recorded over half a million unemployed, which would register as two pictograms. The Vienna Museum team would no doubt have been aware of French census data, and perhaps also of contemporary questions about its accuracy.⁷⁷ It seems likely that only statistics from unemployment support agencies were used for this chart, but was equivalent data used for Britain and Germany? This is not made explicit, neither on the chart nor in the appendix of *Gesellschaft und Wirtschaft*, which provides no specific, numerical data for this example.⁷⁸

All this raises questions about the accuracy of this (and perhaps other) charts, although Neurath’s well-known attachment to scientific evidence would lead us to assume that no deliberate obfuscation was involved here. The

75 See a summary of contemporary criticism in Burke/Kindel/Walker 2013, p. 196–200, and in Sandner 2014, p. 191.

76 The chart was reproduced in the exhibition “Das rote Wien” (Red Vienna) at the Wien Museum (2019) and provoked this very question during a guided tour given by Günther Sandner for students of the Social Academy of the Chamber of Workers – precisely the kind of audience Neurath had in mind for workers’ education in the 1920s.

77 Fuss (1927, pp. 40–58) concludes that the French census figures “cannot be accepted without scrutiny”, and asserts that the number must in fact have been greater due to flaws in the census questions, which did not take into account home workers, for example. He questioned the oft-repeated assertion that France had practically no unemployment during the 1920s. He made further criticism that data from state-supported unemployment funds were not taken into account in official statistics, as they were in other countries.

78 Instead *Gesellschaft und Wirtschaft* (p. 123) frames the issue in a broader context of labour, with references to other charts. This was a characteristic strategy of Isotype, to provide connections to a larger body of work, in order to enrich a wider picture. The appendix text on “Workers’ movement” states: “France is moderately industrialized (chart 76), not overpopulated, and therefore still attracts workers (charts 74, 87), has weak labour organization (chart 84) and has had few strikes until now compared to Great Britain and Germany (chart 88).” See also the later “Unemployed” chart (1932) discussed by Kinross (2013, p.158), in which the data for 1920–8 remains consistent with figure 11.

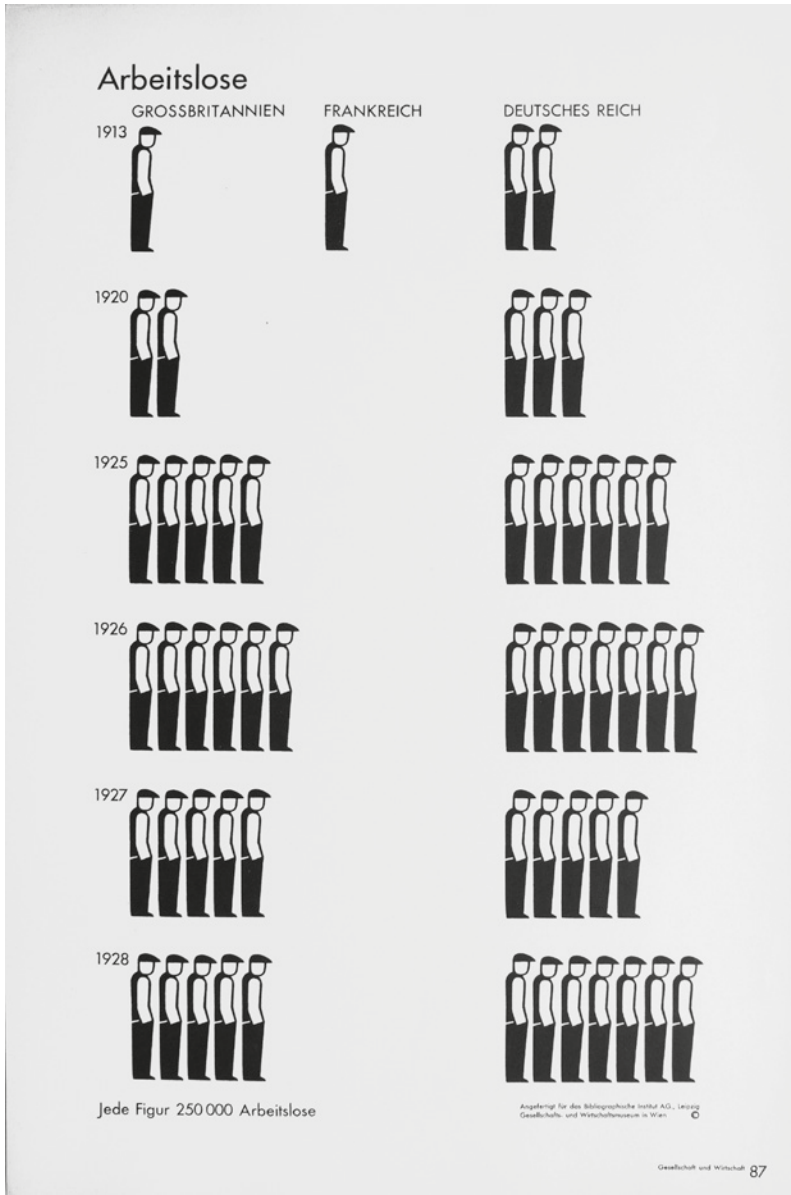


FIGURE 11 “Unemployed”, *Gesellschaft und Wirtschaft*, no. 87, 1930. The chart was evidently designed early in the preparation for *Gesellschaft und Wirtschaft*, before data for 1929 became available. OTTO AND MARIE NEURATH ISOTYPE COLLECTION, UNIVERSITY OF READING

Isotype team were reliant on statistics that were available to them at any given time, and were no doubt forced to make difficult decisions about whether data was trustworthy. An uncomfortable situation arose when Neurath and colleagues from the Vienna Museum went to the USSR to advise on work produced at a Soviet institute for pictorial statistics, Izostat. This body produced propaganda charts with projected data for the five-year plans, and this has been the subject of serious criticism (Figure 12).⁷⁹ Neurath was not, however, the director of that operation, and he came into conflict with Soviet officials about their use of statistics.⁸⁰

Making decisions based on incomplete evidence, despite a desire for exactitude, accords with Neurath's definition of a Logical Empiricist attitude, which should always expect to find a "corner with dirty unexactness, compromise etc. in it", as he revealed to a colleague.⁸¹ In 1941, he gave a lecture entitled "Logical Empiricism and Everyday Problems" at Bedford College in Cambridge, where he explained that it was also characterized by reticence to make simple cause-and-effect claims. Rather, it was concerned with relations and connections.⁸² So, empirically based, logical argumentation should be accompanied by recognition of ambiguities: in Neurath's view, Logical Empiricists "lack the unambiguity of traditional rationalism".⁸³

Let us examine an example of the Vienna Method of Pictorial Statistics to see if it reflects this attitude: a chart titled "Infant mortality and social position in Vienna" (1933; Figure 13). It subtly distinguishes dwellings in what are labelled as "wealthy" and "poor" districts by depicting the former as larger and lighter than the latter. Within these, the number of infant deaths per 20 live-born children is indicated. One may notice first that infant mortality decreased generally during the years 1925–9 compared to the beginning of the century. The simple explanation for this is the improved medical care developed under the health policy of Red Vienna, which would have been an obvious interpretation by the first viewers of this chart. The connection was made explicit

79 See: Chizlett 1992; Kinross 1994; and Emma Minns's essay in Burke/Kindel/Walker 2013.

80 See the letter from Neurath to Carnap quoted in Galison 1990, p. 741; and Sandner 2014, p. 232.

81 Neurath to Patrick Meredith, 20 January 1942 (IC 1/35).

82 In his Cambridge lecture Neurath gave Weber's *The Protestant ethic and the spirit of capitalism* as an example of a dubious cause-effect thesis. Neurath (1945, p.259) wrote: "The Unity of Science Movement is really concerned with a common terminology and with replacing e.g. a 'cause-effect' terminology by a 'grow-out-of' terminology."

83 Neurath 1941, p. 226. Neurath (1930a, p. 45) warned of "the danger that one creates a new idol by the postulate of complete definiteness".

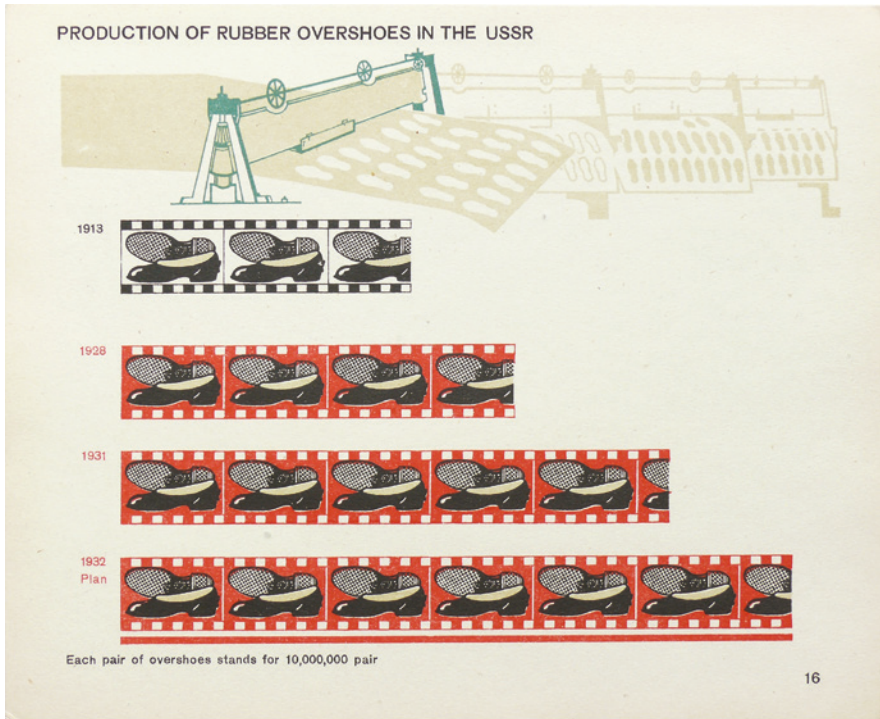


FIGURE 12 “Production of rubber overshoes in the USSR”, from portfolio *The struggle for five years in four*, 1932. The bottom row is labeled “1932 Plan” and uses projected data. OTTO AND MARIE NEURATH ISOTYPE COLLECTION, UNIVERSITY OF READING

in an earlier chart about “Decrease of infant mortality” by a red background added behind the years of welfare (Figure 14). But the later chart implies that there is also a connection between social class and health, and one might have expected infant mortality to have reduced proportionally more in those social strata that were previously denied quality health care. But ambiguities remain and a simple cause-and-effect process cannot be easily interpreted. The reduction in the number of deaths is the same in both districts, which means that it is reduced by two thirds in the wealthy district, and by half in the poor district.⁸⁴ This chart has an extra dimension compared to the earlier, colour chart, which does depict a simpler, cause-and-effect process.

84 Nemeth 2019, p.136.

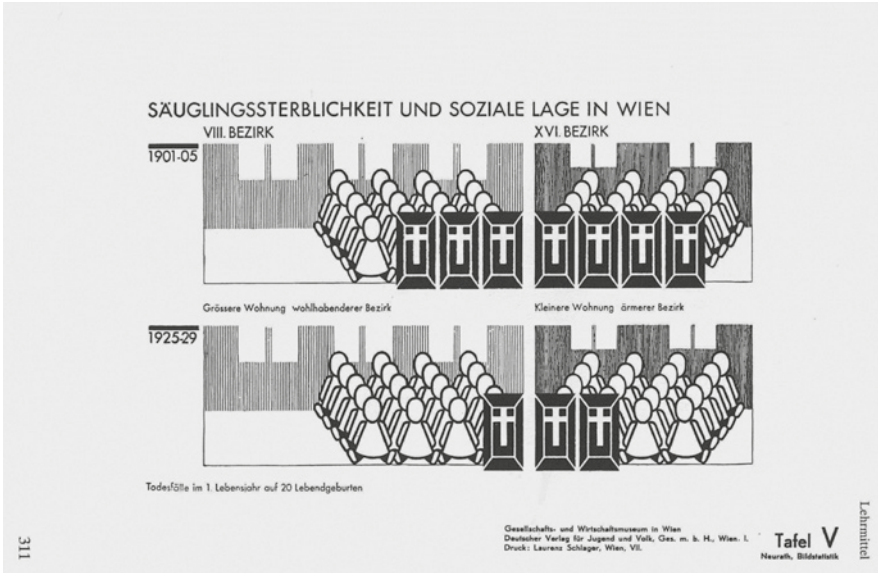


FIGURE 13 “Infant mortality and social position in Vienna”, from *Bildstatistik nach Wiener Methode in der Schule*, 1933

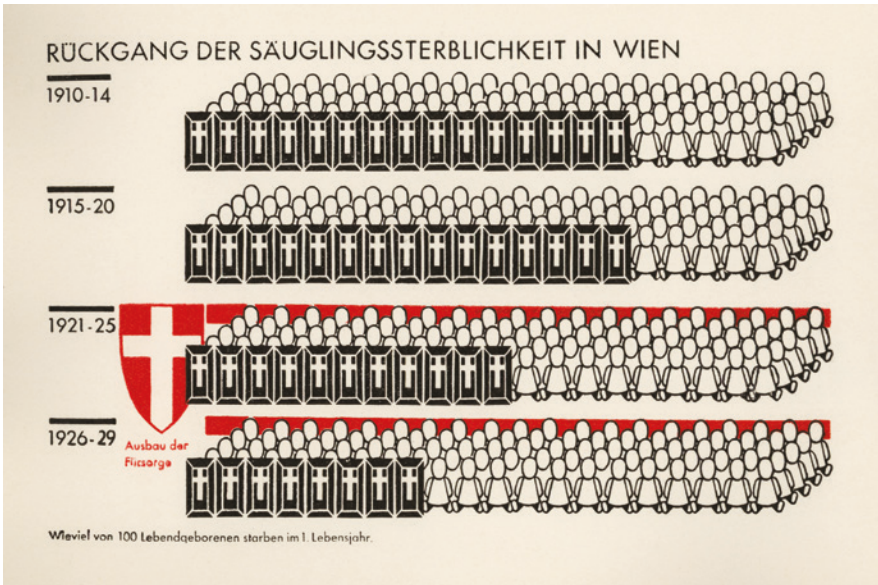


FIGURE 14 “Decrease of infant mortality in Vienna” (c.1929). The title of this chart, as depicting a “decrease”, is less ambiguous than the title and design of fig. 13. OTTO AND MARIE NEURATH ISOTYPE COLLECTION, UNIVERSITY OF READING

6 Visual Argumentation

In the 1940s, Neurath increasingly called Isotype charts “visual arguments”.⁸⁵ He meant this, perhaps, not only in the sense of logical argumentation but also in an everyday sense: an argument builds on information to initiate debate towards a conclusion. The ambiguities in Isotype charts leave room for interpretation and require completion by verbal language – not merely by their titles and explanatory keys, which (though essential) are plain and descriptive, but through questioning, reasoning, and discussion among viewers. (It should be pointed out that Neurath alone was not responsible for instilling such rhetoric into Isotype work: it depended on others in the production team sharing this approach, principally the “transformers”, Marie Reidemeister and Friedrich Bauermeister.)

An illuminating comparison arises from Neurath’s writings that demonstrates his differing stances towards a physicalist language of unified science and towards pictorial education (to some extent, this was a difference between statements and arguments). In his essay “Protocol statements” (1932/3), Neurath proposed that

one could think of a scientific cleaning machine into which protocol statements are thrown. The “laws” and other “factual statements”, including “protocol statements”, which have their effect through the arrangement of the wheels of the machine, clean the stock of protocol statements thrown in and make a bell ring when a “contradiction” appears. Now either the protocol statement has to be replaced by another or the machine has to be reconstructed.⁸⁶

Apart from being a remarkable prefiguration of the hypothetical machines conceived by Alan Turing a few years later, this reveals that the Vienna Circle wanted to refine language as a scientific instrument – “a scientific description which does no more than formulate statements for observational verification”.⁸⁷ Such an approach neglects any aesthetic element of language; Neurath appreciated poetry but (like Carnap) considered it irrelevant to science.

Isotype, by contrast, could not be automated in any comparable way, as Neurath explained: “the ‘Vienna Method’ is, unlike the usual graphic methods,

85 Neurath 1944.

86 Neurath 1932/3, p. 98.

87 Neurath 1931b, p. 50.

not a machine into which one throws sequences of figures in order to get quantitative pictures. The ‘Vienna Method’ requires *creative [gestaltende], educational work.*⁸⁸ So, despite similarities of attitude, Neurath perceived them as different modes of communication. The point of the “purified everyday language (‘universal slang’)” developed in the Vienna Circle was to establish a form of language that could be used by anybody. Isotype was not democratic in this way: although it was intended to be easily understood by everybody, it could, according to Neurath, only be created by those with special training and graphic imagination:

Since the application of the rules cannot become standardized, but each new picture needs, as it were, a somewhat new invention of combinations, *there is no possibility to transfer the rules in a simple way*, one has to become acquainted with the whole structure of rules and to learn how to *weigh* them from case to case ...⁸⁹

I myself stress the point, that Isotype is mainly a technique of *educational style* and a highly complicated *grammar*. The elements are stable, but the wit is *in the arrangement*, like *Shakespeare* is in the arrangement not in the dictionary of our English language.⁹⁰

Neurath seemed to imply here that designing Isotype graphics entailed a certain amount of tacit knowledge, which could not be fully explained. This raises the question of whether Isotype passes the test of “intersubjective accountability”, which Thomas Uebel has identified as the measure of kinship between Logical Empiricist philosophy and “transformative” social activities embraced by the Vienna Circle’s Scientific World-Conception (e.g. architecture, education). This “intersubjective accountability” (somewhat related to the “transparent construction” identified by Galison) demands that an activity can be justified rationally to an interlocutor – “The bottom line is accountability”.⁹¹ Neurath’s claim that “*there is no possibility to transfer the rules [of Isotype] in a simple way*” seems to defy this demand. Indeed Neurath openly explained that sometimes, when transforming statistical data into pictures, figures had to be “incorrectly rounded off” to “bring out the decisive proportions”.⁹² It would be

88 Neurath 1931, p. 185.

89 Otto Neurath, “Memorandum: dovetailed plans on series of books, textbooks, books for children”, typescript, c.1944 (IC 3.2/61), cited in Burke/Kindel/Walker 2013, p. 337.

90 Neurath to H.E. Kleinschmidt, undated [1944] (IC 1/46).

91 Uebel 2020, p. 45.

92 Neurath 1933, p. 286.

difficult to account for this in terms of a scientific method; instead, pragmatic design considerations often prevailed. An anonymous writer for the Social and Economic Museum of Vienna (probably Neurath) addressed this matter:

These are no longer questions of *logic*, rather of *pedagogical tact*. There are many logically justifiable solutions, but they are not all of equal psychological suitability. In deciding on one, psychological factors must be considered, colours and forms assessed for their conspicuity, the viewer's capabilities for perception must be taken into account; one must simplify, omit, underline, and point out connections, not with words, but by the design of the whole, through choice of colours etc.⁹³

An aesthetic element was important in Isotype: attractive, graphic qualities were an aid to effective communication. Each Isotype chart should be visually distinctive, but this did not correspond to wilful and arbitrary invention for its own sake. A balance had to be found with systematic consistency, as Neurath succinctly summarized: "A new picture-script is constructed, which is not only unified and precise, but also forms attractive and appealing pictures and layouts."⁹⁴ "It would be an error", he explained in *International picture language*, "to put a number of pictures before the eye which were as uninteresting as the statements they take the place of."⁹⁵ Verbal statements, formulated in a neutral way, are usually "dull and unattractive"; "Visual education, on the other hand, can be neutral without being dull."⁹⁶ Already in the first article he wrote to introduce the Social and Economic Museum (before the Vienna Method had acquired its name), Neurath pointed out that graphic statistics may be accurate but not visually stimulating: "We must consider factors that can as little be formulated, as recipes, as the means that a good poster artist uses."⁹⁷ Pictorial statistics had to compete with modern advertising and movies, in Neurath's view.⁹⁸

"We should get to the bones of the argument without being boring", Neurath declared as a goal of visual education.⁹⁹ To this end, he always wanted to have

93 Neurath 1927, p. 187.

94 Neurath 1930, p. 145.

95 Neurath 1936a, p. 66.

96 Neurath 1945, p. 253.

97 Neurath 1925, p. 523.

98 Neurath (1933, p. 284) explained that the element of standardization in Isotype "contradicts the essence of advertising".

99 Neurath 1945, p. 305.

colour at the disposal of Isotype, which was not always possible due to financial limitations. Compare the colourful depiction of world-encompassing subjects in *Gesellschaft und Wirtschaft* or *Modern man in the making* (1939) with the banality of subjects treated by Neurath's protocol statements ("Otto observes a thermometer registering 24 degrees"). The physicalist language debate took place on the "icy slopes of logic" referred to in the Vienna Circle manifesto, whereas the deliberately eye-catching material made with Isotype corresponds to the more popular means of spreading the Scientific World-Conception. Neurath suggested that pictorial statistics should have a similar appeal to the stories of Jules Verne: "The important principle, that one should begin from what is immediately at hand, only becomes meaningful when linked with the second principle, that one should show in rough outlines the most distant climes"¹⁰⁰ (Figure 15).

7 Conclusion

Democratisation of knowledge was a leitmotif for both the Social and Economic Museum of Vienna and Ernst Mach Association, as well as for other educational efforts involving representatives of the Vienna Circle's so-called "left wing". Science and education were seen as collective projects in both the Vienna Circle and the Vienna Method of Pictorial Statistics: the philosophy of science and visual education were both positioned against the individualistic cult of genius. The concept of collective work appears repeatedly both in the Vienna Circle manifesto and in Neurath's writings about Isotype. Neurath, in particular, was tireless in listing the work of others (past and present) whom he wanted to embrace within the fold of Logical Empiricism – even, with reservations, Wittgenstein. Yet he bristled at the adulation he detected in the treatment of Wittgenstein by some other Circle members.

There were significant divergences of approach among members of the Vienna Circle: the product of their thinking was a continuing discourse, embodied in written language, and never fully pinned down. Neurath compared philosophers unfavourably to the "feudal lords of San Gimignano", competing to construct ever higher towers to preserve their pristine isolation.¹⁰¹ Teamwork in the Vienna Method and Isotype was better defined, with a clear division of labour, including contributions from scientists, statisticians, artists,

¹⁰⁰ Neurath 1933, p. 305.

¹⁰¹ Neurath 1933c, p. 23.

and craftspeople, and resulting in definite (if not definitive) artefacts of graphic design. The visual and material qualities of these formed part of their purpose.

Both the Scientific World-Conception and Isotype shared a utopian character in the positive sense: they held out the prospect of a social transformation. Not surprisingly, then, they also shared the same enemies: metaphysics, clericalism, nationalism, and anti-Semitism. The Social and Economic Museum and the Ernst Mach Association were closed down at almost the same time in 1934, both due to their alignment with the Social Democratic Party, although neither of them were strictly party organizations. Nevertheless, they represented the same ideological milieu that was loathsome and threatening to the political right.

So there are obvious personal and institutional connections between Logical Empiricism and Isotype. However, it seems that only Otto Neurath considered visual education part of the Scientific World-Conception, and he developed it largely outside the Vienna Circle. There is one particular, tantalizing parallel that occurs in an essay Neurath wrote about unified science in 1933: his oft-repeated catch-phrase of the Vienna Method, “Words divide – pictures unite”, was modified to become “Metaphysical terms divide – scientific terms unite.”¹⁰² It is tempting to interpret an equivalence here, but Neurath certainly did not consider all words metaphysical, nor all pictures scientific; the more significant connection may simply be his favoured rhetorical device of opposing contrasts.

The Unity of Science movement could be seen as a lasting successor to Logical Empiricism, especially considering the project of the *International Encyclopedia of Unified Science*, of which Neurath was editor-in-chief. He also envisaged a multi-volume “visual thesaurus” as the fourth section of the encyclopedia, although no pictorial volumes were ever produced. In a letter to Susan Stebbing, he stated: “The educational background for Visual Education is that of Unified Science”. Referring to the two elements of his thesaurus/encyclopedia plan, he wrote: “The one Unification by Visualization, the other Unification by Logicalization.”¹⁰³ This complementarity was inherent in Neurath’s earliest ideas (from around 1920) for such an encyclopedia, and he explained that two stimuli influenced him in the following years: firstly, “As a director of a museum in Vienna ... I, together with my collaborators ... became profoundly interested in developing a consistent method of visual education”; secondly, and concurrently, was the development of the Vienna Circle, in which philosophy

102 Neurath 1933c, p. 23.

103 Neurath to Stebbing, 8 April 1939 (ONN).

was regarded as “the analysis of science and the discussion of its logical problems, not as a special superscience.”¹⁰⁴ Neurath hoped that these separate, but related, developments would come together in the Unity of Science.

When Neurath, as the “big locomotive”¹⁰⁵ of the Unity of Science movement died in 1945, the strong connection of Isotype to scientific philosophy ceased to a large extent; yet, the connection of Isotype to science was continued by Marie Neurath, who directed the Isotype Institute during the following decades. Her work in researching, writing, and designing books about science for young readers – making complex subjects comprehensible in pictures – was firmly in the spirit of the Scientific World-Conception.

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104 Neurath 1937, pp. 274–275.

105 Carnap to Neurath, 23 August 1945, in Cat/Tuboly 2019, p. 648.

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Kurt Gödel in the Carnap Diaries and Carnap-Neurath Correspondence: a New Look at His Personality and Scholarship

Friedrich Stadler

Abstract

The appearance of Kurt Gödel in the Diaries of Rudolf Carnap and in the correspondence between Carnap and Otto Neurath is certainly not surprising. But given the new primary sources provided by two running research projects on Carnap's Diaries and on the Carnap-Neurath correspondence, we obtain a more detailed and precise information on this scientific communication inside and outside the Vienna Circle and their influence on the subsequent philosophy of science community. In addition, we are enriched by two more research projects in Helsinki and the Kurt-Gödel-Forschungsstelle in Berlin, both of which deciphering Gödel's notebooks, diaries, and manuscripts written in his specific and difficult shorthand Gabelsberger. With reference to the unpublished and published sources, especially the fundamental book series of Gödel's *Collected Works* (Gödel, CW 1986ff.) in 5 volumes ed. by Solomon Feferman et.al., John W. Dawson's biography *Logical Dilemmas. The Life and Work of Kurt Gödel* (1997/1999), and several related books and articles, we uncover a better image of Gödel's life-work with reference to the running research projects as an added value, thereby widening the perspective on the Vienna Circle. This work in progress will be characterized with some surprising preliminary results.

1 1928: First Viennese Encounters

Chronologically, we find the first relevant entries on Gödel in the Carnap Diaries (CD) beginning in 1928: herein Carnap noted a first meeting with Marcel Natkin, Herbert Feigl, and Kurt Gödel in a coffeehouse with discussions on causality (May 15, 1928), which was continued the next day with the same colleagues. This was not by accident because both friends of Gödel wrote their dissertations under the supervision of Moritz Schlick, which were published

posthumously only in 1999 in the volume *Zufall und Gesetz* (chance and law).¹ In this regard it is worth mentioning that Gödel wrote a letter to Feigl (Sep. 24, 1928), dealing with Maxwell's equations and Russell on the request of the former.²

On Nov. 11, 1928, Carnap met Gödel again in a coffeehouse for a discussion about foundations of mathematics. Gödel claimed that deduction from logic has failed and recommended instead an intuitionist-formalist standpoint. Carnap remembers him as clever and mentions their agreement on some discussion points he had proposed, in order to consider the *Aufbau* according to these thoughts and considerations.³ In the middle of December (Dec. 14, 1928), he went with Gödel for coffee once more discussing the definition of decidability and foundation of mathematics.

At the end of this year (Dec. 30, 1928), the exchange with Gödel in the Viennese *Arkadencafé* (at that time vis á vis the main University building) is continued together with Friedrich Waismann, Herbert Feigl, and Marcel Natkin, with Carnap reporting on Gödel, who spoke on statements on language (introduced later as meta-language by Carnap in the Vienna Circle).

2 1929: Exchange Continued – on Axiomatics, Logic and Mathematics

In 1929 (Jan. 16, 1929) in a lunchtime discussion with Waismann, Feigl, and Gödel, Wittgenstein's objection to Carnap's quasi-analysis in the *Aufbau*⁴ was on the agenda, with the participants finding it necessary to clear up basic logical questions. Some months later, at the beginning of April (April 2, 1929) Carnap reported to Gödel on logic, once more, and in the afternoon circle

1 Haller/Binder 1999. The titles of the dissertations are: Herbert Feigl, "Zufall und Gesetz. Versuch einer naturerkenntnistheoretischen Klärung des Wahrscheinlichkeits- und Induktionsproblems" (1927); Marcel Natkin, "Einfachheit, Kausalität und Induktion" (1928); Tschahung, "Das Kausalproblem in der heutigen Physik" (1934). All three philosophers were students of Schlick who advised them and wrote the report in agreement with Heinrich Gomperz, the philosophical teacher of Gödel. Only Feigl, the favorite student of Schlick who published at the same time his results between 1927 and 1929, were highly recommended by Schlick (cf. Feigl 1981).

2 Gödel, *CW*, IV (Feferman p. 398). In this letter we find a remarkable paragraph on Gödel's critique of Russell's *Principia*: "I myself was in Brno the whole time and among other things read a part of *Principia mathematica.*, about which, however, I was less enthusiastic than I expected from its reputation" (p. 403).

3 In 1928 Carnap published his first fundamental and influential books *Der Logische Aufbau der Welt* and *Scheinprobleme in der Philosophie* (Carnap 1928a, 1928b).

4 Carnap 1928a, p. 70.

(participants are not mentioned), he spoke on his experiences in Davos, where he had met Ernst Cassirer and Martin Heidegger as discussants on the role and function of philosophy.⁵

The substantial coffeehouse communications (May 13, 1929) between Carnap and Gödel focused on Carnap's unfinished manuscript on axiomatics, which was published only in 2000 as *Untersuchungen zur allgemeinen Axiomatik*⁶ – after Gödel had attended lectures of Carnap at the University of Vienna in 1928.⁷ One issue at stake was his *k*-concept in the context of the ramification principle (*Gabelbarkeitssatz*) introduced by Carnap in his investigation.⁸

In the period of the preparation of the Vienna Circle manifesto⁹ in 1929, for which Carnap played a major role together with Hans Hahn and Otto Neurath, Gödel agreed with Carnap to be listed as an official member of the Schlick Circle. This seems worth mentioning given some narratives in the research literature where Gödel and others (like his mentor Karl Menger) are characterized as opponents of the Vienna Circle. It is true that several members did not agree to some basic doctrines but the philosophical pluralism was an inherent feature of this discussion group up to its forced dissolution.¹⁰ E.g., this is confirmed once more in the correspondence between Neurath and Carnap (July 30, 1929).¹¹

At the end of this crucial year, which enhanced the public phase of the Vienna Circle (Dec. 18, 1929), Carnap discussed with Gödel differences with the German mathematician Heinrich Behmann.¹² Independently, Gödel showed, according to Carnap, that Russell's antinomy appears also without the insertion of a short-sign and refers to the elimination of the nondenumerable infinity (*Überabzählbarkeit*). Shortly before Christmas (Dec. 23, 1929), Carnap refers to Gödel's claim of the inexhaustible property of mathematics, inspired

5 Friedman (2000) deals with this episode as the beginning of the split between analytic and continental philosophy.

6 Carnap (2000).

7 In 1928 Carnap announced the following lecture courses and seminars in the summer term: Philosophy of space (foundations of geometry), Readings and discussions in philosophy. In the winter term: Philosophical foundations of arithmetic, Discussions and readings in philosophy.

8 Ch. 3.4., esp. pp. 137 ff.

9 Stadler/Uebel (2000).

10 Correspondence Carnap-Gödel (June 26 and July 17, 1929): Gödel, *cw*, Vol. iv. On the theoretical differences cf. Stadler 2015, ch. 4.1.

11 Cf. the FWF-projects listed at the beginning after the abstract.

12 Behmann (1934, 1937/38), and *cw* iv. There is also a letter by Felix Kaufmann to Behmann (Oct. 19, 1930), in which he wrote to Behmann on Gödel's objections (seconded by Carnap) to their proof of constructivity (p. 566 ff.).

by L.E.J. Brouwer's lecture in Vienna:¹³ mathematics is not fully formalizable, to which Carnap comments: "he seems to be right!"

3 1930: Königsberg Conference – Gödel's Proofs

At the beginning of the year 1930 (Jan. 24, 1930), Carnap discussed with Gödel the problem of formalization of mathematics thinking and suggested that perhaps the not formalizable questions, propositions and concepts are not in the realm of mathematics proper. Three days later (Jan. 27, 1930), in a coffee-house evening with Olga Hahn-Neurath, Herbert Feigl and his girlfriend Maria Kasper, Friedrich Waismann, Carl G. Hempel, Samuel Broadwin, Rose Rand, Albert Blumberg, and Gödel, the psychologist Else Frenkel, who later married the psychologist Egon Brunswik, reported about a plan of discussion evenings. This entry is relevant because Gödel later met with Frenkel several times and was interested in a lecture series at the Department of Psychology of Karl and Charlotte Bühler.¹⁴ This interest is a surprising completion of Gödel's image as a pure hardcore logician interested exclusively in the formalization of science. Psychology and theology appear as additional fields of Gödel's rich and complex personality, accompanied by considerable amount of suffering from mental illness causing regular breaks in his academic career extending from Vienna to Princeton.

Nevertheless, the discussion on mathematics between Carnap and Gödel was continued, e.g., with a meeting (Febr. 8, 1930) focusing on the formalization of mathematics, the aim of mathematics, and problems of finitism. One of these encounters took place in Hans Hahn's home (Febr. 16, 1930) together with Alfred Tarski speaking on metamathematics, and on Gödel's problem of formalization. At that time, Gödel made remarkable progress in the strict formalization of arithmetic and set-theoretic proofs (von Plato 2018, 2022).

13 L.E.J. Brouwer was invited by the physicist Felix Ehrenhaft to Vienna for two lectures in 1928: "Wissenschaft, Mathematik und Sprache/Mathematics, Science, and Language" (March 10, 1928), and some days later "Die Struktur des Kontinuums/The Structure of the Continuum". Wittgenstein, who had attended the first lecture on the proposal of Menger, Feigl and Waismann, was so strongly impressed that he decided 6 years after the publication of his *Tractatus logico-philosophicus* (1922) to return to philosophy after having realized that not all philosophical problems were being solved as stated in his first book. The lectures in context: Menger (1994, ch. 10).

14 This is documented in the above mentioned "Gödel Enigma"-Project by Tim Lethen, *Gespräche, Vorträge, Séancen: Kurt Gödels Wiener Protokolle 1937/38 – Transkriptionen und Kommentare*. Cham: Springer 2021.

This regular scholarly communication in private homes, coffeehouses, public locations and university departments was a unique discussion culture. E.g., the dialogue between Carnap and Gödel in a restaurant on logicism, referring to a manuscript of the former (March 4, 1930) is worth mentioning – again dealing with the ramified theory of types (*verzweigte Typentheorie*). Gödel agreed that one can ignore it, if one does not ask about the meaning of φ (ϕ) as a universal quantification, but proceeds meta-mathematically.

The entry, dated March 18, 1930, reports that after a presentation by Richard von Mises, Carnap discussed with Neurath, Hempel, Feigl, and Gödel the problem of atomic sentences: Neurath preferred ordinary language close to physicalist language, and Gödel was in favor of the physicalist one, or perhaps both, namely starting with ordinary language, but not with singular sensations.

In July 9, 1930, the next meeting took place in the Viennese coffeehouse *Café Reichsrat* with the participants Feigl, Kasper, Blumberg, Franz Kröner, Gödel, and Heinrich Neider. Another decisive meeting took place (August 26, 1930) in the same location with Feigl, Gödel, Waismann, in order to prepare the travel plan to the Königsberg conference (2nd Conference on the Epistemology of the Exact Sciences, Sept. 5–7, 1930). In this unofficial circle, Gödel's famous discovery was already on the agenda: the incompleteness of the system of the *Principia Mathematica*, and the difficulties with the proof of consistency. At the end of August (Aug. 29, 1930) Gödel reports on his discovery in the *Café Reichsrat* and Carnap asked him on the renunciation of the ramified theory of types. This was the overture for the public presentation of Gödel's results in Königsberg (speaking on his completeness theorem), where on Sept. 3, 1930 Carnap, Feigl, Gödel, and Waismann were to travel together.

The subsequent entries in the Carnap Diary (Sept. 6 – Sept. 13, 1930) refer to this pathbreaking conference with the dispute on the foundations of logic and mathematics in the focus:¹⁵ they refer to Gödel's lecture (*Vollständigkeit des Logikkalküls/Completeness of the Calculus of Logic*), and to meetings with Feigl, Gödel, Neider, Hans Reichenbach, which took place also in Berlin and during the return trip to Vienna.

Back in Vienna, the dispute on the foundations of mathematics was still on the agenda; as already mentioned above, again with Behmann and Hahn at Kaufmann's home, where they dealt with Behmann's modal logic and Gödel's remarks on Russell's antinomy (Sept. 17, 1930).¹⁶ Two days later this was followed up at Carnap's home with Behmann's reply to Gödel's objection (Sept.

¹⁵ Stadler 2015, pp. 161ff.

¹⁶ *Erkenntnis* 2, 1931, p. 105.

19, 1930). Still one month later (Oct. 16, 1930) Carnap met Gödel who refuted Behmann's claim that the proof of existence can be made constructively.¹⁷ With reference to these interactions, Carnap wrote to Gödel at the end of October regarding the discussion protocol of the Königsberg Conference for publication in the journal *Erkenntnis*.¹⁸

Still in November of the same year, Gödel commented on Carnap's discussion remark at the Königsberg conference: if an interpretation would be possible, then formalisms would be unnecessary (Nov. 21, 1930).¹⁹ At the end of 1930 (Dec. 23, 1930), Carnap reports on an afternoon meeting with Gödel as a most interesting discussion on his meta-mathematics (proofs).

4 1931: Gödel's Results in the Schlick Circle

In the beginning of 1931, Gödel's results were discussed also in the Schlick Circle as we can draw from the few protocols, provided by Rose Rand. Especially the meeting on Jan. 15, 1931 is a concrete source for this crucial exchange in the Vienna Circle, with a discussion on Gödel's preceding presentation:²⁰

About noncontradiction and decidability in axiomatic systems. Questions and answers (about the report by *Gödel*).

Kaufmann asks about the decidability of sentences of partial systems.

Gödel responds that, as far as it can be proven, this proof must employ means which cannot be formalized within the partial system itself. This would be in agreement with his proof.

In response to a question by *Hahn*, *Gödel* once again recalls the principal thought of his proof of the impossibility of a proof of noncontradiction. If the noncontradiction of a system is added to the system itself – and this extension can be formally effected – then an originally undecidable sentence becomes decidable in this extended system; it follows that the noncontradiction of a system cannot be shown in the system itself.

17 See fn. 12. Kaufmann published in 1930 his book *Das Unendliche in der Mathematik und seine Ausschaltung*. English: *The Infinite in Mathematics. Logico-mathematical Writings* (1978).

18 Carnap to Gödel, October 26, 1930, in: CW IV.

19 Carnap's contributions and remarks in *Erkenntnis* 2, 1931, pp. 87–190.

20 Stadler 2015, p. 78f.

In response to a question by *Schlick*, *Gödel* formulates the speculation of von Neumann: If there exists a finite proof of noncontradiction, then it can also be formalized. Thus, Gödel's proof involves the impossibility of a proof of noncontradiction in general.

Hahn asks about the application to the axiomatic system of Heyting. *Gödel*: The system of Heyting is more restrictive than that of Russell. If it is O-noncontradictory, then one can state undecidable sentences for it.

Hahn points to the fact that, since Cantor's use of the diagonal method, one of the basic thoughts of the proof "There does not exist a meaningful whole or totality of what can be constructed" has played a decisive role in set theory. *Gödel* remarks that the application of this thought also renders questionable whether the totality of all intuitionistic acceptable proofs can be fitted into *one* formal system. That would seem to be the weak point in Neumann's argumentation.

Kaufmann asks about the noncontradiction of sentences which do have a pair of concepts in common or the Peano axioms. There exists a first, there exists a last number. *Gödel* replies that the concepts as such are not important for the proof of noncontradiction. It is not at all a matter of noncontradiction in the sense of material [*inhaltliches*] thinking. In response to the interjection by *Kaufmann* that proofs of substantive noncontradiction are not excluded, *Gödel* clarifies: Such 'insights' do not represent proofs in the sense of a formalized theory.

Neumann asks whether there exist systems which are so simple that the concrete form of the undecidable sentence can be given in a transparent manner. *Gödel* replies that it is a question of the system in which it is to be represented. He recalls the decisive technical means of his procedure [:] the isomorphic representation of the forms of argument from sequences of numbers f_1 to sequences f_2 , which alone makes it possible to formulate provability. E.g., if $S(f_2)$ denotes a form of argument, $l(f_2)$ the 'length' of the associated chain, then the provability of f_1 is written as follows:

$$\text{Bew. } f_1 \equiv (\exists f_2) \{S(f_2) \& f_2[l(f_2)] = f_1\}$$

Then one can either rest with that or analyze the symbol S further.

Hahn draws attention to the book by Lusin, "Sur les ensembles analytiques." Concerning the existence of proofs for Borel's sets of higher

orders, Lusin distinguishes carefully whether the diagonal method works or not. Then *Hahn* asks whether the diagonal method can be dispensed with in Gödel's proof. *Gödel* answers that the undecidable formula which he indicates is really constructible. Its content is finite like that of Goldbach's conjecture or Fermat's theorem. Concerning a remark by *Kaufmann*, *Gödel* replies finally that according to the views of Brouwer intuitionism is not touched by his work, because it is not intended to be contained in a formal system.

On Feb. 7, 1931, Gödel visits Carnap at his home speaking on his work, which Carnap found as hard to understand. He presented his plan of a language construction as a sort of preparation for discussions in the Schlick Circle. Gödel replied, that Waismann would not agree and that Carnap would be the only positivist in the Circle. Later in the evening Carnap's girlfriend and later wife Ina (Stöger) joined and Carnap writes in good spirit: "We are teasing Gödel for his arrogant modesty" (*arrogante Bescheidenheit*).

In the next coffeehouse discussion with Gödel (Feb. 28, 1931), Carnap and Gödel dealt with a correction of Walter Dubislav's critique of Behmann with reference to Gödel as is documented in Behmann's published reply in *Erkenntnis* (1931/2).²¹ Afterwards both discussants attended the Gomperz Circle at Viktor Kraft, where Neurath reported on his book *Empirische Soziologie* (1931). This book had caused significant amount of problems and discussions between Neurath and Schlick who had rejected the first version of the manuscript because of its alleged "unscientific" style.²²

The controversy between Behmann and Gödel went on as we can see from the subsequent meeting (March 17, 1931) of Carnap with Gödel in the coffeehouse, covering also the predicate calculus without identity, which Gödel, by the way, did not approve of.²³

On the occasion of an afternoon meeting with Gödel (April 21, 1931), Carnap drew on his draft on arithmetic admitting that real numbers being difficult, to which Gödel proposed Brouwer's method. Another issue was Carnap's attempt of coining a logic without existential assumptions. Exactly one month later (May 21, 1931), Carnap discussed with Gödel the impossibility of formalization of mathematics with reference to Hilbert's new rule, which according to Gödel violates the program. The last meeting before summer (June 10, 1931) between

21 Behmann 1931, p. 305 f.: Zuschriften an die Hrsg. "Zur Richtigstellung einer Kritik meiner Auflösung der logisch-mengentheoretischen Widersprüche".

22 Stadler (2021, forthcoming).

23 See the related correspondence of Gödel with Behmann 1931 in *cw* 5.

Carnap and Gödel addressed the topic of metalogic, on which Carnap lectured three times extensively in the Schlick Circle (in June and July 1931).²⁴

Also during this summer, the private circles convened, one with Herbert Feigl and the Finnish philosopher and psychologist Eino Kaila (who, by the way, was upset by Carnap's personal way of living) discussing analytic equivalence and his *Gestalttheorie*, which was also a topic of Schlick's critical dealing as a metaphysical construction just this time.²⁵

At the end of next month (Aug. 30, 1931), Carnap went to the coffeehouse with Feigl and Gödel to talk about David Hilbert's new treatise (on the Tertium), which was seen as extremely questionable.²⁶

In 1931 Carnap wrote only two more entries in his diary: Sept. 10, referring to a walk with Gödel to a mountain (maybe the Vienna Woods, known as *Wienerwald*), on Hilbert's paper mentioned above, and on his new hometown Prague (to where Carnap had moved after his appointment at the Philosophy Department of the German University), but also on socialism: Gödel had read Lenin and Trotsky, was interested in planning theory, esp. in the impact of economics like the financial capital on politics – certainly a remarkable feature of Gödel's interests (The last meeting is reported by Carnap with Gödel, Waismann, and Philipp Frank from Prague, the latter obviously visiting him at his new hometown at the end of summer break).

5 1932: *Logical Syntax and Tractatus* on the Agenda

Regarding the year 1932, we can draw on Carnap's Diaries again, and on the Carnap-Neurath Correspondence with references to Gödel, in addition.²⁷ These sources will be complemented by the already published correspondence between Carnap and Gödel:²⁸

24 See the protocols in Stadler 2015, pp. 112–129.

25 Published later on as (Schlick 1934, 1935). See also (Fisette/Frechette/Stadler 2020) for this discussion between the Brentano School and Schlick.

26 (Hilbert 1931).

27 Based on the results of the FWF research project P30377 "The Carnap-Neurath-Correspondence", conducted by Johannes Friedl at the Department of Philosophy, University of Graz.

28 The correspondence between Gödel and Carnap, 1930–1964, published in Gödel, *cw* vol. 4. Some letters (1929, 1935) are not included and will be documented in the research project on Carnap's Diaries (FWF-Project P 31716, conducted by Christian Damböck, Institute Vienna Circle, University of Vienna).

Already on the first of January, Carnap in his letter to Gödel asked him whether he would like to read his manuscript on metalogic (most likely the proto-manuscript of his later *Logical Syntax*, published in 1934.) And only one day later he refers to his first part of metalogic referring to Zermelo, Dingler, and Chwistek. After some days (Jan. 5, 1932) he reports on his lecture on Gödel in a “mathematical circle” (*Kränzchen*) with a vivid subsequent discussion, most likely at the Department of Mathematics.

On March 9, 1932, he met Ralph Barton Perry, a student of Charles S. Peirce, having dinner together with Menger, Hahn, and Gödel. Carnap explained his doubts on the logic of modality, and preferred a similar system as proposed by C.I. Lewis. In his first seminar on foundations of geometry at the Department of Philosophy at the beginning of the summer term (March 11, 1932), he spoke on Gödel and continued his exchange with the former on metalogic (March 24, 1932). In this meeting with the participation of Eino Kaila and Waismann, we find a puzzling entry: “Wittgenstein is approaching us; no comparison between proposition and reality, all is ‘grammar’”. This is one more evidence of Wittgenstein’s linguistic coherentism after his turning away from the picture theory of language of his *Tractatus Logico-Philosophicus* (TLP). At Neurath’s home (March 26, 1932), the discussion on metalogic is continued with Gödel, who proposes “semantics” as title of Carnap’s projected book. Independently, directed at Neurath, he claims that there is no relation between socialism and physicalism and articulates the dependence of Waismann on Wittgenstein.

Just in this context, the Carnap-Neurath letters are elucidating, for example, on April 4, 1932, Carnap mentions to Neurath that Gödel had proposed to him the term “semantics” for his manuscript, which was also used by the Polish philosophers – and he continues surprisingly: “I like it better than ‘Syntax’ ... As book title probably “General Semantics” (*Allgemeine Semantik*).” In the related published letter to Carnap (April 11, 1932), Gödel reports on “Metalogik and Kaufmann” and criticizes the Polish philosopher Leon Chwistek (1932/33), who had published on the nominalistic foundation of mathematics in *Erkenntnis*.²⁹

After a longer break, only in the summer of 1932 (July 18, 1932) Carnap tells Neurath that Behmann, Hempel and Gödel will read his book manuscript (= Semantics/Syntax). Neurath’s indirect reply was dealing with the unity of science and the protocol sentence debate incl. Wittgenstein and concludes with the proclamatory saying: “We are the Vienna Circle. And as I believe more frequent the group: Carnap, Frank, Hahn, and Feigl, Gödel, Hempel” (August

29 Leon Chwistek, “Die nominalistische Grundlegung der Mathematik” (1932/33, pp. 367–388).

10, 1932). Some days later Carnap notes in his diary: “semantics v. Gödel’s letter: def. of ‘analytic’ is not correct.” Here, the direct exchange between Carnap and Gödel on analyticity is relevant (Sept. 11, Sept. 25, Sept. 27, 1932).

At the beginning of the winter term (Oct. 9, 1932), Neurath urged Carnap not to use the term “wissenschaftliche Weltauffassung” (scientific world conception) any more, and replace it by “Einheitswissenschaft” (unity of science). Both go on with their exchange (between Oct. 17 and 24) with the agreement of naming Gödel in the large name-dropping regarding logical questions in the book manuscript. The subsequent exchange is documented in the Carnap Diaries:

Gödel himself told Carnap in Prague that the second part of his manuscript is not yet existent. He finds the usage of “analytic” fine, agreeing with Tarski and reports to him from the Schlick Circle. (Nov. 28, 1932). Two weeks later Carnap met Gödel in the famous Café Museum in Vienna (Dec. 12, 1932), where they discussed the definitions of “analytic” and “true”. With reference to Wittgenstein also on finding a definition for “meaning” with the possibility to speak on words and subjects simultaneously. Gödel is reported with his intention to write a book on the foundations of mathematics, and there is a closing remark: “afterwards Popper”!

Apparently in the Schlick Circle at the Dept. of Mathematics (Dec. 14, 1932), these topics like semantics still remained on the agenda: with Hans Hahn, Olga Hahn-Neurath, Gödel, Menger, Kraft, where Hahn claims non-constitutive concepts. After Menger’s lecture “Die neue Logik” (1933), Gödel told Carnap that he (Carnap) had taken over his earlier position.³⁰ During a last meeting in 1932, Gödel declared to Carnap that the material mode of speech is not tenable (Dec. 15, 1932).

6 1933: Wittgenstein, Habilitation and Princeton on the Horizon

Only in the middle of 1933 (June 7, 1933), in the Carnap Diaries we find an entry on the next meeting with Gödel, who talks about his invitation to Princeton with reference to the foundation of mathematics. In addition, they talked about politics and the Schlick Circle with which Gödel was not satisfied, and about Wittgenstein’s letter to Carnap via Schlick dealing with the priority

³⁰ Karl Menger, “Die neue Logik”, in: *Krise und Neuaufbau in den exakten Wissenschaften. Fünf Wiener Vorträge*. Leipzig-Wien 1933, pp. 94–122. English: “The New Logic”, in: *Philosophy of Science* 4, 1937, pp. 299–336.

issues in that Gödel interpreted Wittgenstein's strangeness always as a desire to dominate. At Gödel's home (July 9, 1933) Carnap reports on Gödel thinking that theology begins with indefinite concepts. Carnap completely agrees with his position on the division of signs in arithmetic and discrete ones, and on the notion of inference in a logical and physical (*physikalische*) one. In addition, they spoke on C.I. Lewis' recent book (probably *Symbolic Logic*, 1932). Three days later (July 12, 1933), Carnap was invited "at Gödel's". The passage tells that his mother, introduced as an honest but somewhat narrow-minded wife of a factory owner is concerned because her sons are studying instead of aiming at a secure profession.³¹ Carnap was told, that Kurt was excellent in the school; this is already known in the research literature. The scholarly communication was continued in the middle of July, e.g., with Gödel in the Votiv Park near the University at the Ringstraße focusing on f-concepts and terminology (July 14, 1933), and at his home about Hahn's supposed prejudice in occultism, and his justifications having been pretty weak.³² With an official letter Carnap asked Gödel to help with getting a recommendation of John von Neumann in support of his application for a US grant (Carnap-Gödel, Oct. 9, 1933).³³

In the correspondence of Carnap and Neurath, we find additional reports: e.g., in 1933 Olga Hahn-Neurath tells Carnap about Gödel's successful Habilitation without problems: "es hat viel für sich, wenn man arischer Abstammung ist" (it is advantageous if one is of Aryan descent). Also, Hans Hahn and Hans Thirring as lecturers on parapsychology are mentioned there. At the end of this year (Dec. 23, 1933), Carnap wrote to Neurath on the twofold sources of his *Syntax*: 1. Wittgenstein, 2. Metamathematics (Tarski, Gödel), and later on the development of the Circle (1. Wittgenstein, Aufbau, 2. ...).

7 1934: *Syntax* and the Principle of Tolerance

In 1934, which was the crucial year for the Vienna Circle with the dissolution of the Ernst Mach Society (Verein Ernst Mach), the unexpected death of Hans Hahn, and the forced migration of Otto Neurath to the Netherlands caused by the Civil War, the publication of Carnap's *Syntax* remained an issue in the communication. E.g., already on the first of January, Neurath wrote to Carnap asking him to insert a reference to Carnap, Frank, Gödel, Hahn, and himself regarding the origins of his book. In his diaries (April 1, 1934), Carnap reported

31 Kurt Gödel's brother Rudolf studied medicine and became a medical doctor.

32 Hans Hahn was strongly interested in parapsychology from an empirical point of view (cf. Menger 1994).

33 Gödel, *cw* Vol. IV.

to Waismann and Schlick on Wittgenstein's influence on him regarding the origin of the principle of tolerance; by the way, the origin of this principle was strongly claimed by Karl Menger, too (Menger 1979, pp. 11–60). In addition, Waismann had doubted that Wittgenstein could have been influenced by Gödel via the Vienna Circle. Carnap eventually promised a reference in the preface of the *Logical Syntax of Language* in order to calm down the emerging priority disputes.³⁴

From the subsequent Carnap-Neurath correspondence, it appears once more that Gödel figured as a projected speaker for the forthcoming conferences for the unity of science, beginning with a preparatory meeting in Prague (July 5 and 8, 1934). Most of these invitations Gödel declined because of his several breakdowns due to his mental illness. Nevertheless, Carnap and Gödel exchanged in their letters some problems related to the *Syntax*.³⁵

8 1935: Paris Conference, Tarski and Russell

In 1935, we see an intensification of the communication: in February 1935, Carnap notes in his diaries – on Frank's report from Vienna – that the *Syntax* would overemphasize the formalism; the proper task would be to state what ordinary language means with its words. To this, Frank had objected several times and was supported by Menger, Gödel, and Tarski. Nevertheless, both

34 In the Foreword, further explained in §17 (The Principle of Logical Tolerance in Syntax) of the *Logical Syntax of Language*, Carnap wrote (1934/37, p. xvi): “For the development of ideas in this book I owe much to the stimulation I have received from various writings, letters and conversations on logical problems. Mention should here be made of the most important names. Above all, I am indebted to the writings and lectures of Frege. Through him my attention was drawn to the standard work on logics – namely, the *Principia Mathematica* of Whitehead and Russell. The point of view of the formal theory of language (now as ‘syntax’ in our terminology) was first developed for mathematics by Hilbert in his ‘metamathematics’, to which the Polish logicians, especially Ajdukiewicz, Lesniewski, Lukasiewicz, and Tarski, have added a ‘metalogic’. For this theory, Gödel created his fruitful method of ‘arithmetization’. On the standpoint and method of syntax, I have, in particular, derived valuable suggestions from conversations with Tarski and Gödel. I have much for which to thank Wittgenstein in my reflections concerning the relations between syntax and the logic of science; for the divergences in our points of view, see pp. 282 ff. ... Again, I have learned much from the writings of authors with whom I am not entirely in agreement; these are, in the first place Weyl, Brouwer, and Lewis. Finally, I wish to express my gratitude to Professor Behmann and Dr. Gödel for having read the manuscript of this book in an earlier draft (1932), and for having made numerous valuable suggestions towards its improvement.”

35 Gödel, *cw IV* (July 9, and Nov.7, 1934).

Carnap and Neurath saw Gödel as a normal member of the Vienna Circle, although Carnap refers to a latent opposition represented by Menger, Gödel, and Schlick.³⁶ Some months later, in June 29, 1935, Carnap focuses on his discussion with Gödel on Popper and Schlick, telling that Gödel is not satisfied with Princeton and had postponed his arrival to September because of his illness and that he has worked mainly on quantum mechanics, like v. Neumann, and wants to go on with this. After that Tarski appeared, followed by a joint discussion on the notion of truth.

On the first of July, 1935, a discussion group consisting of Heinrich Neider, Bela Juhos, Rose Rand, Else Frenkel, Walter Hollitscher, and Gödel met at the apartment of Viktor Kraft. Carnap explained Hempel's article³⁷ and continued later with Gödel in a coffeehouse, where he spoke on Russell and his idea to remove coordinate systems from theoretical physics, which seemed viable for Gödel, although unnecessarily complicated.

The subsequent correspondence between Carnap and Neurath in July is related to the forthcoming big First International Congress for Unity of Science ("philosophie scientifique") in Paris, taking place at the Sorbonne, Sept. 16–21, 1935 with an opening speech by Russell.³⁸ (Neurath 1935). The invitation to Gödel was intended by Carnap and Neurath, and Carnap wrote to his friend that he discouraged Karl Popper to speak on quantum mechanics, whose considerations regarding the topic are not correct according to Gödel and Frank (Carnap-Neurath, July 1935).³⁹ Another issue was the unification of terminology and symbolism of logic and logistics (Logistik), which was organized by Carnap with invitations to Becker, Behmann, Bernays, Gödel, Helmer, Hempel, Neurath, and Tarski. (July 18, 1935). This initiative led to the formation of a committee headed by Behmann, Bernays, Carnap, Neurath, and Heinrich Scholz, to which other logicians like Hempel, Helmer, Gödel, Lindenbaum, Łukasiewicz, and Tarski were to be added.

During this congress, there was an invitation for Marcel Natkin who lived in Paris, a good friend of Gödel since Vienna times; Carnap, Gödel, Reichenbach,

36 Neurath to Carnap, March 7, 1935, and Carnap to Neurath, March 13, 1935.

37 In 1935 Hempel published the following articles: "On the Logical Positivists' Theory of Truth", "Some Remarks on 'Facts' and Propositions", both in: *Analysis* 2, 1935, pp. 49–59 and 93–96.

38 A comprehensive report was published by Neurath in *Erkenntnis* 5, 1935, pp. 377–428. A summary can be found in Stadler 2015, pp. 171–178.

39 Popper spoke on the empirical method (empirische Methode) after the publication of his *Logik der Forschung* (1934) and following his article "'Induktionslogik' und 'Hypothesenwahrscheinlichkeit'" (1935), which appeared in *Erkenntnis* (1935/5, pp. 170ff.) based on his paper in Prague 1934.

and remarkably Schlick's daughter Barbara participated in this congress (Sept. 19, 1935).

After attending Neurath's lecture on the Unity of Science at the presence of Carnap, Neurath, Hempel, Walter, Hollitscher, Gödel and Bernays, Carnap met with Russell in his hotel, who expressed objections to Carnap's conception of logic given his point of view of a realistic logic (Sept. 20, 1935). Later, Reichenbach and Tarski joined the two philosophers. With Russell, Carnap subsequently spoke on logic in the hotel and reported on the results of Gödel, with whom Russell was surprisingly not yet familiar (Carnap explained the "new logic" to Russell, who seemed reluctant).

Only at the very end of this year (Dec. 30, 1935), Carnap refers in his diary on a symposium on probability in the USA to Morris, Cohen, Northrop, Savery, and Irving (Princeton) who reported to him on Gödel's return to Vienna after another nervous breakdown. In this regard Thorstein Veblen, who brought Gödel to Princeton, is said to have remarked: "too much introspection".

9 1936–1939: Paris, Mathematics and Logic in the *Encyclopedia Project*

The following years from 1936 up to the outbreak of WWII are to be described mainly by the *correspondence* between Carnap and Neurath, complemented by the entries on Gödel in Carnap's diaries after his emigration to Chicago by the end of 1935.

In his seminars at the University of Chicago, Carnap after his emigration from Prague, regularly explained Gödel's results to his students (e.g., Feb. 1936). Neurath wrote to the above-mentioned committee for the Unification of Logical Symbolism incl. Gödel, reinforced by a separate letter to Gödel (Neurath-Gödel, June 5, 1936). Despite these organizational issues, the conflict on semantics between Neurath and Carnap increased leading up to the publication of his "Testability and Meaning".⁴⁰ This well-known divergence regarding the relation of semantics, empiricism, and the position towards Popper and Wittgenstein, culminated gradually between the main proponents of Logical Empiricism. Neurath explained his agreement with Gödel that one can speak with a part of a language on another part of it, with reference to meta-languages (Metasprachen) of the Warsaw group (Neurath-Carnap, 30. 4. 1936).

⁴⁰ Carnap, "Testability and Meaning", in: *Philosophy of Science* 3, 1937, pp. 1–40.

By the end of 1936, both friends began to plan the huge publication project of the “Foundations of the Unity of Science”, especially the introductory volumes to the big *International Encyclopedia of Unified Science*, accompanied by the Congresses for the Unity of Science in Copenhagen, Paris, Cambridge (UK), and Cambridge (Mass.). From the beginning, Gödel was projected as an invited author on mathematics parallel to possible contributions by Menger and Tarski (Morris to Carnap and Neurath, Nov. 1935). After that, Neurath was prepared to write an invitation to Gödel (Nov. 12, 1936), and mentions to Carnap Gödel’s mental illness (Dec. 22, 1936). In his last letter to Carnap in 1936, Neurath was shocked about the message on Gödel’s worsened disease with the statement that “this would be a sad end of a brilliant man” (Dec. 28, 1936).

As for the following year 1937, it is worth mentioning the exchange between Carnap and Neurath dealing with the placement of logic and mathematics in the Encyclopedia project, especially with reference to the projected authors: Neurath quotes from a report by Philipp Frank on Gödel’s health improvement and comments: “Intra cloacas et urinas nascimur”, and on the other side “Intra depressions et manias vivimus”. Therefore, with the following consideration, the question of inviting Gödel instead of Menger now arose: “He has a lot in common with us, esp. with you and Tarski” (Neurath to Carnap, Jan. 24, 1937). Carnap replied with a pleasant agreement (Jan. 27, 1937) but in the meantime Neurath had also asked Menger, who had declined and proposed Waismann,⁴¹ although he found Gödel and Tarski also well suited (Neurath to Carnap, Febr. 1, 1937).

Afterwards Gödel was proposed for the contribution to mathematics, which did not work out and ended up with Carnap’s own monograph *Foundations of Logic and Mathematics* (1/3, 1939). Herein Carnap dealt in three sections with I. Logical Analysis of Language: Semantics and Syntax, II. Calculus and Interpretation, and III. Calculi and Their Application in Empirical Science. Only in the subsection on Geometrical Calculi and Their Interpretations in the third section we find the following short paragraph on Gödel (Carnap 1939, p. 50):

Gödel has shown (1931) that it is not possible to construct a proof for the consistency of a calculus C containing arithmetic, within a meta-language possessing no other logical means (forms of expression and modes of deduction) than C. Hilbert’s aim was to construct the proof of

⁴¹ Menger had published a foreword for Waismann’s book *Einführung in das mathematische Denken. Die Begriffsbildung der modernen Mathematik*. Wien: Gerold & Co. 1936.

consistency in a ‘finitist’ metalanguage (similar to an intuitionist system, see below). At the present, it is not yet known whether this aim can be reached in spite of Gödel’s result. In any case, the concept of ‘finitist logic’ is in need of further clarification.

But before that publication, still in the beginning of February 1937, Neurath wrote to Carnap and Morris regarding the preparation of the forthcoming “Third International Congress for the Unity of Science”, to be organized in Paris, July 29–31, dealing exclusively with the project of the *International Encyclopedia of Unified Science*.⁴² (Neurath/Carnap/Morris 1970). As for the monograph assigned to Gödel, he thinks of a title like “Logic and Mathematics”, but concedes the need for another brochure on calculus, for which C.I. Lewis would not be a good choice. After Gödel had been invited as an author, the editors were informed by Olga Hahn-Neurath that Gödel is strongly deranged and not able to work (“schwer geschädigt, nicht arbeitsfähig”). After that, Neurath proposed Tarski as a possible replacing author (Neurath-Carnap, Febr. 9, 1937). Two weeks later Morris raised doubts about Menger and Gödel as authors and thought again of C.I. Lewis, nevertheless – which clearly showed his strong commitment to US pragmatism (Morris-Neurath/Carnap, Febr. 21, 1937).

Already on Feb. 5, 1937, Neurath had written to Carnap and Morris on the forthcoming Paris congress, where the project of the unification of logical symbols stood on the agenda. This was proposed by Heinrich Scholz who was judged critically by Neurath as a metaphysician. About logic, Neurath thought that if Gödel takes over mathematics, the brochure could be entitled “Logic and Mathematics”. But besides this, a special brochure on “Calculus” is desirable. He further expressed doubts about Lewis as an author. In a subsequent letter to both colleagues in the same February, Neurath proceeded with an informative recommendation:

Menger is concentrated in special problems of mathematics and not in the position to write about mathematics. I agree with him to invite Waismann, but as collaborator for the other volumes ... I think so we shall invite Tarski ...

I would suggest about a special pamphlet ‘Logic’ to speak with Carnap and Nagel. I think if Tarski writes this pamphlet we should use the title ‘Mathematics and Logic’. I understand your hesitations in the case that Menger or Gödel would make this pamphlet. I think about the possibility

42 Stadler 2015, 182 ff.; Proceedings: *Travaux* 1937.

that Ness could write a pamphlet about the Calculus and the function of the calculusses (sic!) and so on. But I am not certain whether Ness is ripe enough to write such a pamphlet for such a forum.

Later on, still in Feb. 1937, Carnap reports to Neurath that he had written a positive review of the book *Mathematische Grundlagenforschung, Intuitionismus, Beweistheorie* (1934) of the Dutch mathematician Arend Heyting in the *Erkenntnis*, which Gödel and Tarski found too positive.⁴³ Another sign for “logical tolerance” on the side of Carnap?

A look the program of the 3rd International Congress for the Unity of Science – Encyclopedia Conference, Paris, July 29–31, 1937, in connection with the 9th Congrès International de Philosophie/Congrès Descartes, shows that neither Menger nor Gödel took part, for different reasons.⁴⁴

But on the occasion of his trip to Europe (Paris, Vienna, Prague) in the summer of 1937, Carnap resumed his meetings with his friends in the Viennese coffeehouses, e.g., with participating Gomperz, Rand and Gödel in the Arkadencafé (August 28 and 29, 1937): he remembers Gödel as pale and slim, asking for academic jobs in Germany, Prague, and the US in mathematics and philosophy. After his return to Chicago, the Vienna contacts were continued, *inter alia* with Egon Brunswik and Else Frenkel-Brunswik, who reported on Gödel's strong interest in religion and occultism, as she thinks, as manifestations of his occasional schizophrenia.

In November of the crucial year 1938, Neurath wrote again to Carnap referring to his monograph. He mentions problems of the calculus, paradoxes of set theory, and Gödel's results that there is no complete system of mathematics (Neurath-Carnap, Nov. 1938). Once more, he asked about its strictness (*Strenge*) and tells that D. van Dantzig in Cambridge had developed a physics without gauge geometry, only topologically. In the forthcoming monograph, there should be more on mathematics, mathematical problems, like topology – and in the bibliography references to Dedekind, Frank, Gödel!!! (exclamation marks by Neurath, F.S.), Menger, and von Mises are to be made.

To this proposal Carnap replied with repeating their differences regarding semantics, and on his reference to Gödel in his forthcoming monograph *Foundations of Logic and Mathematics*, which turned out as rather marginal in the publication as already described and cited above. Carnap argued that

43 Carnap 1935, pp. 288f.

44 *Erkenntnis* 7, 1937/1938, and *Einheitswissenschaft/Unified Science/Science Unitaire* 1938.

Gödel is only understandable for experts, but that he would mention him several times in the text of his booklet.

In Chicago, Carnap and Gödel met in April 1939 and discussed logical problems with Hempel and Frank. Especially, the issues of meaning, intensional logic and language was addressed by Gödel (June 1, 1939) during extensive personal private meetings.

During 1939, we count only three letters of Carnap to Neurath related to the big “Fifth International Congress for the Unity of Science” in Harvard (Mass.) at the beginning of World War II, Sept. 3–9, 1939, where they both met and gave papers.⁴⁵ Carnap spoke on “Science and Analysis of Language”, and Neurath on “The Social Science and Unified Science”, which indicates their different topics and thematic divergences.⁴⁶ Gödel travelled back to Vienna despite the dangerous circumstances and later escaped as one of the last exits only in 1940 via Soviet Union to Princeton.⁴⁷ Therefore, neither Menger nor Gödel participated in this conference despite invitations, which was one of the last representative meetings of the Vienna Circle in exile. Only in Sept. 2–6, 1941, the smaller and last “Sixth International Congress for the Unity of Science” took place at Carnap’s University of Chicago. There, Carnap delivered a paper on “Can Logic be formalized?” whereas Neurath’s manuscript on “Aggregation Expressions of Physicalism”, obviously related to his forthcoming *Foundations of Social Science* (1944), had to be read in absence.⁴⁸

10 1940–1945: the Vienna Circle in US Exile – Contested Semantics

During his research stay in Harvard 1940/41, Carnap welcomed Gödel and other friends from Vienna like Feigl to Harvard. He attended a lecture by Gödel on the continuum hypothesis, discussed afterwards also with Norbert Wiener (Nov. 14). These conversations went on, covering also the contested term of “L-truth” as introduced by Tarski.

In September 1943 Neurath addressed again his conflict regarding semantics in his letter to Carnap: “it is for me startling, that Tarski in Paris tried to give examples, always from certain mathematical or logical generalisations, with ‘all’ or something like that, and Hempel trying to explain me semantics in relation to empiricism mentions the Goedel (!) business.” And one year later

45 Stadler 2015, pp. 189–192; *Journal of Unified Science* 8, 1939/1940.

46 Stadler 2021

47 Dawson 1997/99, ch. VII.

48 Stadler 2015, p. 192 ff.

(Feb. 4, 1944) dealing with pluralism in agreement with Carnap, he remembers Vienna discussions, now related to Carnap's book *Introduction to Semantics* (1942):

Do you remember how all of us in Vienna, esp. Menger, were angry about the influence of the Wittgenstein-Waismann ideas concerning mathematics upon young mathematicians. We might call those ideas 'defeatism in mathematics'. They were apt to discourage the students (to work in) mathematics. Later I (together with the Warsaw people and Gödel) criticized Wittgenstein's 'defeatism in speaking about language'; it led the young people to the position: if it is not possible to speak about language in an exact way, then let's try not to do it. Your warning against the dangers of an oversystematization, an oversimplification, and schematization is quite all right to some extent. But I have the impression that by exaggerating your warning you turn it into a new kind of defeatism.

This is only one (Gödel related) element of the tragic conflict between Carnap and Neurath in the '40s which ended with the total dissent between the two driving pioneers of logical empiricism since its origins in Vienna, Prague, and Berlin. But this is another story.

11 After 1945: Gödel vs. Carnap between Chicago and Princeton

After the death of Neurath on Dec.22, 1945, the interaction between Carnap and Gödel, who was appointed at the Institute for Advanced Study in Princeton, continued in the US only occasionally. In March 1948 Carnap and his wife picked up Gödel at home and discussed again foundations of mathematics, on his study of Leibniz's cosmology and Kant's synthetic a priori. This personal exchange (also with Gödel's wife) was reinforced substantially during the Carnap's research stay in Princeton 1952–54. Both focused on inductive logic (Oct. 10, 1953), or Gödel on abstract entities with evidence as a psychological fact (Oct. 10, 1952). This scientific communication dealing with Platonic views on logic was accompanied by strange political considerations by Gödel, like the relation of the Catholic Church to positivism and materialism (Nov.14, 1952), or on the bilateral relations of the US and the SU (April 4, 1953). Such strange considerations, by the way, was also the case with the visit of L.E.J. Brouwer, who expressed his positive views on dictator Franco's Spain. Carnap talked to Gödel on his own language I, John von Neumann's concept of entropy, and quantum mechanics (Oct. 28, 1953). On Thanksgiving Day 1953, Gödel reports

to Carnap on his projected, but unpublished contribution to the Schilpp volume on Carnap (1963) focusing on his criticism of nominalism. After having read Carnap's *The Continuum of Inductive Methods* (1952) Gödel challenged his notion of "family" in this context, which Carnap tried to explain in vain despite his reference to his "Empiricism, Semantics, and Ontology" published in 1950 (Jan. 19, 1954 and April 26, 1954). After that, Gödel read this article, but still insisted that Carnap's earlier claim of mathematics as being void without objects is refuted by that contribution. Once more, Leibniz appears as the main reference philosopher for a non-reductive psychology in general. (June 7, 1954). And one month later, Gödel disagreed with Carnap's inductive logic (August 5, 1954), continued by a discussion on Brouwer, and Quine's nominalism (August 30, 1954).

With Carnap's appointment at the UCLA (University of California in Los Angeles) in 1954 as the successor of his friend Hans Reichenbach, the contacts ended, perhaps also due to the new geographical distance. In his last, short and moving letter to Carnap, Gödel and his wife expressed their condolence about the death of Carnap's wife Ina, whom both appreciated much since Princeton days (Gödel to Carnap, June 1, 1964, *cw* IV, p. 358). This is the end of a remarkable encounter and exchange between the two different philosophers, logicians, and strong personalities from Europe to the US. The end of both impressive figures is another story to be told.

12 Conclusions

Based on this case study with new primary sources, we can draw some preliminary conclusions, which correct and enrich the image of Gödel (together with Carnap and Neurath) in the Vienna Circle and beyond:

1. The correspondence between Carnap and Neurath confirms that Gödel was counted by both as an ordinary core member of the Vienna Circle – despite philosophical differences – which made him the first choice as an author of the monograph on the foundations of mathematics for the *International Encyclopedia of Unified Science*.
2. In addition, it turned out that Gödel was not that strange outsider and solitary thinker, despite several breakdowns caused by his mental illness.
3. On the contrary, Gödel appears as a prolific writer, discussant, participant, and also registrar, e.g. in the Mathematical Colloquium, Vienna Circle, the Gomperz/Zilsel-Circles, the Bühler-School (esp. together with Else Frenkel), despite all his eccentric individual features.

4. We gain new insights into the wide range of Gödel's interests, covering philosophy and mathematics (e.g., Plato, Leibniz, Kant, Frege, Hilbert, Russell, Brouwer, Heyting, Wittgenstein, Driesch, and Husserl), and logic, quantum physics, psychology, parapsychology, religion and theology. His diaries uncover a strong tendency towards a permanent self-understanding and orientation in the context of all these overlapping disciplines and research fields.
5. We uncover more on the foundational debate in mathematics, especially on the relation between the overlapping positions of logicism, intuitionism, constructivism, and formalism.
6. We have more evidence on the dominant characterization of Gödel as a lonely arch-metaphysician vis á vis the Vienna Circle. His deviance from some transitory doctrines were not exceptional within the Schlick-Circle, given its theoretical pluralism.⁴⁹ Therefore, his self-characterization as a conceptual and mathematical realist is to be contextualized in the multifaceted profile of the Schlick-Circle with reference to the foundational debate in mathematics.⁵⁰
7. This study hopefully shows the added value by the inclusion of new primary sources (correspondence, notebooks, diaries) unveiling a new and more precise image of Gödel's life and work, and provides a better understanding of his philosophical background besides formal logic and mathematics, beyond any cult of genius (e.g., through Gödel's interaction with J. v. Neumann, Carnap and other members of the Vienna Circle).⁵¹

49 As to the variety of positions towards Kant cf. Stadler 2018.

50 In this regard Gödel's retrospective answers in the Grandjean questionnaire (1974/75) in Wang 1987, pp. 16ff., are elucidating, where he mentions as important, Carnap's lectures of metalogic (despite his disagreement with his *Logical Syntax*) besides Hilbert-Ackermann, and as only important influential philosophers Leibniz and Kant. In addition, he refers to Heinrich Gomperz and Philipp Furtwängler as influential for his philosophical and mathematical development. In an alternative reply on the relation to the Vienna Circle and Wittgenstein, Gödel is cited as follows: "Generally speaking I only agreed with some of their tenets. E.g., I never believed that math is syntax of lang. In the course of time I moved further and further away from their views. ... Wittg's views on the phil of math had no inf on my work nor did the interest of the Vienna Circle in that subj. start with Wittgenst (But rather went back to Prof. Hans Hahn)" (Wang 1987 p. 20). On the foundational debate in context cf. DePauli-Schimanovich/Köhler/Stadler 1995.

51 Regarding the strong interaction between Carnap, Gödel, and the Vienna Circle see their correspondence in Gödel's *Collected Works* (IV, pp. 335–359) and the excellent reconstruction of Eckehart Köhler (Köhler et al. 2002, Bd. 1, pp. 83–128). In addition, the correspondence with Karl Menger (CW V, pp. 83–133) and 2 letters of Otto Neurath, July 9, 1934/June 5, 1936 (Vienna Circle Archives, Reichsarchiv Nord-Holland, Haarlem, NL).

8. Future perspectives will emerge from the three running research projects, the “Gödel Enigma”, “Gödel’s Legacy”, and the “Gödel-Forschungsstelle Berlin” (Notebooks), as well as from the correspondence and diaries of Carnap, in close connection with Carnap-Neurath letters.⁵²
9. The lesson to be learned is the not surprising fact that beyond publications, especially Gödel’s *Collected Works*,⁵³ additional unpublished sources remain as a most insightful corpus for any future historiography.

52 The main running projects are:

1. *Gödel Enigma* (University of Helsinki). ERC project conducted by Jan von Plato: <https://www.helsinki.fi/en/researchgroups/godel-enigma>. “A selection of the notebooks is to be deciphered and transcribed from Gödel’s complex Gabelsberger shorthand: there are several thousand pages of notes by Gödel; written in a long-forgotten, difficult stenographic script” (J.v. Plato). The notebooks cover so far: *Resultate Grundlagen*: 4 notebooks, 368 pp. on logic and foundations (1942/43); *Arbeitshefte*: 16 Notebooks, 1200 pp. On logical and mathematical topics (ca. 1935–1945); *Logik und Grundlagen (Excerptenhefte)*: 6 notebooks, 440 pp. on own and other’s work; *MaxPhil (Philosophical Maxims)*: 15 notebooks (one lost) on philosophical and foundational topics (1934–1955); earlier *Excerptenhefte* and isolated notebooks. The running transcriptions and research is dealing with Gödel’s notebooks on incompleteness (Jan von Plato), Gödel’s ontological proof and theological notebooks (Annika Kanckos and Tim Lethen), Gödel’s *Arbeitshefte* on intuitionism and constructivity in mathematics (Maria Hämeen-Anttila), Gödel’s “Princeton Lectures on Intuitionism” (Jan von Plato and Maria Hämeen-Anttila). Already published in cw: Gödel’s Zilsel lecture 1938, and the Göttingen lecture of the continuum hypothesis 1939. Additional transcriptions are made by Tim Lethen (2021) on Gödel protocols 1937/38 (e.g., on Carnap, Frenkel, Rand, Schächter, Waismann, Taussky, on several meetings at Zilsel’s home, on the lecture series in Bühler’s Psychology Department and on Hans Thirring in the Parapsychological Society, the topic of “Dämonologie”). In the proceedings of the conference on the Vienna Circle and Religion in 2019 (“Wiener Kreis und Religion”, Vienna, October 25, 2019) ed. by Esther Ramharter (2022), Tim Lethen contributed on Gödel and theology, and on his ontological proof (Ramharter 2022).
2. *Kurt-Gödel-Forschungsstelle, Berlin-Brandenburgische Akademie der Wissenschaften*, conducted by Eva-Maria Engelen: <http://www.bbaw.de/forschung/goedel/uebersicht>; The 15 notebooks “Philosophical Maxims” (*MaxPhil*) are to be transcribed and related research literature ed. by E.-M. Engelen. In the meantime, first notebook appeared: Kurt Gödel, *Philosophische Notizbücher/Philosophical Notebooks*, Band 1 Philosophie/Maximen (DeGruyter 2019). In addition, there is a publication on the genre of autobiography, notebooks, and diaries: *Deutsche Zeitschrift für Philosophie*, 67 (2), 2019; Anne Siegetsleitner, “Carnap’s Autobiographie als Autobiographie”; Eva-Maria Engelen, “Kurt Gödel’s philosophische Notizbücher als Denkraum und Exerzitiium”; Ilse Somavilla, “Wittgensteins Tagebuchschreiben als Weg der Vervollkommnung”.

53 Main references to research literature including primary sources: Kurt Gödel, *Collected Works* (cw): *Vol.I* (1986): with reviews of Carnap (1931), Heyting (1931), v. Neuman (1931); a review of Carnap, “The antinomies and the incompleteness of mathematics” (1934); *Vol.III* (1995): “Lecture at Zilsel’s” (1938), Gödel’s unpublished contribution to Carnap’s Schilpp volume (1963); *Vol.IV*: Correspondence Gödel and Carnap (1932–1964), and

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- FWF-Project P 31716 “Carnap in Context II: (Dis)Continuities”, Institute Vienna Circle, University of Vienna, Christian Damboeck (PI): <https://homepage.univie.ac.at/christian.damboeck/>
- FWF-Project P 30377 “The Carnap/Neurath Correspondence”, Dept. of Philosophy, University of Graz, Johannes Friedl (PI)
- ERC-Project, University of Helsinki Jan von Plato (PI): <https://www.helsinki.fi/en/researchgroups/godel-enigma>
- Kurt-Gödel-Forschungsstelle Berlin-Brandenburgische Akademie der Wissenschaften, Eva-Maria Engelen (PI): <http://www.bbaw.de/forschung/goedel/uebersicht>

A selection of relevant books: Mancosu (1998, 2010); Menger (1979, 1994, 1998), Köhler/Weibel/Stöltzner/Buldt/Klein/DePauli-Schimanovich (2002); DePauli-Schimanovich/Weibel (1997); Sigmund/Dawson/Mühlberger 2006; Köhler et.al (2002); Sigmund (2017); Engelen (2019a). Some related articles: Crocco (2003); Goldfarb (2005); Awodey/Carus (2004, 2006); Cat/Tuboly (2019); Siegetsleitner 2019; Engelen 2019b.

with Feigl, Menger, Popper. J.W. Dawson, *Logical Dilemmas. The Life and Work of Kurt Gödel* (1997/1999): references to the Carnap Diaries. F. Stadler, *The Vienna Circle* (2001/2015): “Protocols of the Schlick Circle 1930/31”, and “Karl Menger’s Vienna Circle”. E.-M. Engelen (Hrsg.), Kurt Gödel, *Philosophie/Maximen o – Philosophie/Maxims o*. Band 1 (2019). Related research literature (selection): P. Mancosu (1998); G. Crocco (2003); W. Goldfarb (2005); S. Awodey/A.W. Carus (2004, 2006); J. Cat/A.T. Tuboly (2019). An electronic version of the proceedings of the International Conference “Gödel’s Legacy – Does Future lie in the Past?”, University of Vienna, July 25–27, 2019 is available: <https://phaidra.univie.ac.at/detail/01374870>. Amongst the contributions: Juliet Floyd, “In and Out of Mind: Wittgenstein and Gödel, Post and Turing” (= 27th Vienna Circle Lecture); Jan von Plato, “Gödel’s ‚Resultate Grundlagen‘ notebooks: A Legacy to be”; Maria Hämeen-Antilla, “Gödel’s Notes on Constructive Foundations of Mathematics”; Annika Kanckos, “The Development of Gödel’s Ontological Proof”; Tim Lethen, “Gödel’s Notebooks on the Foundations of Quantum Mechanics” (1935 f.); Oliver Passon, “Gödel on the Interpretation of Quantum Mechanics”; Gregory Lavers, “What Gödel got right about Carnap”.

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Neurath on “Plato-Hitler” and the British Scene of Irritation

Adam Tamas Tuboly

Abstract

This chapter presents Otto Neurath’s crusade, or campaign about the relation between Plato, the general Platonic attitude and Fascism/Nazism. I will reconstruct his papers on German (re)education and Plato with the replies that were published in *The Journal of Education*. Some lessons and main points will be presented that could be abstracted from the debate. As I will demonstrate, all the replies to Neurath exemplified the very same Platonic attitude they criticized and thus it made the whole debate (and the call for a more reflexive critical and rational discourse on the topic) impossible.

1 Introduction

National characteristics and ideological connections between abstract ideas are admittedly hard to disentangle into a clear and one-dimensional line. Whatever we think about the philosophical legitimacy of this entire discourse, it plays without doubt a substantial part in contemporary socio-ethical and political discussions. Understanding the sources of these debates, their internal logic and fields of applications is thus not just current but important as well. In this paper, I will concentrate on reconstructing one such historical event from twentieth-century history of analytic philosophy that might shed some light on this discourse.¹ What I have in mind is Otto Neurath’s crusade (or campaign) about the relation between Plato, the general Platonic attitude and Fascism/Nazism (as participants of the debate often used these two terms interchangeably, I will do the same here).

In Section 2, I reconstruct Neurath’s and Joseph A. Lauwerys’ papers on German (re)education and Plato with the replies that were published in *The Journal of Education*. Some lessons and main points will be presented that

¹ This is a rather shortened and dense version of chapter 8 of Burke and Tuboly (2025).

could be abstracted from the debate in Section 3. As I will demonstrate, all the replies to Neurath and Lauwerys exemplified the very same Platonic attitude they criticized and thus it made the whole debate (and the call for a more reflexive critical and rational discourse on the topic) impossible. Finally, Section 4 provides a sort of a conclusion.

2 The Long 1940s: Readings of Plato and the Nazi-Charge

When Neurath arrived in England on May 15, 1940, he had a recently acquired German passport and thus he was considered to be an "enemy alien". As such, he was interned in various prisons and camps for eight long months, as was his close colleague and future wife Marie Reidemeister.

Neurath found a well-prepared field: in the 1930s, Moritz Schlick, C.G. Hempel, Rudolf Carnap and Neurath himself went to England for lecture-tours. Neurath also organized the *Fourth International Congress for the Unity of Science* at Girton College, Cambridge together with Susan Stebbing on July 14–19, 1938. At this time, members of the British intelligentsia and many philosophers conducted specialized debates about the (supposed) relation between Fascism and German philosophy (conducted often on the pages of newspapers and magazines). It is not known whether Neurath knew about the discussion when he arrived in England, nevertheless he turned quickly to these questions.²

2.1 *The First Papers on Germany and a Fear about Plato*

The first explicit mention of Plato and the relation of his philosophy to contemporary trends is to be found in Neurath's 1942 article, "International Planning

2 It has to be noted thus that it was not Neurath who started the whole issue about certain philosophies' relevance to a more general oppressive and dogmatic attitude on the Continent. Even around World War I and during the interwar period, many of England's well-known philosophers and public intellectuals tried to establish a certain connection between national characteristics and national philosophies, and between Plato and the intellectual rise of Nazism. About these events before Neurath, see Akehurst 2010 and Vrahimis 2015. For a balanced and general account of the debate about Nazism and Ancient philosophy in the context of education, see Tachibana 2012. See further Demetriou 2012 for the critical discussion of Plato, or better the "Platonic legend" between 1930 and 1960. After Neurath, it was Klaus Christian Köhnke who described in great detail the connection between establishing a "Reich" at the end of the 19th century and the rise of an "idealism" that also led a major interest in Kant and Plato among German scholars. That situates Neurath's story, or at least its origins, a few decades earlier. See Köhnke 1986.

for Freedom”, where he claimed that “Plato disliked democracy, the kind of state in which there is the greatest variety of human nature and together with it – as he stressed – much disorder and muddle”.³ Although the next public appearance of the idea is from 1944, Neurath continuously discussed it in his correspondence with Rudolf Carnap (and, of course, with others) in the 1940s.⁴ What prompted Neurath’s intention to go public in 1944 was the critical comparison of the various editions of F.W. Putzger’s *historical school atlas*.

His co-author was Joseph A. Lauwerys, whose family emigrated from Belgium to England in 1914. Lauwerys, an ardent defender of BASIC English, earned various degrees and started to lecture on scientific method at the London Institute of Education. He was interested in films, modern media and became quickly an internationally recognized scholar and leader on comparative education; he became a member of a committee that discussed the possibilities of educational reconstruction after the war. As such, he was involved in the development of UNESCO, and he edited the *World Year Book of Education* from 1947 to 1970.⁵

Neurath and Lauwerys started to work together already in 1941 as they jointly organized the so-called “Terminology” workshop in Oxford, Neurath’s first major event as a scientific refugee in England. In 1944, they started to discuss publicly their common ideas on education and Nazism. The 54th edition of Putzger’s atlas, the subject of Neurath and Lauwerys, shows already the explicit signs of Nazi intellectual occupation of cultural fields. As the earlier editions had a map of Palestine and others about the journeys of St. Paul, “the new edition gives no honorable space to the Jews. Instead we find four pages devoted to straightforward propaganda” describing the expansions of the “Eternal Germany”.⁶ There were new maps showing the tours of Hitler and “one rather disgusting diagram tells the children in which places Nazis were killed during the troubles they themselves had started”.⁷

For Neurath and Lauwerys, the new atlas was much more aggressive and nationalist in its tone, placing Germany in a central place thus interpreting all the historical and geographic changes in Europe as related to the claims of Germany; by doing that, authors of the map often disclaimed the rights and values of others. Neurath and Lauwerys ended their article by calling attention to the difficulties of how to defeat Nazism with democratic tools: “What we

3 Neurath 1942/1973, p. 430.

4 See the letters in the Carnap-Neurath correspondence in Cat and Tuboly 2019, pp. 512–685.

5 For more on Lauwerys, see Cowen 2020.

6 Neurath/Lauwerys 1944a, p. 421.

7 Neurath/Lauwerys 1944a, p. 421.

have to do is to transform a whole environment [...] Nazism is a virulent manifestation of an endemic disease affecting the whole of the western civilization."

One month later, Neurath and Lauwerys (1944b) published the second part of their paper. In that short piece, they described various cases of historical figures that have been considered later as strong, modern, and great men despite their brutality, intolerance, and the persecution carried out in their names. They used as examples a book by Ernst Kantorowicz about Frederick II, the Holy Roman Emperor, and the philosophy of Plato. Although the former did not play a significant role in their article, the archive files of Neurath contains a shorter piece, titled "The Boy from Apulia", which is a critical discussion of Kantorowicz's book. Thus, before I move to the Plato articles, I will quickly overview this manuscript because it also sheds some light on Neurath's target and audience.

When he was in England, Neurath did a strange social experiment: in his correspondence, he asked many of his persecuted, mainly Jewish friends about their opinion of Frederick II. Most of them claimed that although turmoil played a significant role in his life, it was the external development of history that enforced upon him some form of necessary ruthlessness. Thus, in general, Frederick II was a genius, a great man, who achieved important things and could stand up proudly against the waves of history. When Neurath told them all the stories of persecution, intolerance that were attributed to Frederick II, and described to them the "beastly cynic", all of his friends were shocked. Both Neurath and his friends read the same stories and books about Frederick II, e.g. Kantorowicz's biography, but his friends had "totally forgotten the terrible stories published in this book ... strange, is it not?"⁸ What is the explanation of this magical loss of memory of those details that would question the greatness of historical figures?

Long time ago I thought that German Jews did not know the factual material, but that is not the case, in BURCKHARDT and KANTOROWICZ all these details are recorded, BUT THEIR AUTHORS TRY TO CREATE AN ATMOSPHERE OF POSITIVE REACTION TOWARDS THE RENAISSANCE, THE FORERUNNER OF WHICH SHOULD BE FREDERIC II. And this JUDGMENT of these two authors is so powerful in the German atmosphere, that very, very many Jews take the admiration of the Renaissance as granted and did not apperceive the details, they READ THEMSELVES. I think that this preparedness to accept ruthlessness as something connected with

8 Otto Neurath, "The Boy from Apulia," ONN, K.60.

GREAT MEN, is in the German atmosphere more usual, than in others, where FRIENDSHIP, BROTHERHOOD, TOLERANCE founded [...].

NEURATH, "The Boy from Apulia," K. 60, original emphases⁹

Neurath's idea was that in biographies and historical records, even if some of the repugnant acts of the given figure are noted in some details, they are often presented as necessary evils for the greater good, enforced on the historical actor by external conditions. This is the general way of how torturers, warlords, persecutors, intimidators, and "ruthless cynics" became transformed into "geniuses", "men ahead of their time", "revolutionary formers of history", always emphasizing their progressive institutional and infrastructural developments instead of the way they treated their own people. As it often happened, Neurath exaggerated here his narrative to make a radical point. Emphasizing the "'historical' outlook" of the German people, "usually used for sheltering some rascals",

one may also explain that Hitler was a great man, in his period a certain ruthlessness normal etc. Organizer of centralized civil service, unification of national pattern etc., I know how to make a "historical" proper description.

NEURATH, "The Boy from Apulia," K. 60

Even though Kantorowicz's book was translated into English,¹⁰ English and American citizens will not identify themselves with Frederick II because they have "strong democratic currents", said Neurath and Lauwerys, but "in Germany the case is different".¹¹ For that reason, they did not use the case of Frederick II in their articles; instead of him they utilized someone else as their focal point, someone who was well known both in the German and the English-speaking countries, namely Plato.

9 It would be important and interesting to compare at this point Neurath's ideas with those of Kate Manne 2018 who investigated how people's sympathy is often oriented to the male persecutors against their female victims (she calls this attitude "himpathy"). I do not claim, of course, that there would be any substantial and direct link, but Neurath also seems to suggest that persecutors, who were "great men", often received much lighter treatment than one would expect. While Manne would go in the direction of a social analysis, Neurath opted for a more general educational and philosophical idea that goes back to Plato's *Republic*. Comparing Neurath and recent feminism, however, is not without precedent; see Okruhlik 2004.

10 See Kantorowicz 1931.

11 Neurath/Lauwerys 1944b, p. 574.

Though there are different backgrounds and presuppositions for studying Plato in England and in Germany, both German and English-speaking scholars noted that Plato was a "totalitarian reformer".¹² Neurath and Lauwerys claimed that while Plato is held in high esteem and his *Republic* is studied widely among students as well, the book offers a view that contains the following elements:

- (A) The purpose of the State is to preserve the purity of the race and to organize the people for war against foreign barbarians;
- (B) *Élite* classes of the people must be specially trained for fight and that they must be brought, as very young children, to scenes of slaughter and battle, so that, like young hounds, they may develop a proper blood lust.
- (C) In order to attain its supreme aims, the State must control all marriages, literature, and music and a strict censorship of books should be established.¹³

Given these points, they argued, even if after the war one would not explicitly talk about Nazism, racism, and Hitler, "these German writers," by disseminating uncritically the mentioned views of Plato, "would be carrying on Nazi propaganda."

Neurath's and Lauwerys' article was followed in the next month by a short critique put forward by F.W. Garforth¹⁴ who later become known as the author of various books on famous philosophers' educational ideas (Mill, Dewey, and Locke). Garforth denied that Plato ever accepted (A) in the *Republic*. His main claim was that Plato's *Republic* could not have been oppressive and totalitarian because the aim (and thus the educational background) of the philosopher kings was "the vision of the Good (which is the source of all truth and understanding), of the Beautiful and the other Forms. [...] Plato's goal for the State was the organization of government and of society according to the ultimate laws of reality".¹⁵

2.2 *The Actual Paper on Plato and Its Reception*

A month later, Neurath and Lauwerys replied to Garforth's short note with a longer article centered on three major issues.¹⁶ First, they tried to restate (A) by claiming that even if Plato did not claim *that explicitly*, the implication

12 Neurath/Lauwerys 1944b, p. 575.

13 See Neurath/Lauwerys 1944b.

14 See Garforth 1945.

15 Garforth 1945, p. 16.

16 See Neurath/Lauwerys 1945a.

and actual meaning of his views come really close to that. To make a somewhat rhetorical point about this, they repeatedly quoted the program of the NSDAP: there one finds such aims as the welfare of mankind, the flourishing of the sciences and the arts, and so on. What happened in the late 1930s and 1940s, however, is that they constrained these general claims in a highly specific and peculiar manner: the welfare of mankind was important for Aryans; German and purified sciences shall flourish indeed and so on. Not stating something explicitly does not mean that it is not lurking around in another form. So even if Plato did not say something, his other statements might imply or convey a certain commitment. On the other hand, they quote (on the request of Garforth) many particular claims of Plato (with their Stephanus numbers) to show that indeed there were such tendencies in Plato's *Republic* that point towards the motivating force of war and the intentional reservation of the purity of blood within each class. Given these considerations, concluded Neurath and Lauwerys, "one could hardly object to a Nazi teacher quoting Plato's authority as justification for praising institutions aimed at preserving a master-race and at fitting them to fight".¹⁷

Their second point was an interpretational game: one should read the *Republic* as "naively as possible," pretending that one does not know who the author was, that is, "without undue reverence".¹⁸ In this way, they thought, the name and historical significance of Plato would not blind the reader and she would recognize those disagreeable points that would be disrespectful in general to attribute to Plato. Their final conclusion could be quoted at full length, since others, replying to this article, did hardly recognize this:

In conclusion, we feel it essential to stress once again that we do *not* think that the *Republic* represents the whole of Plato, nor that the preparation of a warlike caste on a racial system represents the whole of the *Republic*; but we do think that these and other proposals, which seem equally terrible and repellent from a democratic point of view, are to be found there by naïve readers.

NEURATH/LAUWERYS 1945a, p. 58

Although their initial aim in 1944 was to call attention to certain specific problems regarding the re-education of German people and de-Nazification of German cultural life, after Garforth's defense of Plato and his *Republic*, Neurath

¹⁷ Neurath/Lauwerys 1945a, p. 58.

¹⁸ Neurath/Lauwerys 1945a, p. 59.

and Lauwerys were forced to move the debate towards much more sensitive grounds.

2.3 *Responses to Neurath and Lauwerys and Their Self-Defense*

After their response, the British intelligentsia took the lead and three separate "correspondence notes" appeared in *The Journal of Education*. G.C. Field from the Department of Philosophy, University of Bristol, who published a book about Plato already in 1930, aimed at producing a devastating critique of Neurath and Lauwerys. He had some particular and general points: regarding the latter, Field claimed that "it is clear that Messrs. Neurath and Lauwerys completely misinterpret Plato on some of the most important points" and "they are displaying a sad lack of understanding [...] and [that] underlies their whole approach to the study of Plato".¹⁹ The main focus of the criticism was Neurath and Lauwerys' suggestion that we should read Plato naively as possible. According to Field, this is a "fundamentally wrong approach" as we cannot disregard our own preconceptions, hence Neurath and Lauwerys read Plato in their own context, thus they place the *Republic* in the "for or against Nazism" net of considerations.²⁰

On the other hand, we *should not* read Plato naively, since that way we could miss the relevant context and background that makes his statements significant and understandable. Even though Plato's remarks might seem to be similar to some of the Nazi ideas,

[t]here is nothing about "purity of the race" in the sense that we have come to attach to the phrase in the *Republic* at all. It is true that Plato advocated the use of eugenic methods, to a degree that would shock modern sentiment, in order to breed for certain qualities. But that is quite a different thing. It is the qualities that he is interested in, not the ancestry which is only a means to these.

FIELD 1945a, p. 161

So, from Field's viewpoint, even though Plato had the idea of controlled quality-check in society and accepted the eugenic ideal of human-engineering, his conception is substantially and relevantly different because it was based

19 Field 1945a, p. 161.

20 It might be of further interest that Field (1945a) begins his article by raising the question: what could be Plato's opinion about Hitler and Mussolini. He claims that Plato seemed to reject ancient Tyrannies, thus he would reject Hitler and Mussolini as well as if there were not any differences in time, place, and context.

on certain vague *human qualities* and not on the blood-lines and the role of ancestries.

The other reply to the Neuraths came from C.E.M. Joad, a popular philosopher and public intellectual from London (also the Head of the Department of Philosophy at Birkbeck College, London), who was unsparing with general criticism, claiming that Neurath's and Lauwerys' paper was "vitiating by a simple error" and "if one wanted a short guide to what Plato did not believe, one could scarcely do better than read their articles".²¹ He also repeated some claims from Garforth's paper (that the aim of Plato's state and its rulers was to understand and perceive the Good and the Beautiful), and tried to make a point regarding the nature and necessity of war in the *Republic*. Though Joad tried to create a distance between Plato and Nazism – despite the alleged similarities put forward by the Neurath – his main aim was to show that presence of war between nations was a pure empirical fact. This fact, according to Joad, was wisely acknowledged by Plato, and the fact that a society trained a special class for war was a measure of civilization, sparing the life of those in the community who were unable to fight.

Since no one stepped in to defend Neurath and Lauwerys against any of the charges, they had to reply on their own. In the second part of "Plato's 'Republic' and German Education", they turned some of their former general weapons against the specific authors: Neurath and Lauwerys claimed earlier that Plato is an especially dangerous figure in the sensitive web of educational studies on the continent, because his texts are often read with "undue reverence". Field was concerned with the idea that this is just an unnecessary rhetorical move since no one in Britain considers Plato as an authoritative philosopher and, because of this, no uncritical judgment would be present in the debate. Neurath and Lauwerys pointed out that the vehement and many-sided correspondence notes perhaps pull in the other direction; that is, Garforth, McNicholas, Joad, and perhaps also Field take Plato to be an authoritative figure, even if "they may feel inclined to disclaim having done so".²²

Two major points should be emphasized here from Neurath and Lauwerys' answer to Field. He claimed that Neurath and Lauwerys's method is entirely mistaken on, at least, two levels: it ignores the historical and social conditions

21 Joad 1945, p. 163.

22 Neurath/Lauwerys 1945b, p. 222. Rev. P.J. McNicholas (1945b) was also involved in the debate as a defender of Joad, scrutinizing some of his points about the views of Plato regarding whether people are capable of changing their nature or not. See further McNicholas (1945a) where he also argued that war does not play such a morally significant role as Neurath and Lauwerys claimed.

both of Plato and of the authors. In the reply, Neurath and Lauwerys claimed that already in the early eighteenth century, Martin Wieland noted similar charges against Plato's educational and social ideas, that is, even before Nazism was present, the intolerant, dogmatic, and anti-democratic tendencies of Plato were recognized.²³ Thus, Neurath and Lauwerys were not forcing their own peculiar situation and experience on Plato; others, with different backgrounds and social experience pressed the very same interpretation.

On the other hand, they also summarized their methodological credo about what historical explanations amount to:

We were chided for not taking sufficiently into account the social and historical circumstances which decisively influenced Plato. Apparently the argument is that, if we only remember that to him a State without slavery was unthinkable, &c., we shall see why he wrote as he did. *Tout comprendre, c'est tout pardonner*. Is it? One may understand completely and how rattlesnakes bite, and yet consider them unpleasing animals not really suitable as pets for young people – and not really suitable either as specimens for observation by young students of zoology.

NEURATH/LAUWERYS 1945b, p. 222

Even though from a philosophical point of view Neurath's general idea of a unified science would dictate that there isn't any principled difference between the human and the natural sciences, discussing rattlesnakes and philosophers regarding their actions and our normative stance towards them in the same breath might still be somewhat problematic, or unexpected. Nevertheless, their point is clear: even if we *understand* the origins and context of Plato's thought, namely why and how he came to the conclusions that we consider anti-democratic and oppressive, it does not mean that we have to *forgive* him these thoughts. In other words, understanding something does not make it right. That is, even if it is understandable why Plato considered that certain ideas are worth following *in his time*, those ideas could still be regarded as anti-democratic and dogmatic, thus worthy of sharp criticism and warnings.

Taking seriously the historical conditions, however, Neurath and Lauwerys pointed out that even before and after Plato we find among the Greeks such philosophers and statesmen who "are in harmony with our modern democratic standards," thus, Plato's *own* situation could not itself explain all the

23 See, e.g. Wieland 1766/1767.

views he held. They concluded these considerations again by a somewhat rhetorical move:

Does Prof. Field assume that in Plato's time there were no democratic and cosmopolitan persons? Of course there were, just as there are to-day, even though *Mein Kampf* is a work which will probably be described by philosophers in years to come as a book to be interpreted in the light of the social and historical circumstances of the twentieth century and as being in harmony with the situation in the years 1920–1940.

NEURATH/LAUWERYS 1945b, p. 222

Neurath and Lauwerys replied to Joad as well: they pointed out that they did not mean those things that he attributed to them. They took up again the question of war and claimed that if Plato and the Nazis understand the structure and nature of war better than Neurath and Lauwerys – as Joad stated in his reply – then they are “rather proud and pleased to be told that”.²⁴ And finally after they accused Joad of using structurally similar arguments to those considerations that were put on the table by the Nazis, Neurath and Lauwerys thought that they could rest their case.²⁵

2.4 *How to Close the Debate? The Death of Neurath*

Closing the debate, however, was not a real possibility for them. As the discussion became more vehement and somehow more personal, McNicholas, Paul Prentice and Field took up the gloves again. Field made references to various issues and claimed repeatedly that Neurath and Lauwerys missed all of his points, and he thus came up with new ones. He called attention to important nuances, such as that understanding the context of a historical figure may not necessarily involve forgiveness, or that being anti-democratic does not commit oneself to being a Nazi and “shouting ‘Yah! Nazi!’” may not improve the quality and strictness of the debate. By describing further logical possibilities of certain philosophical points of an interpretational question, Field came to the

24 Neurath/Lauwerys 1945b, p. 224.

25 During the late 1940s and early 1950s, Joad followed Giles Romilly in accusing A.J. Ayer's ethical emotivism (presented mainly in *Language, Truth and Logic*) of preparing such a moral vacuum in Oxford that was easily filled by Fascist values. That is, while Joad defended Plato against all of Neurath's charges (which he considered both methodologically suspicious and philosophically erroneous), he immediately joined others in a very similar campaign against logical positivism without further ado. See further Joad 1950, Preston 2020, Tuboly 2020b.

conclusion that "Messrs. Neurath and Lauwerys had no conception of what the aims or the methods of the study of philosophy are".²⁶

Before the journal's editor ended the correspondence, Neurath and Lauwerys were able to react once more to these attacks. In August, they took a step back and called the participants' attention to the fact that the whole issue at the start was about the re-education of Germany and not about Plato's philosophical, historical, and philological interpretation. Plato became relevant for them only as a typical author in this context: "German youth, not acquainted with democratic arguments, are unlikely to learn from the *Republic* anything about that co-operation of free citizens or that tolerance towards people of different types which are the foundation of life in free societies".²⁷ From the *Republic*, German youth would learn mainly how to praise absolutistic and dogmatic ideals, such as "the State", "the nation", "the arts", "the race", "the war" and so on.

This final note is much more relaxed and timid; they emphasized their democratic concerns and raised the *possibility* that they have put too much weight on Plato's work in the educational context. Neurath and Lauwerys were perhaps right in lowering their tone. Seemingly, they made many people angry by their style, by putting Plato and the Nazis on the same table, and thus they stepped on too many important scholars' toes.

Interestingly, no reply or correspondence note appeared in the journal

Neurath and Lauwerys. In an unpublished "Letter to the editor", Neurath noted that "[t]hree against us, we hope very much that also scholars exist – perhaps not traditional philosophers – who will protect us within the framework of this periodical otherwise one gets perhaps a wrong impression of the public opinion on this point. What would a Gallup poll show us ... etc something like that, kindly, but nevertheless decisive."²⁸ The published version of this remark was much more polished (perhaps the above quotation is Neurath's original and the published is a joint work), but more concerned at the same time: "We seek comfort, however, in the thought that quite possibly a Gallup Poll might show that many of your readers agree with our description of, and analytical remarks on, Plato's *Republic*, though they have not yet written to you".²⁹ Neurath and Lauwerys were indeed much concerned whether they will get any support from the public and from similar-minded scholars, or will they remain alone in the crusade – and thus the fact that people will see them only as two

26 Field 1945b, p. 294.

27 Neurath/Lauwerys 1945c, p. 394.

28 Neurath, "Letter to the editor" (no date), ONN, K.78.

29 Neurath/Lauwerys 1945b, p. 222.

eccentrics weakens their points. When they got the proofs of Field's first article, their first note on the paper was "Where remain our supporters?"

It is unknown whether no one was asked to defend Neurath and Lauwerys or no one accepted the invitation, but the following note was published in May 1945:

SIR, – I do not feel inclined to join in the controversy about Plato's *Republic*, though I strongly agree with Neurath. My reason for not joining is that I have a long history of philosophy in the press, which incidentally, sets out a similar point of view, and I prefer the fuller statement which is possible in a book.

This short note was written by Bertrand Russell whose *History of Western Philosophy* was published in the United States in 1945, and a year later in England.³⁰ Though Fascism and Nazism do not surface explicitly during the discussion of Plato's ideas, Russell indeed seems to suggest that Plato's utopia is entangled with totalitarian and oppressive ideas. But for Russell, the *Republic* was much like a Communist state than a Fascist, and his whole terminology in Chapter 14 of the book is based on the comparison with threatening Communist notions. It is still understandable though that Russell did not want to engage readers and colleagues on the pages of *The Journal of Education* within such a limited range. Ironically, his monumental book did not earn him that kind of respect and recognition from the academic and professional historian/philosopher audience that he might have wished for.³¹

Even after the end of the war, Neurath prepared talks, lectures and articles about the topic. Shortly after his debate with the British intelligentsia, the whole issue was continued by Russell's abovementioned book, and even with Karl Popper's *The Open Society and its Enemies*.³² Though there were differences between Neurath and Russell and Popper, their general line of thinking and concerns went in similar directions and they could have supported each

30 For the note see Russell 1945, p. 224, for the book, see Russell 1945/1946.

31 On the critical reception, supporting this point, see Wahl 2019.

32 See Popper 1945. On Popper and Neurath, see Soulez 2019. The Neurath-Popper relation, especially with respect to Platonism and Fascism, is an extremely interesting issue: given all their shared experiences, background, and the forthcoming similarities, it might be surprising that one does not find any references to Neurath in Popper's book. Neurath's name appeared only in the later editions when Popper started to react to his critics (one of which noted the similarities to Neurath). See Burke and Tuboly (2025).

other by not letting any of them stand alone on stage.³³ For Neurath, the debate ended with his unexpected death in December 1945, without any proper reconciliation and agreement even on the most basic issues and problems.

3 Points and Lessons of the Debate

Neurath was often poked because of his temper and indefatigable search for a bit of intellectual fight. Nonetheless, he could also be smooth and pliant to make and maintain friendships. In between was the Neurath who wanted to make a scientific point that naturally requires some sort of friction but without further ado or unnecessary intellectual mocking. He wrote about this to Carnap as follows: "You see I am a little doubtful, about 'coercion' exerted by me. Reason: when writing in a very conciliant [conciliatory] way (no noise, no bullying), e.g. in the Plato article, the people who answer, answer exactly in the tone of irritation."³⁴ Although Neurath did not provide further explanations to Carnap, one might sense that the irritation of those who replied to Neurath goes back at least to two factors: namely treating Plato in such an authoritative and sensitive manner that they would not admit, and by reproducing exactly those features of Plato that Neurath was criticizing.

3.1 *Authorities*

What was the reason for the irritation of British scholars? After all, Neurath and Lauwerys's tone was indeed modest in their papers, especially compared to Neurath's general style in correspondence and in his polemical publications. Nonetheless, he touched upon sensitive points and pressed such issues that were in the air for years, and many people had a chaw at it in their publications already. But as it was just a wide possibility during the '30s that some of Plato's views might present some sort of similarity to Fascist and Nazi views; in the '40s – when millions of people were killed at war, when persecution of selected groups of people became explicit and internationally well-known – drawing

33 It should be noted, which is not emphasized enough by any of the mentioned, that not every Plato scholar accepted the Plato-package as it stood. Hermann Cohen, for example, was one of the most distinguished Plato scholars of the late 19th century, but he explicitly rejected Plato's political philosophy. See Cohen 1916/1923.

34 Neurath to Carnap, 24 September 1945, in Cat and Tuboly 2019, p. 670. The topic of Nazism and Platonism was a regular topic between Neurath and Carnap in the 1940s, especially as Neurath accused even Carnap of being a Platonist and thus exemplifying many of those overtly rigid, hierarchical, oppressive, and dehumanizing features that emerged in Germany.

the same parallels could have been more shocking, compelling philosophers to react more harshly and to distance themselves from the comparison.³⁵

One might say that no one likes to be told by a strange, eccentric, and somewhat aggressive figure that your hero is no better in fact than a Fascist or a Nazi – especially at a time when Fascists and Nazis are killing your friends and colleagues on the front on a daily basis. In fact, all the defenders of Plato seemed to state that Plato is not their hero – theoretically they do not consider Plato as an authority (in their views, dogmatic dependence on the thoughts of others was a feature of citizens of oppressive states). I say ‘theoretically’, because it quickly turned out in the debate that Plato was not just a randomly chosen philosopher who was defended by British scholars as a simple historical figure. In his reply article, Field claimed, for example, that “I am quite serious in stating that no one I know or have heard of treats Plato [as?] ‘authoritative’”.³⁶ Take now the above-mentioned Prentice. He jumped into the debate by proudly stressing that he is an “ardent admirer” of the *Republic*, and that he “feel[s] piqued for the honour of that great work. Nothing would ever persuade me,” said Prentice, “that anything but good, and good of deep and lasting character could be derived from its study”.³⁷ He goes on to call Plato’s book a “masterpiece”, and calls attention to the defectiveness of Neurath and Lauwerys, who were simply ignorant and missed the *Republic*’s “essential and vitalizing message as it comes floating down the ages, and makes for nobility of character, of will, of soul”.³⁸

But if that would not be enough to make a point against the blind enemies, Prentice closes his short correspondence by the following:

To some it seems incredible that anything but the highest honour can be accorded this work [Plato’s *Republic*] when seen as a whole, but, alas! we recall that time of old when a voice came from heaven, and the people that stood by and heard it said that it thundered. It was only a few others who said an angel speaks to him.

PRENTICE 1945, p. 228

35 At the end of the 1930s, H.B. Acton (1938) published sort of a summary paper about the “alleged Fascism of Plato”. He argued that all of these charges are somewhat misleading and weak until we come into possession of a sufficiently detailed philosophy of Fascism. Without that conception, we cannot decide whether “these likenesses [between Plato and Fascists] are mainly accidental, or are due to some deep-seated similarity of outlook” (Acton 1938, p. 302). Although Acton defends Plato, his tone is much more calm and low-key than those who contributed to the issue in the ‘40s.

36 Field 1945b, p. 292.

37 Prentice 1945, p. 226.

38 Prentice 1945, p. 228.

Prentice did not have deep or detailed philosophical points and arguments; the only implication of his note was that those generalities and lofty statements in the *Republic* that bothered Neurath and Lauwerys – because of their possibly totalitarian renderings – are due to the “common method of exaggerating, or deliberately overdrawing, a particular point in order to accentuate a general principle,” so there is nothing to see there.

But when we read Prentice's elated statements and uncritical admiration for the wisdom that has been dripped through the centuries by Plato's philosophy, it might be easy to see what Neurath and Lauwerys had in mind when they talked about people who treated Plato “authoritatively”. Even while no participants of the debate ever defined this term, Prentice should definitely be a match. And though one swallow does not make a summer, perhaps all the participants against Neurath and Lauwerys came close to what they had in mind, simply by aggressively attacking a point in their original paper that had only a secondary importance, namely that Plato was just an instance of re-education, and the issue was about re-education of Germany in general. They picked up on Plato, instead of defending Frederick II or anyone else from the original papers. Neurath might have been justified in thinking that he had his fingers on the pulse of British intelligentsia and there *was indeed* a point to discuss.

3.2 *Methods of Defending Plato*

Whether Neurath was right about Plato regarding the particular interpretational questions of philology and systematic philosophy in general is one question. Whether he found similar patterns of effect in the contemporary scene is another. And in fact, even though Neurath did not say so, he would have been in a good position to claim that the way people started to defend Plato is exactly that type of dangerous reasoning he raised against his voice.

As I noted above, Garforth defended Plato as follows:

The goal of [the philosopher kings'] education is the vision of the Good (which is the source of all truth and understanding), of the Beautiful and the other Forms. In the light of this vision they are to govern. Hence it may truthfully be said that Plato's goal for the State was the organization of government and of society according to the ultimate laws of reality.

GARFORTH 1945, p. 16

What do we see here? Garforth tries to highlight the differences between Plato and the Nazis: while Nazis go directly for war (mainly for itself and for power),

Plato's aims were knowledge and education for the greater good. "Good", "truth" and "reality" are all terms from Neurath's *index verborum prohibitorum*, that is, Neurath excluded them from any rational discussion that aimed to remain metaphysics-free. That might be Neurath's own problem – not many people shared Neurath's obsession in collecting and cataloging terms to exclude them. (In fact, Friedrich Waismann, an old colleague of Neurath from the Vienna Circle who was also a refugee in Oxford in the 1940s, did collect and catalogue terms but he did that to utilize them in his philosophical points that were based on the ordinary usage of natural languages.) On the other hand, these words were rejected by Neurath exactly because they do not help much in the rational and intersubjective discussion of critical points. As "good", "the truth" and fundamental "reality" are empirically and sensually unavailable, they cannot be substantiated by any evidence that is equally shareable by everybody. They are rather words of some general character (motivated by socio-political goals) without any direct and substantial anchoring in the world. As Neurath and Lauwerys noted, the Nazis also claimed that they are committed to the welfare of the world and aimed to improve the life conditions of Germany, also by revealing the true structure of reality.

In many letters to Carnap in the '40s, Neurath often draws the parallel between Nazism (or more generally: between a dogmatic and oppressive attitude) and Platonic thinking. Here is one characteristic passage:

I see, how well known philosophers in Germany always quote Plato, when speaking of the ideal state, and I imagine how many young people accepting that, became weakened against Fascism. Plato is the only author in antiquity and in history, with some fame as moralist, who thought pure and simple cruelties pure and simple oppression as ideal. Children should look from horseback, when the parents disembowel enemies in battles, that they get, as he says the proper "taste of blood like young hounds". He supports censorship, allows only military [sic] music, doctors have not [do not have] to help ill people who are responsible for their illness, better for them and for the community when they die, all people of Hellenic blood should be united and then start the conquest of the barbarians, the "enemies by nature" that is the way to be irresistible and to do, what is the highest ideal of the leading groups: the purity of blood.

How can we expect that an enthusiastic youth full of preparedness to self-sacrifice, prepared to think of happiness as something dirty, English utilitarian, can reject the voice of the highly admired Plato-Hitler?

So, all in all, what Neurath might have had in mind is that defenders of Plato are using such strategies, words and views that do not help any critical discussion of democratic issues at all, but rather hinder real solutions behind the veil of "reality", "truth" and "the good".

Actually, similar ideas (i.e. instantiating a Platonic defense of Plato) could be observed in Joad's reply, though he is a hard nut to crack. He obviously defends Plato against Neurath and Lauwerys, usually by pointing out that there might be striking similarities between Plato and the Fascists, e.g. that both

envisage an authoritarian State in which the best make the laws and the many achieve happiness and virtue as lie within their compass, by cheerfully obeying the laws and giving their services, thus enabling the State to function and the best to realize the purposes which are appropriate to the best.

JOAD 1945, p. 164

For Neurath and Lauwerys, this should suffice, actually, as they obviously think that the major aim of the people could not be "cheerfully obeying the laws and giving their services" and reaching happiness simply by serving. Independently of the fact that this seems to be one of the characteristics of Fascist states, if Plato's state is built on these ideas, it goes against the enlightenment vision of Neurath.

But Joad tried to highlight the *differences* of Plato and the Nazis. First, he mentions that Nazi leaders select themselves and points towards ambiguous ideas as no clear standard is provided about why they should lead. On the other hand, "in Plato's State the criterion by reference to which the best are selected is that of knowledge or wisdom".³⁹ Joad continues by stating that in Plato's view, "there is an absolute good and an absolute justice". As I noted above, this is exactly the conception that Neurath repudiates as metaphysical, nonsensical, and even dangerous! What is the absolute good, and how to grasp it? How to know who has the right intellectual means to grasp the third realm of Good and Justice? Plato would of course say that citizens are ordered in accordance with their nature and capabilities. Joad presents Plato's views as follows: "Justice, the contended doing of the job for which he is fitted, is the highest morality of which the ordinary man is capable".

The main difference here between Plato and the Nazis seems to be that in a Nazi state people are ordered into hierarchies in accordance to their blood

39 Joad 1945, p. 164.

and origins, while for Plato what mattered are their intellectual capacities. But as origins cannot be changed, intellectual capacities and “nature” are rigid as well. But Joad does not see any problem here. Neurath and Lauwerys claimed before that Plato treats ordinary people only as means (for war or for the State) and does not respect their values, freedom, right, and happiness. In Plato’s view, however, emphasizes Joad, fulfilling one’s purpose, or living “according to the laws and ordinances which the Guardians have framed for him”, ordinary man “achieves such happiness as appertains to his nature”. Joad thus concludes with saying, “[i]t is not true, then, to say that Plato treats the ordinary man only as a means; he is prepared to regard his welfare as an end, though as an end of inferior value”.⁴⁰

Whatever one thinks about achieving happiness by fulfilling externally enforced conditions and laws, this is exactly the view that Neurath and Lauwerys stressed as dogmatic, suppressive, and dangerously resembling the Nazi ideas. Even if Plato’s and the Nazi’s views are somewhat different regarding how to arrive at their hierarchical and oppressive social arrangements, there are very similar and substantial structures that make Plato’s educational and political ideas dangerous for broad dissemination without further ado.⁴¹

There was one more very interesting argument in Joad’s article. He raised the rhetorical question at one point that “is it not [...] a little disingenuous to find fault with Plato for recommending the killing off of mental and physical defectives at the very moment when we are engaged in the slaughter of sane and healthy human beings by the million?” There are many things to discuss about this question and its implications. But it might be the best to cite here Neurath and Lauwerys:

40 Joad 1945, p. 165.

41 The relation of Joad to (Platonic) idealism is a strange one. He did not accept (actually wrote an entire book against) Sir James Jeans’ and Sir Arthur Eddington’s idealism in physics – he thought that they have thrown the baby of scientific realism with the bathwater of mechanism. That is, in order to provide space for values in science (that was abolished in the mechanistic worldview of the 19th century), “[t]he wheel [of Jeans and Eddington’s views] has turned full circle, and in their enthusiasm for idealistic interpretations of phenomena many scientists seem anxious to deny the revelatory character of science altogether” (Joad 1932/1963, p. 12). His solution was that as science can reveal the *real* world, human intellect could grasp the third realm of objective, unchanging and absolute values. This is very similar to Plato’s views about forms and knowledge, thus it is not that surprising that Joad stood up to defend Plato against Neurath and Lauwerys. On Joad, the physicists, and for further context, see Tuboly 2020a.

We regret that so well-known a lover of freedom as Dr. Joad should use such an argument. It is a dangerous one, for some Nazis (for instance the brutes in charge of concentration camps) argue as follows: in periods when healthy people have to be sacrificed, one cannot pay attention to sentimental pleas in favour of the weak, the sick, or those of lower race. What Dr. Joad says is, we believe, *in harmony with a Platonic attitude*, and it is precisely this which many people, and only in western societies dislike lock, stock, and barrel. For that attitude pays no attention to the sanctity of each single individual within a society.

NEURATH/LAUWERYS 1945b, p. 224, my emphasis

Instead of stating a negative dependence-relation between healthy and non-healthy people about when shall we consider any of them against the other, Neurath and Lauwerys suggested in an unpublished correspondence article (in connection to their note above that "each single individual within a society" matters) the following: "We should perhaps open a debate about the question, whether it would be advisable, to have a kind of 'handicappers' who take care of these people by giving them additional support. A deaf-mute person could perhaps get more books than other people, more opportunities to visit galleries and big shows, to have particular clubs with peculiar facilities."⁴² That is, instead of deepening and widening the gap between healthy and non-healthy citizens, Neurath's aim was to empower the masses by localized and special educational means.

Turning the wheel against Joad, Neurath and Lauwerys claimed that what Joad seems to ask rhetorically could be asked equally (and indeed was) by Nazis. They do not say, of course, that Joad is a Nazi or that he is like them; they only say that the attitude, or the type of question that is exemplified by Joad's utterance plays into the hand of radicals, dogmatists and extremists. That is indeed what Neurath and Lauwerys feared, namely that using Plato's ideas and conceptions (either for defending him in the debate or externally) amounts exactly to creating such a general climate of opinions that not only weakens Nazism but also even contributes to it.

There are two interesting things to note in Field's first reply as well. Recall that I mentioned above his ideas about "the purity of race". Field claimed that though there are certain ideas about eugenics in Plato as well, they are not based on ancestry, but on qualities in general.⁴³ That is, while the only thing

42 Neurath/Lauwerys, "Plato's Republic, German Education and Human Brotherhood." ONN, K.73.

43 See Field 1945a.

that matters for the Nazis are your ancestors (regardless of your personal characteristics), what mattered for Plato (and what he tried to improve by introducing strange and to the modern mind “shocking” techniques) are your personal qualities. So “[i]f the qualities appear, on occasion, from an unexpected ancestry or fail to appear from an ancestry from which we might expect them, it is they, not the ancestry, which determine the individual’s position in society”.⁴⁴ Of course, the two things seem to be different and Plato’s views might allow the possibility for people to obtain different places and roles in society; nonetheless, as also noted above, some interpreters in the debate went for the claim that people have their own nature that determines their role somehow, so social mobility might be a tricky issue after all. One could also raise the question (as did Neurath and Lauwerys about similar passages) who shall decide about the qualities that matter and the degree of improvement that counts?

Finally, Neurath and Lauwerys’s have brought here into discussion the Barbarians. “The Nazis, too, say they are not interested in ancestry as such, but only in the qualities likely to be associated with the right kind of ancestry”.⁴⁵ It is certainly possible that “Barbarians” had the right or required cognitive *qualities* in Ancient Greece, nonetheless, it is known that their treatment (even by Plato) were anything but democratic and just. After all, they did not have the right qualities, simply because they were Barbarians – that is, had the wrong ancestry.

4 Conclusion

The list could be continued, of course, by analyzing one by one all the replies and how they missed Neurath’s original point. But the general atmosphere of the whole debate could be seen perhaps already.

For a few more months in 1945, Neurath was concerned with his original ideas of re-education of Germany. He wrote up unpublished materials about these questions, published some papers under pseudonyms. He was even invited in June 1945 to a committee (chaired by Lauwerys) to talk about the re-education and de-Nazification of Eupen-Malmedy, a small region in eastern Belgium that was part of Prussia in the 18th century, went into hands of Belgians after World War One, and was annexed by the Germans during World War Two. That talk has something for us in this context. Neurath noted that the

44 Field 1945a, p. 161.

45 Neurath/Lauwerys 1945b, p. 222.

situation is, obviously, very complex. Although everyone knows how effective German propaganda was, the grand propaganda of the Nazis was just the tip of the iceberg. Actually, there are "main traits of Nazism, which seems to be of such a kind that they impress even gifted children and adults. Just these traits in the Nazi propaganda are particularly dangerous [and] not the primitive expressions of cruelty and brutality."⁴⁶ Therefore, one shall be cautious how to handle the issue of Nazism after the war.

That is where education comes into the picture. Instead of doing grand propaganda against Nazism and for the democratic way of life, Neurath suggested more local ways of dissemination. Instead of general films, and individual posters that resemble propaganda, exhibitions shall be organized in museums and even in schools. Biographies of democratic people shall be written, and "[o]ne could think of picture sheets distributed to children, dealing with various subject matters; these sheets have the great advantage that they remain in the hands of the children, if they are not directly biased." The same goes for education:

It is not too difficult for sincere anti-Nazi teachers to fight Nazi cruelty and unquestioning obedience to their ideology, but they are very often helpless where they have to cope with the such less obviously dangerous elements, used by the Nazis, in the "best" pre-Hitler literature, which attracts the sensitive and decent children.

NEURATH, "Education in Occupied Germany: Intricate Problems," ONN/K.74

Neurath claimed repeatedly that it is easy to spot the explicit and declaredly Nazi materials. Those items that are not Nazi *per se* but were used by Nazis for their causes, or produced such a general atmosphere in which Nazi thought could emerge more easily cause the problem.⁴⁷ In Neurath and Lauwerys's view, Plato was an important figure in this process by being "*influenced* by the overemphasis on certain things in the tradition of the 'best' German literature", emphasizing regularly "the state", "the rules", "the morals", "the good", "the

46 Neurath, "Meeting, Belgium committee ... chairman Lauwerys (1945)." ONN/K.79.

47 I cannot analyze and discuss the so-called "German climate"; in the 1940s, Neurath was virtually obsessed with the idea of a special German climate, a certain cultural and intellectual environment, which increased the willingness and inclination to accept National Socialist ideas and created a certain bias towards any cultural goods and products that could have been rendered under the National Socialist agenda, e.g. the philosophies of Plato and Kant. For further information, see Sandner (2011) and Burke and Tuboly (2025).

virtues". "It is more difficult to create a new German human environment than to uproot Nazidom proper", concluded Neurath.

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Rudolf Carnap (1891-1970) and Otto Neurath (1882-1945) had a decisive influence on the development of the scientific world view of logical empiricism. Their relationship was marked by mutual intellectual stimulation, close collaboration, and personal friendship, but also by controversies that were as heated as they were rarely fought out in public. Carnap and Neurath were, in the words of Olga Hahn-Neurath, “like-minded opponents”. The essays in this volume deal with these key thinkers of logical empiricism from different perspectives, shedding light on the complex development of one of the most influential philosophical currents of the twentieth century in the midst of dark times.

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